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Journal of the Royal Statistical Society

SERIES A (GENERAL)

PART I, 1957.

THE REPLACEMENT COST OF FIXED ASSETS IN BRITISH MANUFACTURING INDUSTRY IN 1955

By T. BARNA

[Read to the Royal Statistical Society 21st November 1956, the PRESIDENT, PROFESSOR E. S. PEARSON, C.B.E., in the Chair]

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1. INTRODUCTION

1.1. In this paper an attempt is made to define and measure an important part of the national capital, and thereby to contribute to the discussion of public policies on investment. The method employed is new for this field in so far as the primary data were obtained by direct inquiry addressed to industry. The paper describes in the first place an experiment in method which, in the event, was successful. As in all experiments, new ideas and new suggestions emerged in the course of the work which may usefully be incorporated in further research. Again, as in other experiments, the time and resources available were strictly limited and no doubt in further research more adequate results could be obtained by the employment of larger resources. Nevertheless sufficient information was obtained to establish economically significant estimates and conclusions, and the results are presented here in their final form.

1.2. During the long history of this Society the measurement of national capital has often been the topic of contributors.* The subject has been given perhaps as much attention here as anywhere, and in due course measurements of modern standards of accuracy have been achieved. Yet at the same time, although the importance of capital in relation to current economic problems and policies is recognized, a sufficient quantitative knowledge of capital, particularly of industrial capital, is still lacking.

1.3. The reasons for the successful early development of measurements of national capital, as well as for the subsequent limits of growth of this development, are not far to seek. The administration of income tax and death duties gave rise to statistics of income (and more particularly, in the present context, of income from property) and of capital passing at death. These statistics have been consolidated since the re-introduction of income tax by Peel in 1842 and the introduction of estate duty by Gladstone in 1894, and have provided substantial bases for estimating the national capital by two alternative methods. Under the first method income from property is capitalized with reference to the ruling yield of different types of assets. Under the second method capital passing at death is regarded as a sample of total capital in private ownership and an estimate of the total is arrived at by the use of mortality rates. Under either method supplementary estimates are required to arrive at total national capital or to make the results of one method comparable with that of the other.

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* For a list of works before the First World War, *cf.* Stamp (1916).

1.4. The first method of estimating national capital is associated with the name of Giffen (although his was not the earliest contribution) and the second with that of Mallet. The best known modern estimates, using the first and second methods respectively, are those of Stamp (1937) and Campion (1939). These latter contributors had at their disposal more developed primary statistics and their estimates are more refined than those of their predecessors.

1.5. The importance of the Mallet (or estate) method is that the distribution of capital between persons is obtained as its by-product, and therefore it is relevant to a study of social questions and policy. No such importance is attached to the Giffen (or income) method which, apart from making possible an estimate of total national capital, adds little to the knowledge directly obtained from statistics of income. It is true that by the latter method certain broad components of national capital have been estimated, such as capital in agriculture or in the railways, but the theoretical basis of these figures makes them useful only for certain specialized purposes. Moreover, only a very broad classification of industries is possible by the Giffen method. Income is classified by business unit and not by activity, and in so far as enterprises operate in more than one industry classification by industry is not feasible. Nor is it possible to separate fixed assets from other assets, or even domestic from oversea assets, as there are no grounds for saying that the income of an enterprise is derived from one asset rather than another. Lastly, with the present system of statistics the Giffen method is believed to be less accurate than the Mallet method.

1.6. In fact the two alternative methods described in the preceding paragraphs are alternatives only as regards statistical technique: they are based on the same concept of capital. According to this concept the value of capital is determined by the income which it is expected to yield. As the ownership of most of the national capital is represented by stocks and shares, the discussion can conveniently be focused on the valuation of these assets. Both the Giffen and the Mallet methods accept stock-exchange values for this purpose. The first method takes income from stocks and shares, and uses the relevant stock-exchange yields for capitalizing income; in other words, it is a sample method for estimating the total market value of stocks and shares, yields being determined by a sample of stock-exchange quotations. The second method by definition adopts the market value of stocks and shares, and takes the estates of those passing at death as a sample of the total.

1.7. Both these methods are derived from a common approach to capital, that is an approach through ownership. Titles to ownership are identified and a value placed on them. Indeed the statistical limitations of the results obtained are inherent in the approach. There is, however, an alternative approach through which physical assets are identified and a value placed on such assets. This is more suitably associated with an alternative concept of capital, namely value based on replacement cost. The two approaches are theoretically conflicting and would yield the same numerical results only in a world of perfect foresight and perfect competition.

1.8. The dominant concept of theoretical economics is capital defined as the discounted value of future income streams. No doubt certain valuations, such as those by the stock exchange, are implicitly based on this concept. But the concept is so general that only in rare instances can it be used in practical problems. One important objection to its use is that it is dependent on short-term waves of optimism and pessimism as witnessed by stock-exchange fluctuations. For a wide range of problems in economic theory and policy, more objectively measurable and less volatile concepts are required. But the most serious objection encountered is that in practical affairs a value has to be placed on physical assets or groups of such assets in circumstances in which the discounted income stream concept is not relevant. In fixing prices, for instance, either by government or business, the future income stream is to be determined by the value of capital employed rather than the other way round. In such cases recourse may be had to the replacement cost concept.

1.9. One important aim in measuring industrial capital is to evaluate the contribution it makes to production. For this purpose a "replacement cost" concept of capital appears suitable but a "discounted future income stream" concept clearly is not. This is not the place to enter into a detailed discussion of the merits of different principles of valuation of capital assets. It is enough to say that the most relevant objection to the "discounted future income stream" concept is that the income of an enterprise reflects not only the amount of capital employed by it but also managerial efficiency and the influence of outside factors (particularly the state of demand) on the industry in which the enterprise happens to operate. We need a concept of capital which is objective in the sense that the value of an asset does not depend on the persons who own and control it, or on the industry in which it is employed. Replacement cost is not, of course, independent of income to be derived from assets, in much the same way as costs in general are not independent of prices. But the influence of income in determining replacement cost is much less than in determining the value of assets under the other concept. It should also be noted that replacement cost is not an unambiguous concept but rather a term which, as will be shown in section 2 below, denotes a family of concepts.

1.10. The unsatisfactory nature of the Giffen valuation was explicitly recognized by Harris (1894) when he told this Society that "this is not what we want when we are comparing inventory wealth" (p. 542). He was particularly concerned that "the limited liability system has brought an immense amount which consists of 'prospective profits capitalised' rather than of 'concrete wealth' into the meshes of the revenue," (p. 544) and since then the importance of limited liability companies has increased enormously. Yet the only attempt in this country so far to measure a substantial proportion of the national capital at replacement cost, that is as "concrete wealth", is that of Redfern (1955). Redfern follows the "perpetual inventory" method which has been previously applied to United States data by Goldsmith (1951). By this method the stock of assets is estimated as the cumulative total of investments (corrected for price changes) over several past years and thus the method calls for estimates of the average life of different types of assets and of price changes over time. This is an indirect method of estimation and Redfern has given a warning that the results are liable to error on account of certain deficiencies in the available empirical information. The estimates for British manufacturing as a whole which are the result of this paper are, as will be shown in section 4, very substantially higher than the comparable figures which would be arrived at following Redfern's procedures; the possible explanations of this difference will be explored at the end of section 4. Let it be said, however, that Redfern's ultimate aim, as the title of his paper indicates, has been to divide annual gross capital formation into capital consumption, on the one hand, and net capital formation on the other. The estimate of the stock of assets is an intermediate step and an error in this estimate does not necessarily imply an error in the final figures. Indeed the possible explanation of Redfern's relatively low estimate for the stock of assets, which is given below, suggests that compensatory factors make the final estimates of capital consumption, which have now been adopted by the Central Statistical Office (1956), fairly safe.

1.11. Redfern has given estimates of the stock of fixed assets for a number of sectors of the economy, including one comprising manufacturing taken together with some other industries. The currently published series of official statistics on capital formation was begun only in 1948, and for earlier years detail by industry is not available. In consequence the perpetual inventory method could not be applied to individual manufacturing industries (with one or two possible exceptions) even if one were prepared to apply Redfern's assumptions and procedures to individual industries.

1.12. The inquiry described in this paper is the first attempt in this country to estimate by direct method the replacement cost of fixed assets in different sectors of manufacturing industry. The inquiry is confined to manufacturing for the sake of limiting the task, and also because some of the problems of valuation are more difficult in other sectors of the economy than in manufacturing. In agriculture there is the problem of land values, in transport that of large indivisible units, and in the government sector that of assets not yielding a money return. Attention is here confined to fixed assets, and stocks are only brought into the picture in section 5 when total physical assets are discussed. The valuation of stocks, although much debated, gives rise to problems which are quantitatively less important than those arising in connection with the valuation of fixed assets; the reason for this is the much longer average life of fixed assets. Another reason for neglecting stocks is that the government already regularly collects information on them.

1.13. Among fixed assets estimates are made here only for buildings and constructions, and plant and machinery. Land and vehicles are excluded from physical assets, and no place can be found for "intangible" assets in an inquiry of this nature. The value of land employed by manufacturing is comparatively small,* even though it may be important in factories in densely populated districts, and special methods would have to be employed for its measurement. Vehicles are excluded because they are not assets pertaining to manufacturing in the same sense as other fixed assets, and their inclusion might have disturbed the comparison between industries.† It was also thought that alternative empirical methods are available for the study of economic problems connected with vehicles. Intangible assets cannot be defined on a replacement cost basis; in fact the only meaningful definition which can be given to the intangible assets of a business, such as "goodwill", is the difference between the market value and the replacement cost of assets. Replacement cost valuation of tangible assets is thus a prerequisite for valuing goodwill but replacement cost including goodwill is a meaningless concept.

1.14. The major basis adopted for the present study is valuation for fire insurance. To my knowledge there is only one important example of a somewhat similar use of such statistics: Helfferich (1914) estimated the national wealth of Germany for the years 1888–1913 and used fire insurance values to confirm his estimate of the totals. At present information on fire insurance values is collected in Sweden (Kommerskollegium, 1954). Since 1937 the Swedish Government has initiated an annual inquiry into industrial investment and investment plans. For 1946 and from 1949 onwards this inquiry has also sought to obtain information on the insurance value of fixed assets and stocks, supplemented by information on the value of uninsured assets—relatively small in manufacturing industries. The exact definition of these figures, however, is not ascertained nor is their meaning interpreted.

1.15. In this country the Census of Production Committee considered the question of obtaining information on the fire insurance value of establishments, but rejected the idea in its report (Board of Trade, 1945). The main reason at the time was that fire insurance values did not then fully reflect post-war prices. It is believed that insurance values became generally adjusted to the price level only gradually over several years after the war, and by now this objection must have largely fallen away. On the other hand the government obtained in the 1946 Pilot Census of Production data on the written-down value of assets as determined for income-tax purposes. The very limited use of these data was soon discovered and the results were never published. Lastly, in the full census of 1951 data were collected on the horse-power of machinery, a set of statistics which is regularly collected in other countries but had not been included in our censuses since 1930. These data can, of course, indicate capital employed only very crudely.

1.16. The next section of this paper discusses more fully the bases of valuation possible for measuring assets and the basis adopted here. The organization of the sample inquiry into manufacturing assets is described in section 3, while its results are evaluated in section 4. The new estimates for fixed assets are related to certain industrial statistics already available and in particular the relationship of value added to assets is examined in section 5, and of new capital formation to the stock of assets in section 6. Some conclusions are drawn and the findings summarized in section 7.

1.17. The inquiry described here is part of a larger undertaking into the measurement of the growth of industrial capital which I am conducting at the National Institute of Economic and Social Research. The project is financed by the Board of Trade under the Conditional Aid Economic Research Programme from counterpart funds derived from United States economic aid, and this help is gratefully acknowleged. My thanks are also due to numerous officials of industrial firms and insurance companies who gave me information and advice. Some of this information is confidential and, to safeguard against divulging it, it was decided not to give a list of companies or their officials who have co-operated in this inquiry. Lastly, I wish to thank Mr. W. A. B. Hopkin, the Director of the National Institute, and my colleagues there, particularly Mr. A. Luboff, for discussing various aspects of the inquiry, and Mrs. Patricia Kaye for the efficient administrative and secretarial help which she gave.

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* This is strictly true only if gross replacement cost is taken. As land does not depreciate with wear and tear and age, as other fixed assets do, the value of land can be relatively high if written-down replacement cost (that is, after depreciation) is taken.

† The exclusion of vehicles does not raise particular statistical problems as the corresponding employment is also excluded. Similarly, the post-war censuses of production exclude, at least in principle, the contribution of transport fleets to the value of output.

2. The Basis of Valuation

2.1. The success of estimating the value of assets in different industries depends not only on the adoption of an economically significant basis of valuation but also on the possibility of obtaining data from industrial firms on that definition. The danger in an experimental study of this kind is that an ideal operational definition of the value of assets may be developed but very few industrial firms would be in the position to supply data in the form required. Hence the normal procedure adopted in statistical investigations was put into reverse: industrial firms were asked to give data on the value of fixed assets on a suitable basis and, further, to explain the basis of valuation. A higher rate of response can evidently be expected by this technique but estimates for different industries can only be made if respondents happen to use sufficiently uniform definitions. The most important conclusion of this inquiry is that in fact it is feasible to obtain information from business firms on the replacement cost of fixed assets, and almost all the information obtainable is based on one or other of two alternative concepts of replacement cost. This section is devoted to the discussion of these concepts and to a description of the nature of fire insurance, which is the most frequent reason for determining replacement costs. But before coming to this one should explain why book values of assets are not a satisfactory basis for the purpose in hand.

Book Values

2.2. All business firms are, either for tax purposes or by company legislation, required to place a certain value on their assets at the close of each business year. For public companies and also for some categories of private companies the book value of assets is easily available from published balance sheets. There is thus extant a mass of data which appears deceptively homogeneous and which on occasion is used for economic measurement of a type for which it is not suitable. This, however, is not to deny the validity of balance sheet values for the purposes for which they are meant, and the argument against the use of balance-sheet values for other purposes is by no means a plea for a reform of accounts.

2.3. The main reason for not using book values for the measurement of assets in any "real" sense is that they are in terms of original cost. To be more precise, book values are in terms of cost of acquisition and when second-hand assets are acquired the truly original cost is ignored. Moreover, for balance-sheet but not for taxation purposes, companies may re-value assets and subsequently express them in terms of prices ruling at the date of revaluation. In consequence the figures published for different firms are not comparable and, as all figures are aggregates of data relating to more than one price level, it is not possible to introduce any precise correction for comparability.

2.4. Further, balance sheets always show the net (that is, written down) value of fixed assets, while gross values (that is, at cost or as re-valued) when shown are seldom truly gross. The Companies Act of 1948 made it obligatory for companies to which it refers to show the value of assets at cost as well as written down. But this provision only applies to assets acquired since the Act, and previously acquired assets may be entered into the gross figure at an arbitrary value or not at all. With net values there is the added difficulty that assets are believed to be written down, on the whole, prematurely. To write off an asset completely while it is still in use is regarded as a virtue but to have it on the books after it has been discarded would be dishonest. On balance the bias is bound to be in one direction. In recent years some companies, but only a small number in the manufacturing field, have re-valued their assets and, on occasion, have shown a striking divergence from previously published figures. While revaluation must be welcomed by economists on general grounds, it is likely to make the balance-sheet values of companies which have re-valued and those which have not even less comparable.

2.5. The occasions on which companies themselves indicate the unrealistic nature of book values are rare. The chairmen of some of the largest public companies have in recent years disclosed the extent to which book values are below some realistic value but in no instance have they given the exact scope of the alternative figure. There is, perhaps, greater inducement to disclose "real" values when a private company is made public and shares are introduced on the stock exchange. On such an occasion Barr and Stroud Ltd., for instance, stated in 1954 that the book value of fixed assets was £120,000 but that these assets, without land, were insured for over £1 million.

Replacement cost

2.6. In general, replacement cost gives a more objective basis to the valuation of assets than any alternative method since almost all assets used in industry are replaceable; it remains to be discussed in the following paragraphs what exactly is meant by "replacement cost".

2.7. Any industrial firm, taken as a whole, is a highly specific unit and the market in such units is bound to be very imperfect. On occasion the value of such a unit to a particular buyer may be exceptionally high and, conversely, the unit may fetch little in the event of forced sale. These extreme cases, however interesting from a theoretical point of view, are ignored and normally valuation is made on the assumption of "willing seller and willing buyer". Further, there is an important difference between the value of assets as installed, in certain relation to each other, and the so-called break-up value when each asset might be sold separately. For the purpose in hand it is customary to value business as a "going concern", that is, to value physical assets at their worth to the existing management on the assumption that they want to continue in business. Replacement cost prevents an exaggerated value being placed on assets, say for sentimental reasons, as it sets an upper limit at which assets for replacement can be obtained. It can, of course, be higher than market value. Replacement cost must include, in addition to the cost of acquisition of assets for replacement, also the cost of installation and other incidental costs.

2.8. The value of a factory also depends on the units into which assets are divided for valuation as it is a recognized principle of property valuation that the value of the sum total of assets may differ from the sum total of the value of each asset. A machine consists of different parts which wear out at different times; the life of the machine may well be determined by a relatively short-lived component. If the lay-out of a factory is obsolete, its value may even be less than the break-up value. On the whole, however, the value of a complex unit is higher than that of its component parts because of the labour necessary to create the complex. The choice of a suitable unit for valuation is important in, for instance, valuation for fire insurance. This is especially so in modern factories where the insured is more concerned with the possibility of partial loss than with total destruction. The usual practice is to value neither a whole factory or workshop as a unit, nor the smallest possible items, but something intermediate such as a machine with all its attachments or a complex plant which operates as a single entity.

2.9. Two entirely different concepts of replacement cost are distinguishable: the cost of replacement with new and the cost of replacement with second-hand assets. The first sets an upper limit to replacement cost as it is normally possible to replace by new assets, though, exceptionally, an allowance must be made for scarcities (when the asset is not readily obtainable at the ruling price). The manufacturer would clearly be better off, apart from the disturbing effects of the change, if he were to replace old assets by new. The second concept makes an allowance for the actual state of the assets, which is the result of age, wear and tear, and obsolescence. But, as it is not easy to find assets in exactly the same state in the second-hand market, the manufacturer may be forced to spend more than the written-down replacement cost if he wishes to replace assets.

2.10. Replacement cost new is a clear-cut concept only if it is intended to replace assets with identical units, but this is seldom the case in the strict sense. It is perhaps a valid generalisation to suggest that as a result of inventions capital assets undergo an almost continuous and steady improvement and that hardly any two assets produced at different times are identical. This generalization is applicable to buildings as well as plant. In these circumstances the concept is never understood in the literary sense but as replacement by a substitute. If the substitute has the same output and inputs but costs less, the lower price is taken. A further allowance may be necessary to take account of the lower running costs of the substitute. The art of evaluating replacement cost in fact consists of finding that amount of modern assets which can be regarded as the equivalent of the old, in the economic rather than in the engineering sense.

2.11. The calculation of the cost of replacement with second-hand assets is in most instances not made with reference to recognized second-hand prices. For almost all assets the second-hand market is narrow and volatile. The ratio of second-hand to new prices fluctuates, apart from the actual state of the equipment sold, with the general state of the market; second-hand prices are particularly low in a depression. Instead, the calculation is usually made by taking replacement cost new and allowing for the shortening of the life of the asset with age. If the rate of depreciation used in this calculation is realistic, the results should of course be consistent with normal second-hand prices making due allowance for costs of installation and other incidental costs. From a statistical point of view replacement cost new is therefore a primary concept. Additional information on the life of the asset and other factors is required for the calculation of written-down replacement costs.*

2.12. An interesting finding of this inquiry is that for a firm as a whole written-down replacement cost does not differ from replacement cost new as much as might have been expected in the abstract. In balance sheets assets are gradually written down to nil, but are often kept in further use. Hence the value of an asset half-way through its effective life is about one-half of its original cost or less. But for other purposes assets are normally written down less quickly and writing down stops at a certain minimum. The rate at which assets are written down in the balance sheet can be regarded as the highest rate at which they would be written down for the calculation of replacement cost. Firms which calculate written-down replacement cost appear not to diminish the figure below 40 to 75 per cent. of replacement cost new, varying from firm to firm.† The tendency to adopt the lower of these limits is perhaps more prevalent in the light industries, where marketing conditions depend on less predictable factors and thereby the expected effective life of equipment is uncertain.

2.13. It is also my impression that the amount of seriously obsolete assets in British manufacturing industry is less than popularly believed. Almost all firms can give an example of an archaic piece of equipment still in use. But there may be very good economic reasons for retaining such pieces and, in any event, they are quantitatively swamped by modern equipment. By and large, old buildings and plant are modernized in the course of time and consequently their economic value remains a significant proportion of replacement cost new. It is for this reason that written down replacement cost never falls below the levels mentioned in the previous paragraph. Save in periods of exceptional depression, the normal maintenance of assets ceases only after a decision has been taken to scrap equipment or put it into reserve; this decision is reached any time up to 3 years (or exceptionally even more) before the proposed date of scrapping. The valuation of such obsolete equipment is admittedly the most difficult. Equipment condemned to death would be valued at a nominal figure or written down replacement cost might be estimated with reference to the remaining life of the asset. The replacement cost new of the firm would also include this low figure rather than be inflated by an unrealistic estimate. Thus in practice the concept of replacement cost new must be interpreted loosely.‡

2.14. Whether replacement cost new or written down is a more useful concept is a controversial subject. Certainly more needs to be known about the life history of assets before this question can be answered. Written-down replacement cost is a more comprehensive concept—it possesses what one might call an additional dimension—and it is closer to the basic concept of capital in economic theory. In some instances it may be possible to estimate it directly, as mentioned in 2.13, when replacement cost new could not be estimated. On the other hand powerful arguments can be brought up in favour of using replacement cost new for certain problems. The value of assets declines with age partly because the expectation of further life declines and partly because of increasing costs of maintenance. It logically follows that value declines faster than productive efficiency, and it is even believed that for important classes of assets the decline in productive efficiency in the course of effective life is negligible. No doubt replacement cost new is less an economic and more an engineering concept than written-down replacement cost but it may be held to be a more relevant concept when examining the relationship of assets to current output (Edwards, 1955), and certainly more relevant in forecasting requirements of incremental capital. This paper presents results in terms of replacement cost new; this is done, however, not so much out of choice but, as will be indicated in the next section, as an empirical necessity.

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* I prefer this term to the more ambiguous "net" replacement cost.

† E.g., a firm may write off 5 per cent. for 5 years and then stop; no asset will have a value of less than 75 per cent. of replacement cost new put upon it. At the other extreme, a firm may write off 10 per cent. for 6 years and then stop at 40 per cent. of replacement cost new.

‡ For the same reasons, given in this and the preceding paragraph, great caution must be exercised in using even this type of valuation of capital in connection with measurements of productivity.

The nature of fire insurance

2.15. There is another valuation which has to be made about as frequently as that for balance sheets, and this is valuation for fire insurance. The term "valuation" may, of course, denote either an elaborate procedure by experts or simply a guess by the business man. This valuation ought to be realistic, and in the great majority of cases it is, as on its appropriateness the survival of the firm may well depend. The question is, what exactly do industrial firms insure and how can insured values be related to the concepts of replacement cost discussed above?

2.16. Today insurance against fire is general. The business of insurance is broadly competitive, rates are more or less scientifically differentiated according to risk, and the bulk of industry regards insurance premiums as a necessary and relatively minor business cost. Only in rare instances do industrial firms go uninsured. Although some large concerns may operate an internal insurance scheme, and may or may not re-insure in the market, they would have to follow the same principles as those taking a policy with an outside insurer.

2.17. A fire policy is a contract whereby the insurer undertakes to compensate the insured for direct losses incurred in the event of fire. There are a number of different types of policy but in this country they commonly embody the following principles: (i) The policy gives cover for the destruction or physical deterioration of assets and not for consequential losses due to fire damage; production may be interrupted as a result of fire and profits may consequently suffer but this loss is not covered by a fire policy. Another policy may be taken out to cover for loss of profits, including such items as the salaries of a nucleus staff and prestige advertising while production is interrupted, but such policies need not concern us here. (ii) It is universal practice to place a definite value on the assets insured, the determination of this value depending on the type of policy. (iii) In the event of fire the insured cannot receive more than full compensation, in terms of a given policy, even if he has placed an excessive value on the assets; but he can be under-compensated if he has placed an insufficient value on the assets.

2.18. Generally, fixed assets are specified separately from stocks and the technique of valuation is also different for the two types of assets.* Seldom is a detailed list of items attached to the policy but more commonly a factory is insured as a whole, specifying separately buildings and plant. In larger factories there is a tendency to insure by zone. There is also a tendency on the part of the insurer to insert the "average" clause in certain types of policy under which all parts of a whole are deemed to be valued on the same basis. This means that if in the event of a loss the value insured is less than the true value, the insured is required to bear a proportionate part of the loss himself. In other words, if the value insured is £10,000, the true value £20,000, and the loss £4,000, then the insured can only recover £2,000.

2.19. Two main types of policy exist: insurance for reinstatement and insurance for indemnity. The values to be insured under the former type correspond to replacement cost new. Under the latter type current market values are insured; in fact the most frequent procedure is to determine current values as equivalent to written-down replacement cost. The choice between the different types of policy is left to the insured. He may of course always replace items lost with new assets but under an indemnity policy he would not recover the full cost.

2.20. The insured generally excludes assets which he regards as indestructible. Apart from land, foundations to buildings fall into this category; they may account for 7 to 15 per cent. of the value of buildings. Most other uninsured assets are in heavy industry and include roadways, railway sidings, blast furnaces, coke ovens, and similar assets which are not inflammable. The extent to which these assets are in fact indestructible is debatable. Cases are known when foundations have cracked as the result of fire and had to be replaced at great cost. Properly, the values insured ought to allow even for the demolition of damaged buildings and for the clearing of debris. The practice of excluding so-called indestructible assets varies from firm to firm, but the impression was gained that the majority exclude foundations to buildings.

2.21. In industries where a significant proportion of assets, if not the bulk, is uninsured, fire insurance values would give a misleading indication of the value of fixed assets. Fortunately in most of such cases firms were able to supply information on the replacement cost of all fixed assets, whether insured or not. Indeed, it appears that an increasing number of firms is interested in possessing information on the replacement cost of assets quite apart from the need for such information for fire insurance. These figures are used for estimating depreciation in excess of normal balance-sheet depreciation and for assessing the profitability of the business in relation to real capital employed. Only a few of the firms possessing this information did revalue assets in the balance sheet.

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* Under a "block" policy, however, all assets are insured together and the division of the total value between fixed and other assets may not be known to the insurer.

2.22. Whether for fire insurance, or for other purposes, the technique of valuation can be divided into three categories. (i) A detailed valuation may take place based on an expert assessment of physical assets either by a firm of valuers or by the manufacturer's own engineers. This procedure is slow and expensive, and takes place only infrequently. (ii) The replacement cost of assets may be estimated by finding in the records the cost of acquisition and the date of purchase of each item, and by applying price indexes to bring these values up to date. The price indexes may be general or specifically prepared for the firm, and may or may not differentiate between different types of assets. (iii) Short-cut methods may be used for bringing previously established values up to date by applying some index of prices.

2.23. In principle, policies are annual but values insured are altered in the course of the year as additional assets are acquired or sold. It is not customary to make changes during the year to allow for the effect of changing prices but such changes should be made annually. It is known, however, that the latter kind of change is not made as regularly as it should be and the valuation, especially of the smaller enterprises, is often brought up to date only once in several years.

Under-insurance

2.24. The question of under-insurance is the most difficult one to discuss. Over-insurance may perhaps be ignored as the insured is not allowed to benefit by it; over-insurance, if it exists, is due to errors in valuation rather than to choice. But many firms are known to under-insure in order to save on the premium. Although rates are differentiated according to trade, and whether or not the factory has a sprinkler system, many insured feel that in their particular case the risk is less than is implied by the rate. An important element in the manufacturer's own judgment is the consideration of total loss and therefore close attention is paid to factors such as the location of the factory (in relation to water supply or fire brigades) and the closeness of workshops to each other. For a large firm the wisest course is to prepare a good valuation of assets and then try to obtain a reduction in the rate, but this way may not be open to a small firm. Some firms under-insure by a given percentage with the express approval of the insurer. Here the difficulty can be overcome by asking for insurable values rather than for the actual sums insured. The problem is really confined to firms who under-insure in relation to the provisions of the policy. It should be made clear that the insurer is not trying to discover whether or not manufacturers under-insure so long as no fire takes place, and in the event of fire he would reduce the claim in proportion to the degree of under-insurance.

2.25. The existence and often the extent of under-insurance can be discovered by questioning the insured on the technique used to arrive at valuation. Under-insurance must be understood, of course, strictly in relation to what is meant to be insured and account must be taken of (*a*) the basis of valuation, whether for reinstatement or for indemnity, (*b*) how far certain types of assets are not covered, and (*c*) the method of appraisal. For instance, an insured having a reinstatement policy and assets only a few years old may insure original cost even though the cost of replace--ment has risen since he bought the assets. He may exclude foundations and make an excessive deduction for them. But more generally, in a period of rising prices values are not regularly revised upwards, or revised by only an insufficient percentage. Some firms, even though they have a reinstatement policy, assume that the annual rise in prices just about offsets the depreciation of assets through age. Others add 50 per cent. to pre-war values instead of 300 per cent. The largest firms employ at least one clerk whose business is to report to the insurer the acquisition of new assets, and policies are frequently revised, pretending to accuracy; yet the policy may be substantially different from the correct value. The impression gained is that in a number of instances manufacturers take out a reinstatement policy and in the course of time allow it to approach the value of an indemnity policy, without however explicitly admitting this.

2·26. Considering the full information obtained from manufacturers, it is the conclusion of this section that the valuation of fixed assets for fire insurance gives a good basis for estimating, in an economically significant sense, the value of assets. But the figures which can be obtained from manufacturers are meaningless without an adequate explanation of the basis of valuation and of the coverage. In certain instances supplementary information is required on uninsured assets and on the degree of under-insurance. Apart from foundations, uninsured assets appear to be confined to a small number of industries, but the extent of under-insurance is difficult to gauge. The problem was partly by-passed through obtaining from the largest firms insurable values or other estimates of full replacement cost. It is also thought that firms who were aware of defects in their insurance preferred not to co-operate in the inquiry. In consequence under-insurance is unlikely to be the cause of important errors in this inquiry. In general, under-insurance to a serious extent, say by more than one-third of the full value, is not common and, as it is confined mainly to small firms, its importance in relation to total values insured should not be exaggerated.

3. THE ORGANIZATION OF A SAMPLE INQUIRY

3.1. In the organisation of the sample inquiry into fire insurance valuation, attention was paid to the experimental nature of this approach to estimating capital values and also to the need to obtain results with limited resources and quickly. Hence more emphasis was laid on procuring reliable information from a small number of large industrial units than on the development of adequate sampling techniques.

3.2. It is desirable that the results of this inquiry be related to the results of the census of production. But the establishment, which is the unit of inquiry in the census, is not a suitable unit to approach in a private inquiry of this nature. Apart from the difficulty of obtaining a sample list of establishments from the census authorities, firms may not have the required information available on an establishment basis. I was fortunate, however, because in connection with another inquiry at the National Institute very full information had been collected on public companies, supplemented by information on the largest private companies. These companies between them account for a high share of the country's industrial production. Lists were available classifying them by industry and size. Size is measured in terms of book values, with reference to either net assets or net fixed assets.

3.3. I have included in my sample all manufacturing companies with net fixed assets over £5 million in the balance sheet for the financial year ending in 1952–53, and a varying proportion, as shown in Table 1, of other companies. Private companies were added to the lists, as well as companies which had important manufacturing interests although their main business was not in manufacturing; in the latter instances the value of assets in manufacturing was estimated.* Although the lists for the smallest companies have not been adequately adjusted, the sampling fraction

TABLE 1

Results of Sample Enquiry into Value of Fixed Assets in Manufacturing

Book Value of Net Fixed Assets in 1952/3 *£ million.*	*Proportion of Companies in Sample*	*Number of Companies in Sample*		
		Total	*Usable Returns Obtained*	*No usable Returns Obtained*
Excluding breweries, etc.				
Over 5	1 : 1	94	53	41
2·5–5	1 : 3	27	14	13
1.0–2·5	1 : 8	28	8	20
0·5–1	1 : 20	14	4	10
0·5 and under	1 : 50	28	9	19
Total		191	88	103
Breweries, etc.		37	2	35
Grand total		228	90	138

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* The Co-operative Wholesale Society is treated throughout as if it were a company.

for these groups is small. For each size group companies were arranged according to industry—that is, what is believed to be their main line of business—and within each industry in alphabetical order. From each size group thus arranged every n^{th} name was taken, starting from an arbitrary point.*

3.4. The lists included only independent companies, that is, those which were not the legal subsidiaries of other companies (or rather of companies included in the list).† The unit of inquiry was the company together with all its subsidiaries, that is, companies in which it had an interest of over 50 per cent. The book values referred to were taken from consolidated balance sheets relating to the parent company and its subsidiaries.

3.5. Altogether 228 companies were included in the sample.‡ In each case the structure and business of the company (more exactly, group of companies) was investigated, making use of material available in the Institute, and then the company was approached, generally through its secretary. Letters were written from August 1955 onwards.

3.6. Companies were asked to give for a recent date the value for insurance purposes of (*a*) industrial and office buildings, excluding dwellings, and (*b*) plant and machinery, excluding vehicles. Only assets in the United Kingdom were to be included; in most cases assets abroad are held by separate subsidiaries and their exclusion is therefore not difficult, and in any case such assets would be covered by a different insurance policy. Distinct selling and other non-manufacturing assets were also to be excluded to bring the coverage close to that of the census of production. In some cases selling is undertaken by separate subsidiaries. Also some non-manufacturing assets, such as public houses, are easily distinguishable. But in some other cases this request did cause a certain amount of difficulty.

3.7. Companies were also asked to give a separate return for their activities in each of different manufacturing trades. This request was made only to companies which were believed to be engaged in different trades, but it caused most difficulties.§ In a few cases information was readily available in terms of census of production trades, and in some other cases, although it was not possible to separate different activities, one activity was dominant. In general, the value of assets can be obtained separately for each factory and some companies gave very detailed information indeed. But sometimes even very large factories are engaged in more than one trade and capital values could not be broken down. An instance of this difficulty is in the heavy electrical engineering trade where some of the activities nowadays overlap with mechanical engineering and with vehicle construction. It is, of course, easier to classify output than, say, break up a building between two activities. Also, in an enquiry of this nature excessive demands are likely to be met with complete refusal.

3.8. On the average about 3 returns were obtained per company supplying information but no significance can be attached to this figure as some companies gave no breakdown when it would have been appropriate, while others gave more than the necessary information. Also, although the unit of the inquiry was a given company or group of companies, the returns did not necessarily cover the whole group. Apart from overseas assets, subsidiaries (especially smaller ones) may have been omitted in order to facilitate the completion of the returns. In particular, some holding companies do not keep centrally a record of fire insurance values, and the request was transferred to the most important operating company or to a set of such companies; in the latter event some operating subsidiaries may have responded and others not. The degree of centralization of company organization differs very much from case to case and a private inquiry has to be adapted to this fact.

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* In fact, some of the size groups shown in Table 1 are the result of combining several size groups available. The sample was taken separately from each list. Similar lists are used by the Board of Trade in the new sample inquiries into investment expenditure, but the two samples are not the same.

† However, subsidiaries of foreign companies were included as well as subsidiaries of trading companies.

‡ In two cases substitutes were taken instead of the companies first chosen as these were found to be subsidiaries of other companies on the list. In a third case the company chosen suggested that the trade association be approached instead, and in this particular case a group of firms covered by the trade association has been substituted.

§ The request could not be made in exact terms because of the experimental nature of the inquiry. Having gained experience in classifying the results by trade, no doubt if the inquiry were repeated more exact requests could be made for the classification of a given company's activities.

3.9. Companies were asked to comment on the basis of valuation. In general, the basis of valuation is uniform for a group of companies but sometimes the different constituent units follow different policies. The comments received were most valuable and readily usable although in a number of cases further information was sought from the companies. Again, having gained experience of the types of valuation in force, if the inquiry were repeated it should be possible to ask more specific questions to elicit the nature of the valuation.

3.10. In addition, companies were asked to give the number of persons employed corresponding to the assets returned and for approximately the same date. It should be noted that some element of estimation is involved even in the data on employment, especially as there is no connection between records on insurance and records on employment, but on the whole I am satisfied that the data received on employment are comparable to numbers employed as given in the census of production.

3.11. The reason for asking for employment was that it was considered best to relate the sample to the universe in proportion to employment. Thus the sample is taken from a list of companies stratified by book value and by main activity. The returns received are then reclassified more or less according to census trades, and if necessary individual companies are divided between more than one trade. The universe is then taken as the whole of the census trades instead of listed companies only, and the size of the sample is established with reference to the number of persons employed. The book value of assets would not have been usable for "blowing up" the sample. It includes an unknown proportion for overseas assets, for non-manufacturing assets and for subsidiaries not included in the returns. Also, the relevant information is not available for other than public companies, and even then only a single figure is available for the aggregate of activities in several trades. Nor was it considered useful to obtain information on output and relate it to the census of production because of the great heterogeneity of commodities produced. Value added might have been an alternative measure but it was felt that firms would more readily supply information on employment.

3.12. Of the 228 companies included in the sample, 90 supplied usable returns. It is interesting to analyse the rate of response and the reasons for non-participation in the inquiry. In the first place, the response rate for breweries and other firms in the drink trades was extraordinarily poor: only 2 out of 37 in the sample gave information. The surprisingly large number of breweries, etc., in the sample is due to the organisation of the trade. Almost all these companies own public houses and in most cases the bulk of assets is in this form rather than in brewing, malting and bottling, but on the lists breweries were nevertheless classified as manufacturing companies. Moreover, the industry is largely a local one, relatively heavily capitalized, and thus a high share of it fell into the size groups where the chance of inclusion in the sample was large. Comments on the rate of response are more significant if breweries, etc., are left out of account.

3.13. Of 191 companies in the sample, other than breweries, etc., 88 gave information, little less than one-half. The rate of response, however, was correlated with size as measured by book values. While of companies with net fixed assets over £5 million, as seen from Table 1, more than one-half responded, in the next size group about one-half, and in the other groups only about one-third. This is not surprising and is no doubt partly due to the fact that the smaller companies have less time available for answering questions although the volume of requests for information from public and private bodies is just as large.

3.14. The response rate is shown in Table 2 by industry, in terms of numbers as well as book values. Again it is better to exclude breweries, etc., since the bulk of book values does not represent manufacturing assets. Excluding this group, the companies from whom information was obtained account for about 70 per cent. of the total book value of companies included in the sample. The response rate was best in tobacco—an interesting contrast between the two most heavily taxed industries. Relatively poor response rates are shown for iron and steel, shipbuilding and non-electrical engineering. Part of the explanation is that the de-nationalization of iron and steel companies, which has been in process during the inquiry, has put an unusual burden on company administrations and also may have made them reluctant to supply figures which were likely to be widely different from those at which steel companies were recently floated or were about to be floated. Further, both in breweries and in the iron and steel industry a significant proportion of assets is old and particularly difficult to value.

TABLE 2

Results of Sample Enquiry in Terms of Book Values of Fixed Assets

	Companies with Fixed Assets Over £5 million.				*Companies with Fixed Assets Under £5 million.*			
	Usable Returns		*No Usable Returns*		*Usable Returns*		*No Usable Returns*	
Group	*No.*	*Fixed Assets £ million.*	*No.*	*Fixed Assets £ million.*	*No.*	*Fixed Assets £ million.*	*No.*	*Fixed Assets £ million.*
1 Mining, quarrying and treatment of non-metalliferous mining products other than coal	3	32	3	32	4	5·3	3(1)	2·4
2 Chemicals and allied trades	9(2)	561	6	55	1	1·4	4	8·6
3 Iron, steel and non-ferrous metals	5	174	8	135	1	2·8	8	11·6
4 Shipbuilding and non-electrical engineering	2	12	4	40	6	15·7	10	14·0
5 Electrical engineering and electrical goods	5	66	2	12	2	7·9	4	6·4
6 Vehicles	5	42	2	36	3	7·1	5	9·7
7 Other metal goods, precision instruments and jewellery	1	10	1	20	2	2·3	5	4·6
8 Cotton	2	26	2	20	1	1·8	2	0·2
9 Wool	1	8	—	—	1	0·4	2	2·1
10 Other textiles	4	89	1	6	2	1·4	6	6·4
11 Clothing and footwear	1	10	—	—	—	—	2	1·6
12 Food	6	60	7(2)	52	2	1·3	4	8·2
13 Drink	1	15	18	223	1	1·8	17	35·3
14 Tobacco	3	90	—	—	1	4·1	—	—
15 Paper and printing	3	37	5	47	6	16·1	4	8·0
16 Other manufacturing industries	1	31	—	—	3	1·1	3	3·8
Not classified	2(2)	—	—	—	—	—	—	—
Total	54(4)	1,263	59(2)	678	36	70·5	79(1)	122·9
Total, excluding drink (group 13)	53(4)	1,248	41(2)	455	35	68·7	62(1)	87·6

Note.—The figures in brackets indicate the number of companies whose main activity is not in manufacturing. The fixed assets of these companies are not included in the table.

3.15. Of companies not giving information, approximately one-half, 55 companies, gave no answer whatever.* In each case the original letter was after a month's interval followed by a reminder and, after a further month, the company was included among those refusing to give information.† Among 48 companies who answered saying that they would not give information, 15 gave no reason or referred to the confidential nature of the information; 20 explained that they were too busy and staff was working under pressure, that it was complicated to extract the figures asked for, or that already a lot of information was collected by the government. Evidently my request necessitated the attention of senior staff and in some companies they were under particular pressure at the time owing to the end of the financial year, re-organization, new issues, etc. In 8 cases technical reasons were given for not supplying information, such as the existence of a large proportion of assets for which no reasonable value could be given, that manufacturing accounted only for a small proportion of the company's activity, that the insurance policy was under revision, and (in one case) that the company did not insure for fire. In five cases the companies definitely promised information, or prepared it ready for sending, but the information was not received.

3.16. Contact with companies threw light on their attitude not only to this inquiry but also to the problem of asset values in general. The fire insurance value of assets is of practical use not only in its own right but also as a realistic indication, unless a better measure is available, of the value of assets. The more progressive companies are aware of this and, even if the information is not published, use these figures for a number of internal purposes in assessing the performance of the company or of its constituent units. In these companies senior officials have the information in readily available form. In other companies, however, great difficulties were experienced in obtaining the figures or it was found impracticable to extract the information from scattered units of the company. Some companies were acutely aware that they did not possess satisfactory information on the value of assets.

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* This number includes two who asked for a copy of the original letter but, after this was sent to them, gave no answer.

† Some returns, however, came in subsequently, in which case the company was reclassified.

3.17. The attitude of companies to the release of information also varies greatly. The company secretary may release the information on his own responsibility, or instruct the chief accountant, insurance manager, or an outside broker to do so. Alternatively, he may have to consult one or more members of the board. Different authorities within the company may disagree and disagreement is bound to result in refusal.

3.18. The returns analysed relate to 1,100,000 persons* employed in manufacturing, about 13 per cent. of the national total. This result must be considered as satisfactory, especially since the percentage of assets covered was even higher, as will be shown in the next section. But the coverage varied greatly from industry to industry, owing partly to variation in the rate of response and partly to the nature of the sample. Evidently, for satisfactory results, in some industries a sample of public companies ought to be supplemented by additional inquiry.

3.19. The returns were "edited" to make them comparable. The information relates to around mid-1955 and small adjustments were made in returns not relating to this date to make an allowance for changing costs of construction. This allowance, a maximum of 2 to 3 per cent., is small relative to the uncertainty in the values. Next, additions were made for foundations and other "indestructible" assets. Where it was stated that foundations were excluded, or where this could be assumed, foundations were taken as 10 per cent. of the value of the building. For other excluded assets companies were asked to give an approximate estimate. Corrections were also made for disclosed under-insurance.

3.20. Assets were expressed on each return per person employed and the returns were classified. The possibility existed of stratifying the sample by trade, size of company, and the nature of the valuation (that is, replacement cost new or written down). Quite clearly the sample was too small for cross-stratification, and stratification by trade appears to be far more important than stratification by any other criterion. Assets per head shown on the different returns vary from a few hundred pounds to more than £12,000 and this variation is closely connected with the nature of the trade. In contrast, variation between the returns due to different types of valuation is at most from 50 to 100 per cent. of replacement cost new.

3.21. Differences between companies of different size were examined, but no systematic variation in assets per head was noticed. It was decided therefore to abandon the stratification of the sample by company size. Correctly, each return ought to be given a weight inversely proportional to the sampling fraction shown in Table 1 but this would give a spurious accuracy to the results. In any event, this procedure would hardly have made a difference to the industry averages and might indeed have made the results less reliable in so far as information obtained from smaller companies is generally less reliable than that obtained from large ones.†

3.22. The lack of difference between large and small companies may at first sight appear surprising as it is sometimes held that large companies are more heavily capitalized. However, within a given industry the possible higher degree of mechanization of the largest companies may well be offset by the fuller utilization of plant and equipment. Also, larger companies consist of several manufacturing units and very often represent a cross section of the industry while the smaller companies are specialized and may be more or less heavily capitalized according to the exact nature of the business.‡

3.23. As regards the basis of valuation, it was found that most returns adopted replacement cost new (or something very near it). This is partly because some important companies gave this value irrespective of their fire insurance values and partly because most fire policies were for reinstatement. It does not follow that the sample is in this respect typical of all industrial firms. Ideally, it would have been desirable to give two sets of estimates, in terms of both replacement cost new and written-down replacement cost. This, however, was not found practicable because of the scarcity of information on the latter basis. Instead, written-down replacement costs were converted into replacement cost new and all figures in this paper are presented in these terms. In some cases sufficient information was available to make the necessary estimates but in the nine most important cases where estimates had to be made they were submitted to the company for approval.*

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* Two companies were unwilling to give the figures asked for but agreed to give assets per employee. For these two companies employment is estimated.

† Partly because more effort was made to seek supplementary information from large companies.

‡ The smaller companies included in the sample are still large relatively to a significant proportion of most trades and the sample completely excludes "small business" proper. In this respect the sample is of course unrepresentative.

4. ESTIMATES FOR MID-1955

4.1. In estimating the value of assets in a given industry the greatest problem is that caused by heterogeneity. For any industry, even if the coverage of the sample in terms of employment is good, the sample contains only a few units, and it cannot be automatically assumed that it is representative of the industry as a whole. An attempt has been made, however, to group trades in such a way that the sample obtained for a given group could be considered as representative. The problem of estimation is converted into a problem of classification: no two firms are similar in every respect and the problem is to assimilate those parts of the universe for which information on assets is lacking to either one part of the sample or another. The main estimates are given in Table 3 and are based on procedures outlined in the following paragraphs.

4.2. The estimates were in fact made for a somewhat greater number of categories than is shown in Table 3 but some figures were either omitted or grouped mainly because individual estimates were not sufficiently reliable or to avoid disclosure of information. Employment, as shown in the first column, is estimated as official figures are not yet available. The estimates of the Board of Trade for 1954† were extrapolated to 1955 with reference, for each category, to the nearest Ministry of Labour series or group of series (the classification of trades by the latter being different).‡

4.3. The following comments on the individual estimates are called for:

China and earthenware.—The coverage is good but the figures were obtained indirectly through an intermediary and it is not absolutely certain that all assets are included at full replacement cost; it is possible that the estimates are on the low side.

Glass.—The sample includes mainly the manufacture of glass containers and the sample average is raised to allow for the higher level of assets in the manufacture of plate glass. The final estimates are still likely to be conservative.

Other non-metalliferous mining products.—This is a heterogeneous group containing bricks and tiles, roofing felt and other building materials. The coverage of the sample is very uneven within the group, but is considered as representative for the group as a whole. For an important sector not covered by the sample this was confirmed by a large firm not included in the sample inquiry. In the roofing felt trade the level of assets is much less than the group average, but in the production of the newer type of manufactured building materials it is higher.

Coke ovens.—The sample is small but is considered as representative.

Chemicals, including general chemicals, coal-tar products, fertilizers, dyestuffs, plastic materials, fine chemicals and explosives.—This group includes the manufacture of all heavy chemicals and some of the more finished lighter products. In the different sectors of the group the estimates for assets per head range from less than £3,000 to nearly £10,000. Details are not given for several reasons. Some of the largest plants are engaged in more than one trade and it would have been difficult to obtain estimates in sufficient breakdown. Further, for one important company, it was possible to obtain satisfactory estimates on the required definition for the above group of activities but more detailed information was available only on a less satisfactory basis. Also, for

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* Four companies agreed to the estimate, 2 revised it slightly downwards and 3 significantly upwards.

† These exclude outworkers but include working proprietors, and are annual averages. An allowance for small firms not covered by the census of production is included.

‡ These relate to Great Britain only. For the sake of simplicity of calculation, mid-year figures were taken for extrapolation but the results in Table 3 are intended to be estimates of annual averages.

TABLE 3

Replacement Cost New of Fixed Assets in Manufacturing, United Kingdom, Mid-1955

Standard Industrial Classification Minimum List Heading	*Industry*	*Employment Thous.*	*Employment on Sample Returns %*	*Total Fixed Assets* *Buildings, etc. £million*	*Plant, etc. £million*	*All £million*	*Fixed Assets per Person Employed* *Buildings, etc. £*	*Plant, etc. £*	*All £*	*Error in All Fixed Assets p.h. £*
21	China and earthenware	73	78	23	22	45	310	310	620	100
22, 23	Glass	77	3	42	50	92	550	650	1,200	200
24	Cement	13	31	20	20	40	1,570	1,530	3,100	300
	Other	159	8	118	146	264	740	920	1,660	200
Order III	Treatment of non-metalliferous mining products	322	24	203	238	441	630	740	1,370	110
30	Coke ovens	20	5	20	158	178	1,000	7,900	8,900	1,000
31, 33	Chemicals, etc.	203	43	199	869	1,068	980	4,280	5,260	600
32	Drugs and toilet preparations	64	17	52	50	102	810	790	1,600	300
34	Paint and varnish	40	16	32	27	59	790	680	1,470	100
35 (i)	Soap, etc.	23	44	24	38	62	1,040	1,660	2,700	300
36	Mineral oil refining	18	96	67	170	237	3,760	9,590	13,350	500
	Other	47	24	49	41	90	1,040	880	1,920	300
Order IV	Chemicals and allied trades	415	34	443	1,353	1,796	1,070	3,260	4,330	300
40, 41, 43	Iron and steel	276	33	216	791	1,007	780	2,880	3,660	300
42, 44	Foundries, tubes, etc.	171	1	86	188	274	500	1,100	1,600	300
49	Non-ferrous metals	113	30	119	164	283	1,050	1,450	2,500	400
Order V	Metal manufacture	560	23	421	1,143	15,64	750	2,040	2,790	190
50	Shipbuilding	223	—	89	112	201	400	500	900	400
51–69	Mechanical engineering, etc.	990	5	55	1,030	1,485	460	1,040	1,500	300
70–79	Electrical engineering, etc.	800	29	376	512	888	470	640	1,110	100
Order VI	Engineering, etc.	2,013	14	920	1,654	2,574	460	820	1,280	160
80, 82, 83	Motor and aircraft	647	23	394	788	1,182	610	1,220	1,830	200
	Other	248	—	124	124	248	500	500	1,000	300
Order VII	Vehicles	895	16	518	912	1,430	580	1,020	1,600	170

Standard Industrial Classification Minimum List Heading	*Industry*	*Employment Thous.*	*Employment on Sample Returns %*	*Total Fixed Assets*			*Fixed Assets per Person Employed*			*Error in All Fixed Assets p.h. £*
				Buildings, etc. £ million	*Plant, etc. £million*	*All £million*	*Buildings, etc. £*	*Plant, etc. £*	*All £*	
Orders VIII and IX	Metal goods n.e.s., precision instruments, etc.	608	5	255	414	669	420	680	1,100	200
113	Rayon, etc., production	38	80	62	112	174	1,630	2,940	4,570	200
	Other	902	6	1,100	1,342	2,442	1,220	1,490	2,710	400
Order X	Textiles	940	9	1,162	1,454	2,616	1,240	1,550	2,790	380
Orders XI and XII	Leather, fur, clothing, etc.	641	3	257	192	449	400	300	700	200
150	Grain milling	37	4	66	117	183	1,770	3,170	4,940	500
151	Bread, etc.	215	6	133	132	265	620	610	1,230	200
152	Biscuit	60	11	24	25	49	400	420	820	200
155	Sugar and glucose	19	76	28	75	103	1,470	3,950	5,420	200
156	Cocoa, chocolate, etc.	105	12	73	82	155	700	780	1,480	300
	Other food industries	219	10	182	243	425	830	1,110	1,940	400
163–168	Drink industries	138	1	310	242	552	2,250	1,750	4,000	600
169	Tobacco	37	87	30	30	60	810	820	1,630	100
Order XIII	Food, drink and tobacco	830	13	846	946	1,792	1,020	1,140	2,160	160
Order XIV	Manufactures of wood and cork	276	1	138	110	248	500	400	900	200
180, 181	Paper and pulp	88	22	123	291	414	1,400	3,300	4,700	300
182, 183	Cardboard and paper products	109	17	81	107	188	740	980	1,720	200
186	Newspaper printing	112	10	69	146	215	620	1,300	1,920	300
189	Printing	210	4	141	189	330	670	900	1,570	300
Order XV	Manufactures of paper	519	11	414	733	1,147	800	1,410	2,210	150
190	Rubber	110	33	55	138	193	500	1,250	1,750	100
	Other	131	1	83	98	181	630	750	1,380	200
Order XVI	Other manufacturing	241	16	138	236	374	570	980	1,550	120
Orders III–XVI	Total manufacturing	8,260	13·4	5,715	9,385	15,100	690	1,140	1,830	70

the group of trades as a whole the sample is considered as representative but this is not necessarily so for its individual components. It should be added that in the heavy chemicals trades the distinction between buildings and plant is of little significance as the two are integrated in construction. The break up of total assets between buildings and plant is partly estimated with reference to supplementary information, including statistics of capital formation in recent years.

Drugs and toilet preparations.—The sample shows a variation from £1,200 to £2,100 in assets per head and is not necessarily representative of these trades.

Paint and varnish.—The sample is small but can be taken as representative. Little variation around the average is shown.

Mineral oil refining.—The coverage of the sample was checked with reference to the throughput of crude oil. The sample is very satisfactory but the magnitude of the estimate depends on where the boundaries of this trade are drawn. In particular, oil companies may own jetties and harbour installations and may own, rather than rent, storage capacity. The degree to which these assets, and the corresponding numbers employed, are included, increases assets per head. The information obtained, however, is consistent with censuses of production and statistics of capital formation. For the division of assets between buildings and plant the same considerations apply as in the case of chemicals, etc., above.

Other chemicals trades.—The coverage of the sample is very uneven but all trades included appear to be among the lighter chemicals industries. Assets per head are considerably above the group average in vegetable oil mills.

Iron and steel.—The assets of these trades are particularly difficult to value. Fire insurance generally excludes a large share of assets and the basis of valuation may not be satisfactory. The data utilized are therefore estimates of replacement cost based mainly on the original cost of assets employed, inflated by cost indexes. The level of assets per head varies between companies and this variation is attributable to the extent to which equipment is up to date. The sample as a whole is considered as representing modernized and other companies equally. However, the Steel Company of Wales appears to be so different from other companies that its value was added separately to the estimated value of the rest of the industry. Its assets per head come to over £7,000, more than double the figure for the rest of the iron and steel industry. As regards the division of assets between buildings and plant, the same considerations apply as in chemicals. The total value of the iron and steel industry, including coke ovens (which are in the chemicals trades), is estimated here at nearly £1,200 mn. or approximately £60 per ingot ton. Present costs of constructing an up-to-date plant are about £75 to £80 per ingot ton and the two estimates are consistent since new plants are more highly mechanized. Assets per head vary between the different sectors of the industry but as the large firms contain integrated units it was not practicable here to distinguish detail. The high level of capital in the Steel Company of Wales, however, is difficult to explain by its product structure alone.

Other ferrous metals.—The coverage of the sample is particularly poor and the estimate takes account of assets per head in other trades. The figures shown are the average of a lower level for foundries and a higher level for tubes. The estimates given are unlikely to be on the high side.

Non-ferrous metals.—The coverage of the sample is good but this is a heterogeneous trade and there are differences in the level of assets between firms. In particular, the primary stages of production require more capital per head than the fabricating stages. The primary stages are more akin to the iron and steel industry. The production of primary aluminium is particularly capital-intensive in so far as the hydro-electric works, which are essential to this industry, are regarded as part of the trade; assets per head are near £10,000.

Shipbuilding.—No information was obtained and the figures given are in the nature of a guess, with a wide margin of error attached.

Mechanical engineering, etc.—Although the coverage of the sample is not large, a considerable number of returns were received representing different sections of the industry. The returns show a variation from one-half of the average level to one-half above it but the distribution appears to be normal. Because of great variations in the industry, it is not possible to comment further on the validity of the estimates given.

Electrical engineering, etc.—Here the coverage of the sample is good and appears to be well balanced between the different trades included in the group. It was not practicable to give a more detailed break up as the largest units are engaged in more than one trade. In fact, the data are likely to include elements attributable to the mechanical engineering and the vehicles trades. The level of assets in the largest firms, which have wide-spread interests in the electrical industries, are not far different from each other. In so far as it is possible to distinguish different sectors, assets per head are a little above one-half of the group average in the manufacture of domestic appliances and are nearly double the group average in the making of cables and wires. In electrical engineering proper, however, no great difference was found between the so-called heavy and light sectors.

Motor and aircraft.—It was considered best to take the manufacture of motor vehicles, cycles and aircraft together. The sample is satisfactory but shows considerable variations around the average. Assets per head appear to be relatively low in the manufacture of some components and relatively high in the making of engines and the assembly of motor vehicles. The range is from about one-half the group average to one-half above it. In aircraft manufacture assets include runways and installations for testing but the level of assets is not high, presumably because production is not as standardized as in motor vehicle production.

Other vehicle trades.—The sample is poor and the figures shown are guesses, with a wide margin of error attached. The group comprises mainly the manufacture of railway rolling stock on the one hand and motor vehicle repairing on the other.

Metal goods n.e.s. and precision instruments.—It was necessary to take these two major industrial orders together. The sample is small but probably representative. The variation is from one-half of the group average in the scrap trade to double the group average in trades making highly standardized products.

Rayon, etc. production.—The manufacture of man-made fibres is clearly more highly capitalized than the textile trades proper. The coverage of the sample is very good. Assets per head appear to be significantly lower in the manufacture of the newer fibres than in rayon production, probably because the former is more in the development stage.

Other textiles.—The coverage of the sample is reasonably good for cotton, woollen and rayon textiles, including finishing and including carpets. These trades were taken together as there does not appear to be any systematic difference between assets per head in the different trades. The coverage of the sample is small and not representative for hosiery, while the minor textile trades are not covered. It was assumed that all textile trades, other than rayon, etc., production, can be taken together. This assumption is not unreasonable as net output per head appears to be about the same in the trades not properly covered by the sample as in the major textile trades mentioned. The highest figure obtained for assets per head is 2½ times the lowest but most of the returns are near the average.

Leather, fur, clothing, etc.—The coverage of the sample is small and two major industrial orders were again taken together. In most trades, except in boot and shoe manufacture, assets per head are low and the final estimate is likely to be correct within the margin of error attached.

Grain milling.—Although the sample is small, it is thought to be representative. All mills appear to be relatively capital intensive.

Bread, etc.—A wide range is observed with concentration near the average. Although the trade contains many different units, the estimate is likely to be reasonable.

Biscuit.—The coverage of the sample does not appear small but nevertheless it may be unrepresentative. The estimates made may well be on the low side.

Sugar and glucose.—The coverage of the sample is very good. Assets in the beet sugar section and in the rest of the trade were separately estimated. Assets per head are considerably higher in the beet sugar section. For the most part this is due to the seasonal nature of beet sugar production and the results would be different if assets were related to peak instead of average annual employment.

Cocoa, chocolate, etc.—There is some danger of the sample being unrepresentative but the averages are probably correct.

Other food industries.—This is a very heterogeneous group (which includes some capital intensive products) and a much larger sample would be desirable for satisfactory results.

Drink industries.—The coverage is very poor. Breweries, which form the bulk of the group, are known to be capital intensive but valuation is inherently difficult. This is partly because the product is sold largely through tied houses and the problem of monopoly values arises, and partly because almost all breweries are very old.

Tobacco.—The coverage of the sample is very good and the level of capital in the different firms very similar, with the exception of one firm. Here assets per head are more than twice the average but the firm is known to have suffered from reduced activity at the time of the inquiry.

Manufactures of wood and cork.—The coverage of the sample is very small and the firms included cannot be taken as representative since in each case their main activity is in other industries. However, the final estimate given, which is significantly below the sample average, is consistent with information obtained in a more detailed inquiry into the furniture industry.

Paper and pulp.—The coverage of the sample is good but there is some evidence for believing that the level of assets varies with the product structure of firms. The estimate shown is slightly below the sample average.

Cardboard box and paper manufactures.—Only large firms are included but assets per head appear to be consistent between them.

Newspaper publishing and printing.—The coverage is reasonable though it includes an element of general printing. There is, however, evidence of important variations around the average.

Other printing.—The coverage is small and the firms included cannot be taken as representative. The figures obtained are consistent with newspaper printing, showing a generally lower level of assets.

Rubber.—The coverage of the sample is good and the figures for different firms are close together except one. This is about twice the average but the firm is mainly producing rubber chemicals.

Other trades.—The sample covers only some of the trades in the group. Assets per head are relatively high in the linoleum, etc., trade but nowhere in the group are they likely to be as low as in clothing or wood manufactures. The estimate for some of the trades not covered was made with reference to similar trades in other groups.

4.4. The margins of error attached to the estimates indicate the range within which the figures are likely to be.* In a number of instances the margins given indicate the likely upper limit but the likely lower limit should be well within the margins shown. The errors are obtained for the sub-totals and the total by the usual rule applicable to stratified sampling. The margin of error calculated for total manufacturing comes to 4 per cent. Margins of error are not shown separately for buildings and for plant and machinery. The margins of error are shown for assets per person employed as numbers employed are in the nature of weights. It can be taken that the percentage margin of error for the estimated value of assets in any industry is almost the same as for the corresponding estimate of assets per person employed although numbers employed are also subject to some error.

4.5. The replacement cost new of assets in manufacturing is estimated at about £1,800 ± £70 per person employed or £15·1 ± 0·6 thousand million in total. A little under two-fifths of the total consists of buildings and structures and the rest of plant and machinery.

4.6. It should be noted that employment is estimated on the definition used by the Board of Trade in censuses of production. On the more inclusive definition used by the Ministry of Labour, employment in United Kingdom manufacturing would be at least 1 million persons greater than given here.† Part of the difference is accounted for by persons not in manufacturing activities, e.g. lorry drivers, but in part it is difficult to explain. The adoption of the narrower definition is justifiable in so far as the amount of capital per head in marginal occupations is likely to be small.

4.7. A note should be added on the assets of the Atomic Energy Authority. Investment by the Authority is now included in statistics of capital formation under the heading "manufacturing—government research and development". Hence for the sake of consistency the assets of the Authority ought to be included here. They are not so included above because employment by the Authority is not covered by the Board of Trade figures and in any case assets per head in the Authority are very much higher than in the relevant parts of the sample. By making an appropriate allowance for the Authority, total manufacturing assets, on the definition used here, come to £15,300 million.

4.8. On the whole the coverage of the sample is better in the heavier than in the lighter in-

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* If a mathematical interpretation is necessary, these margins can be taken to correspond roughly to twice the standard deviation of the estimate.

† For 1954 the Board of Trade estimate of average annual employment in United Kingdom manufacturing came to 8,058,000 and the Ministry of Labour estimate for Great Britain only to 8,776,000. An even higher figure is given for manpower in manufacturing on a more inclusive definition of employment.

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dustries. As a result of this, while the sample accounts for 13 per cent. of the total number employed in manufacturing, it accounts for 17 per cent. of the estimated value of assets.

4.9. Ten trades or groups of trades are shown with assets over £3,000 per head—in decreasing order: mineral oil refining, coke ovens, sugar refining, chemicals, grain milling, paper, rayon, drink industries, iron and steel, and cement. These trades account in the aggregate for 10 per cent. of employment in manufacturing and 26 per cent. of total assets in manufacturing. At the other extreme four trades or groups of trades were given with assets per head under £1,000: clothing, etc., wood manufactures, china and biscuits.* These trades account for 13 per cent. of total employment and 5 per cent. of total assets. Seven-tenths of the total number employed are in trades with assets per head estimated between £1,000 and £2,000.

4.10. The figures for assets per head in electrical engineering may appear lower and those for textiles higher than expected. If anything, one would expect an under-estimate in textiles because plant is old and a lot of it is not going to be replaced. The explanation which might be offered for the difference is that in textiles capacity is generally under-utilized and single-shift working is the rule, while the electrical engineering industry was booming in 1955 and shift working was common. With full employment in both industries, and with the same amount of shift working, the difference may largely disappear.

4.11. This comparison, although probably a comparison of extremes, serves to illustrate one limitation of the concept of assets per person employed. But in spite of qualifications certain systematic differences between industries are found and trades can be divided, as above, into three more or less distinct categories: the capital-intensive industries accounting for one-tenth of total employment; the industries which have only recently left the handicraft stage and which account for about one-eighth of total employment; and the bulk which occupies an intermediate position.

Comparison with Redfern's estimate

4.12. The estimates of Redfern (1955)† are not directly comparable with the above figures but can be adjusted to compare them with my totals for manufacturing. Redfern's relevant category includes not only manufacturing, but also mining, other than coal; building; road transport; and distribution and broadcasting. Factories built by the government during the war and now leased to industry are excluded, as are the assets of government research and development.‡ Redfern's latest estimates are for the end of 1952 and are expressed in 1948 prices. These figures were brought up-to-date by adding gross capital formation in the following 2½ years and deducting for the assumed retirement of assets one-half of the corresponding amount estimated by Redfern for the five year period 1948–1952. The resulting estimates were then converted into 1955 prices. It is suggested by Redfern that manufacturing itself accounts for 80 per cent. of the plant and machinery and 95 per cent. of the industrial buildings included in his catch-all group, and the estimates were correspondingly reduced. Finally, £500 million was added to allow for factories built by the government during the war and now leased to private firms and for assets for government research and development (the latter consisting mainly of the assets of the Atomic Energy Authority). The two sets of estimates, in a comparable form, are shown in Table 4.

TABLE 4

*Comparison of Two Estimates of Fixed Assets in Manufacturing, United Kingdom, Mid-*1955

(In £ thousand million, 1955 prices)

	Buildings	*Plant and Machinery*	(*Total exc. Vehicles*)
Redfern[1]	3·5	6·6	10·1
Barna[2]	5·8	9·5	15·3
Difference as percentage of Redfern .	66	44	51

[1] Original coverage increased to include (*a*) factories built by the government during the war and now leased to private firms, and (*b*) assets for government research and development.

[2] As in Table 3, plus Atomic Energy Authority.

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* Shipbuilding is left out of account because of the uncertain estimate.

† The comparison is confined to Redfern's estimates of the gross stock of capital.

‡ In the latest blue books on national income, but not in those available at the time of Redfern's paper, capital expenditure on government research and development is classified under "manufacturing".

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4.13. The differences between the two sets of estimates, obtained by different methods, are so serious that I ought to discuss the factors which may possibly explain them. In the first place the share of manufacturing in Redfern's catch-all category may be higher than suggested. On the basis of post-war capital formation statistics, 83 per cent. may be more appropriate for plant than 80 per cent. and some addition may be made to buildings to allow for office buildings; but the difference is unlikely to exceed 5 per cent. under this heading. Further, it would be unwise to pay undue attention to the very large difference between the two estimates for buildings. The dividing line between buildings on the one hand and plant on the other is not unambiguous. Certain equipment, such as lifts, heating installations, kilns, etc., are sometimes regarded as part of the building and sometimes as part of plant and machinery. It is possible that many items, which for valuation purposes are regarded as part of the building, are counted in statistics of capital formation as plant and machinery.

4.14. Even so, the difference of the order of 50 per cent. for all fixed assets (excluding vehicles) remains. This can be due to (i) conceptual differences, (ii) imperfections in the mathematical formula employed by Redfern's perpetual inventory method, or (iii) differences in the statistical material and methods.

4.15. It is difficult to find conceptual differences between the two estimates. Redfern's "gross capital" is in fact replacement cost new. As regards technique of valuation, however, his approach corresponds to a short-cut method, mentioned in section 2, and is based on original cost multiplied by a price index. Much depends therefore on the conceptual basis of the price index used; in particular, it ought to make an allowance for changes in "quality". Redfern's price index is subject to criticism in so far as no corrections for quality are incorporated in it. But as the quality of capital goods has been improving over the years, this factor would make his estimates high rather than low.

4.16. The question of mathematical formula does not arise in my estimate, which is based on direct measurement. Redfern, however, uses an indirect method. He assumes that the stock of capital assets of a given type consists of gross investment aggregated over the previous l years, where l is the normal life assumed by the Inland Revenue. The deficiency of the mathematical formula implied is that it assumes that assets of a given type have a unique and constant length of life instead of taking life, more realistically, as a probability function. This aspect of the perpetual inventory method is more fully explored in the Appendix and it is shown that if the probability function of life is positively skewed, and if normal life is taken at the modal value, Redfern's method gives an under-estimate. However, if there is an increasing trend in gross investment, as there has been in the past, a tendency in the opposite direction is set up. Altogether, in the actual circumstances, the error involved in Redfern's formula could not have been large and could not have accounted for more than a small part of the difference between his figures and mine.

4.17. There remains the reliability of the statistical material and methods. To sum up my method: in so far as valuation is incorrect, it is likely to be an under rather than an over-valuation; it is possible that assets per head are lower in the small firms which were not taken into account* than in the larger firms which were included in the sample, and this may cause an over-estimate; however, the estimated averages were multiplied by employment in manufacturing on the Board of Trade definition which is narrower than the alternatives. All in all, there is no reason to suspect my estimate to be on the high side, and the likely margin of error in it is far less than the difference between my figure and Redfern's. Hence the major part of the explanation is likely to lie in the weaknesses of the statistical material and of the assumptions available when the value of assets is estimated by the perpetual inventory method. The estimates of capital formation for pre-war years, especially for the more distant past, are not particularly reliable and there may be an inherent tendency to under-estimation. It was noted by Redfern that the pre-war figures for factory building are especially subject to doubt. Further, it appears that expenditure on a number of assets, such as small tools, is for many purposes recorded on revenue account. However, when a valuation takes place, these assets are, quite rightly, included and are, for instance, insured for fire. Lastly, serious doubt must be thrown on the assumptions of life implied in the Inland Revenue depreciation rates.

4.18. The problem of depreciation, especially of depreciation for taxation purposes, is too

* Cf. 3.22, above.

large to be re-opened here. In recent years depreciation allowances for taxation contained an incentive element, and it is better to regard them as instruments of policy than as records of the past behaviour of capital assets. Moreover, income-tax depreciation rates are uniform over large classes of assets and over the life period of such assets; they do not take into account the particular shape which a true depreciation curve may take. It does not therefore follow that life can be simply computed from depreciation rates. For instance, if an asset depreciates fast in the first few years of its life, but rather slowly afterwards, if a simple depreciation rate is adopted this ought to be based on a shorter life than the actual. All that can be said is that a large amount of assets must be in existence which have survived the age at which they would have been scrapped according to Redfern's assumptions. In particular, this category includes all industrial buildings more than fifty years old and all plant, irrespective of type, more than forty-five years old. These assets were kept in use of course only by expenditure on their maintenance. Buildings, for instance can be kept in use almost indefinitely by periodic modernization. Statistically error enters the estimates in so far as such maintenance expenditure is not included in capital formation and this must have happened on a considerable scale. Thus, the propositions that statistics of capital formation were deficient, or that lives were assumed to be too short, are to a large extent alternative statements of the same source of error.

5. THE RELATION OF VALUE ADDED TO ASSETS

5.1. One immediate application of the new estimates of fixed assets is the examination of the relationship between the value of current production and assets employed in manufacturing. We expect the value added by an industry* to vary with the value of its assets. For this purpose stocks and work in progress, as well as fixed assets, have to be taken into account. There are other assets also employed in manufacturing which ought to be, but are not, taken into account: the value of land and of cash holdings is normally low in relation to total assets and, if groups of firms are taken together, debtors tend to cancel creditors.† Throughout, the estimates of assets, employment and value added relate to manufacturing activities proper and exclude transport by the manufacturer's own vehicles.

5.2. The levels of fixed assets, stocks and value added in 38 industrial groups—all figures expressed per person employed—are shown in Table 5 for 1954. The division of value added into wages and salaries on the one hand and the residue, which I call gross profits, on the other, is also shown. The estimates, other than for fixed assets, are based on censuses of production.‡ For stocks the means of figures for the beginning and the end of the year are taken. Value added was extrapolated from the 1953 census in proportion to the movement of wages and salaries as the preliminary results of the 1954 census gave gross output (value of sales plus increase in stocks) only. It was not possible to bring the estimates for stocks and value added up to 1955 and therefore my estimates for fixed assets were adjusted to a 1954 basis; I deducted a uniform 5 per cent., irrespective of industry, to allow for the somewhat lower quantity and lower price of assets in 1954.

5.3. Value added, as statistically determined, differs in several respects from the pure theoretical concept. The figures include certain overhead expenses, such as advertising, local rates and postal services. These are deducted in the estimate for the contribution of manufacturing given in the blue books on national income and the deduction, for manufacturing as a whole, appears to be about 10 per cent.§ Value added is also uncorrected for inventory profits or losses; these corrections were negligible in 1953 and 1954 for the economy as a whole although they may have been significant for particular industries. Finally, no deduction is made for depreciation. We have as yet no accurate information on the normal life of assets and rates of depreciation, and no

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* Equal to the value of production (sales plus increase of stocks) *less* materials, fuel and power, &c., consumed. I prefer the term "value added" to the official "net output" which is used in more than one context in economics.

† Net current assets (other than stocks, etc.) of public companies amount to about 5 per cent. of the replacement cost of fixed assets and stocks; but only some current assets are employed for manufacturing, while others are financial investments and are not reflected in value added.

‡ I am grateful to the Board of Trade for giving me data in addition to those published and for helping with the preparation of the estimates.

§ This is equivalent to a deduction of 24 per cent. from gross profits as shown here.

TABLE 5

Assets and Value Added per Person Employed, United Kingdom, 1954

Number in Charts	*Industry*	*Fixed Assets* £	*Stocks, etc.* £	*Total Assets* £	*Value Added* £	*Wages and Salaries* £	*Gross Profits* £	*Ratio of Fixed Assets to Value Added*	*Ratio of Total Assets to Value Added*	*Gross Profits to Total Assets* %
1	China and earthenware	590	110	700	490	350	140	1·2	1·4	20
2	Glass	1,140	220	1,360	770	470	300	1·5	1·8	22
3	Cement	2,950	680	3,630	1,870	620	1,250	1·6	1·9	34
4	Other non-metalliferous mining products	1,580	250	1,830	820	500	320	1·9	2·2	17
5*	Coke ovens	8,450	340	8,790	1,100	520	580	7·7	8·0	7
6	General chemicals, plastics, etc.	5,000	510	5,510	1,150	560	590	4·3	4·8	11
7	Drugs and toilet preparations	1,520	500	2,020	1,210	420	790	1·3	1·7	39
8	Paint and varnish	1,400	550	1,950	1,100	500	600	1·3	1·8	31
9	Soap, etc.	2,560	620	3,180	1,220	490	730	2·1	2·6	23
10*	Mineral oil refining	12,680	2,040	14,720	1,600	560	1,040	7·9	9·2	7
11	Other chemicals	1,820	730	2,550	1,220	500	720	1·5	2·1	28
12	Iron and steel	3,480	530	4,010	1,080	540	540	3·2	3·7	13
13	Foundries, tubes, etc.	1,520	280	1,800	810	500	310	1·9	2·2	17
14	Non-ferrous metals	2,370	600	2,970	880	530	350	2·7	3·4	12
15	Shipbuilding	860	70	930	630	490	140	1·4	1·5	15
16	Mechanical engineering, etc.	1,420	630	2,050	810	500	310	1·8	2·5	15
17	Electrical engineering, etc.	1,050	420	1,470	730	460	270	1·4	2·0	18
18	Motor and aircraft	1,740	530	2,270	840	540	300	2·1	2·7	13
19	Other vehicles	950	390	1,340	600	470	130	1·6	2·2	10
20	Metal goods n.e.s., precision instruments, etc.	1,050	310	1,360	720	430	290	1·5	1·9	21
21	Rayon, etc., production	4,340	500	4,840	1,580	510	1,070	2·7	3·1	22
22	Other textiles	2,570	450	3,020	650	360	290	4·0	4·6	10
23	Leather, fur, clothing, etc.	670	260	930	480	320	160	1·4	1·9	17
24	Grain milling	4,690	850	5,540	1,290	470	820	3·6	4·3	15
25	Bread, etc.	1,170	60	1,230	640	370	270	1·8	1·9	22
26	Biscuit	780	180	960	610	320	290	1·3	1·6	30
27	Sugar and glucose	5,150	1,260	6,410	1,270	550	720	4·1	5·1	11
28	Cocoa, chocolate, etc.	1,410	580	1,990	900	370	530	1·6	2·2	27
29	Other food industries	1,840	450	2,290	840	390	450	2·2	2·7	20
30	Drink industries	3,800	1,220	5,020	1,390	430	960	2·7	3·6	19
31	Tobacco	1,550	1,520	3,070	1,600	440	1,160	1·0	1·9	38
32	Manufactures of wood and cork	860	310	1,170	660	440	220	1·3	1·8	19
33	Paper and pulp	4,460	490	4,950	1,140	500	640	3·9	4·3	13
34	Cardboard and paper products	1,630	280	1,910	690	390	300	2·4	2·8	16
35	Newspaper printing	1,820	110	1,930	1,010	590	420	1·8	1·9	22
36	Printing	1,490	250	1,740	690	450	240	2·2	2·5	14
37	Rubber	1,660	390	2,050	810	460	350	2·0	2·5	17
38	Other manufacturing	1,310	280	1,590	680	400	280	1·9	2·3	18
	All manufacturing industries	1,740	440	2,180	780	450	330	2·2	2·8	15

* Numbers 5 and 10 are not shown in the charts.

estimates can be given by industry; but, for manufacturing as a whole, depreciation is likely to be around 5 per cent. of value added. It must be also remembered that the estimates were obtained by extrapolation from 1953 on the implicit assumption that gross percentage profit margins were the same in 1954.

5.4. Stocks and work in progress, as can be seen from Table 5, amount to one-fifth of total assets in manufacturing. The value of stocks and of fixed assets is equal only in the tobacco trade where stocks of raw tobacco are valued inclusive of the heavy customs duty. On the other hand the low stock figure for shipbuilding is the result of statistical conventions as ships under construction are counted in the same way as finished ships, instead of as work in progress. These are comparisons between stocks and fixed assets where the latter is taken at replacement cost new; the importance of stocks would appear greater in relation to the written-down replacement cost of fixed assets. If written-down replacement cost is assumed to be two-thirds of new values, stocks would amount to approximately 40 per cent. of the value of fixed assets.*

5.5. The ratio of fixed assets to value added varies from about 1 to about 8 between the industrial groups shown in Table 5, with an average of 2·2 for manufacturing as a whole. The ratio is lowest in the tobacco and the drug and toilet preparations trades—where value addded is high—and in the china and earthenware, wood manufactures and biscuit trades. It is highest in mineral oil refining and coke ovens and next highest—around 4—in the general chemicals, sugar refining, paper and pulp, and man-made fibres trades. Inclusive of stocks the ratios vary from 1·4 to about 9, with an average of 2·8. If value added is corrected for statistical imperfections (but not for depreciation) and if assets are taken on a more inclusive definition,† the average ratio would become 3·1 to 3·2. In other words, somewhat over £3 was required to replace manufacturing assets contributing £1 to *gross* national income. But the use that can be made of this statement is very much reduced by the great variation round the average ratio which was observed.

5.6. In measuring the relationships between value added and assets, as well as in the two charts, 36 industries are taken. Two trades distinguished in Table 5—mineral oil refining and coke ovens—are omitted from consideration in the rest of this section. In both, the figures for value added are to some extent arbitrary as the trades are part of integrated industries—mineral oil refining is carried on by firms which control the production of crude oil and the distribution of the final product, and coke ovens are owned either by steel works or the coal mines. Prices at which value added is calculated are not free market prices but rather intra-firm accounting prices.‡ The two trades omitted happen to be the two with the highest level of assets but both are very small in terms of employment and their inclusion would have had an unduly large influence on the calculations which follow.§

5.7. Further, in the following calculations each of the 36 industries was taken without a weight attached to the estimates, which are all expressed per person employed. The alternative of weighting with employment was rejected because of the very large differences in size between industries: to a great extent these differences are the result less of design than of the available empirical data. Industries with the highest level of assets are the smallest in terms of employment partly because in the larger groups trades with exceptionally high and exceptionally low levels of assets would be averaged. The usual regression analysis is therefore more applicable to the unweighted data, as with weighted data the distribution of the variables would be far from normal.

5.8. Little purpose would be served by taking stocks and fixed assets separately as factors influencing value added. It is interesting to note, however, that there is a strong correlation between the two types of assets, with $r = 0\cdot534$. Writing s for stocks per employee and f for fixed assets per employee, both in £, the regression line is

$$s = 209 + 0\cdot133\,f \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (1)$$

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* In balance sheets stocks appear even more important since their book value is the same as the values used here but the book value of fixed assets is generally much below written-down replacement cost.

† cf. 5.3 and 5.1 above.

‡ For some years the censuses of production show practically no difference between value added and wages and salaries paid in mineral oil refining.

§ Shipbuilding and "other vehicles" are not omitted but attention must be called to the large margin of error in the estimates for fixed assets in these trades.

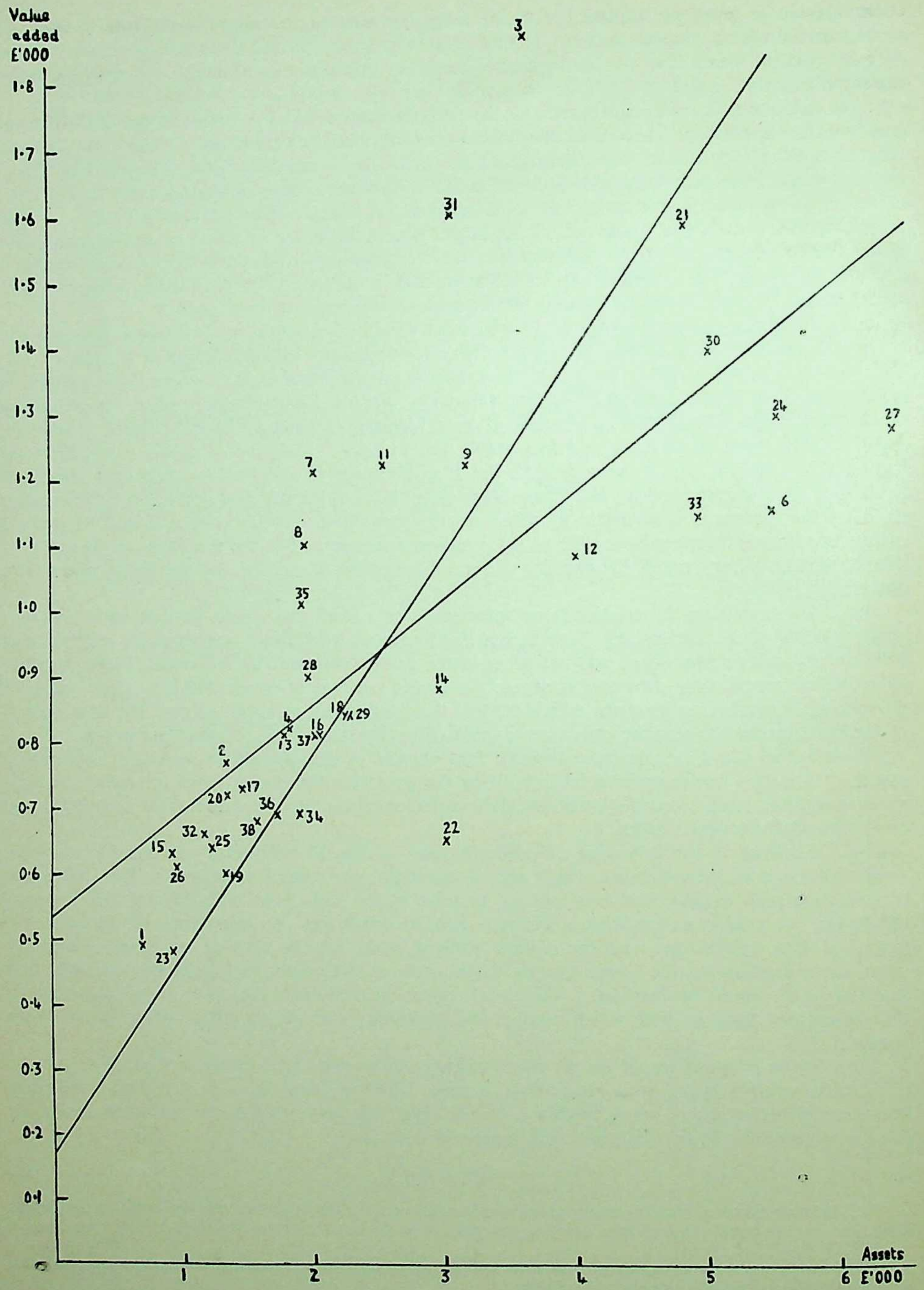

CHART I.—Value added and assets per employee in different manufacturing industries, United Kingdom, 1954
Note: For key to numbers, see Table 5.

There appears to be a basic quantity of stocks, £209 per head, supplemented according to industry by 13 per cent. of the replacement cost of fixed assets.

5.9. Let us now write v for value added per employee per annum and $k(= f + s)$ for total assets per employee, both in £. The correlation between v and k is indeed high, with $r = 0{\cdot}726$ which is significant far below the 1 per cent. level of probability. The two elementary regression lines, which minimize the squares of the deviations in the direction of v and k respectively, are

$$v = 532 + 0{\cdot}161\,k \qquad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (2)$$

$$k = -540 + 3{\cdot}28\,v \qquad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (3)$$

or, expressing (3) differently,

$$v = 165 + 0{\cdot}306\,k \qquad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (3')$$

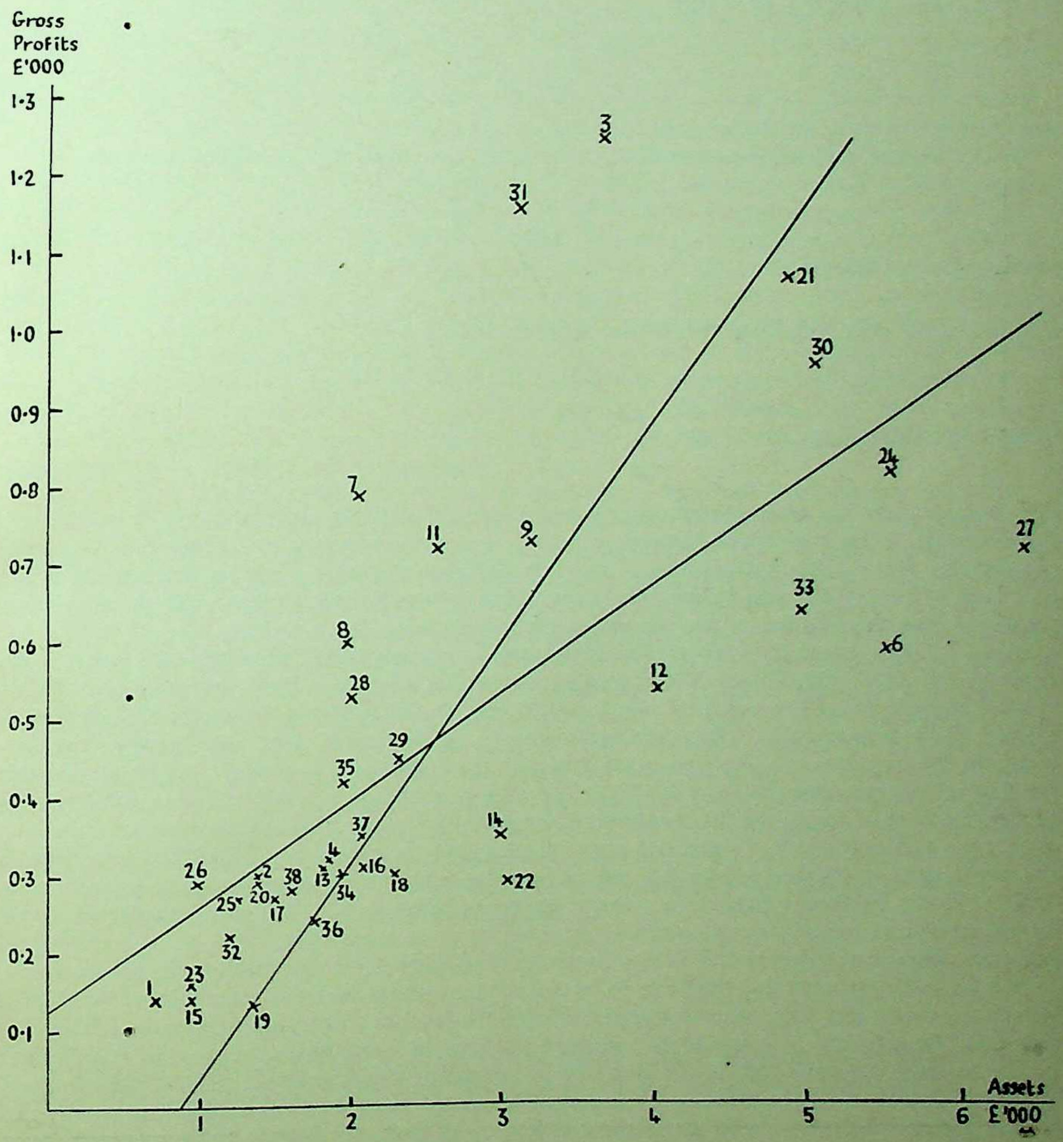

CHART II.—Gross profits and assets per employee in diffent manufacturing industries, United Kingdom, 1954
Note: For key to numbers, see Table 5.

5.10 The linearity of the relationship between v and k is not surprising—and at the same time it is convenient as some of the industries distinguished are in fact aggregates of several trades.* If we write v' and k' for the theoretical variables corresponding to v and k respectively, then a unique linear relationship

$$v' = a + b\,k' \qquad (4)$$

will obtain under certain conditions. These conditions are that labour and capital are homogeneous, that the economy is competitive and that it is in long-term equilibrium. For the present purpose these conditions need not be interpreted strictly: it is enough that the average quality of labour and capital, the price of labour and the rate of return on capital should be the same between industries. If this is so, a is the average wage per employee in industry and b the rate of return on assets employed.

5.11. Let us write $v' = w' + p'$, where w' is wage or salary and p' is profit, interest or rent per employee. It then follows that

$$w' = a \qquad (5)$$

and

$$p' = bk' \qquad (6)$$

where a and b are the same constants as in (4).

5.12. Fitting the empirical data to equation (5), the elementary regression lines are

$$w = 403 + 0{\cdot}023\,k \qquad (7)$$

$$k = -\,2{,}006 + 9{\cdot}84\,w \qquad (8)$$

which can be written as

$$w = 203 + 0{\cdot}102\,k \qquad (8')$$

and fitting the data to equation (6), the regression lines are

$$p = 129 + 0{\cdot}138\,k \qquad (9)$$

$$k = 864 + 3{\cdot}50\,p \qquad (10)$$

which can be written as

$$p = -\,247 + 0{\cdot}285\,k \qquad (10')$$

Any one of these sets of equations can of course be arithmetically derived from (2) and (3) and the other set. The correlation between p and k is almost as strong as between v and k, with $r = 0{\cdot}694$. The correlation between w and k is also strong, with $r = 0{\cdot}388$ which is significant at a level of probability just below 1 per cent. It is, of course, noted that while the theoretical equations pass through the origin, the empirical lines do not.

5.13. The question is which coefficients to take to represent the true values of a and b. The value of a is £532, 165, 403 or 203, according to whether we take it from equations (2), (3′), (7) or (8′), respectively. The value of b is 0·161, 0·306, 0·138 or 0·285, from equations (2), (3′), (9) and (10′), respectively. Three problems have to be discussed: first, whether the statistical errors in the variables are likely to cause a bias in the coefficients; secondly, whether to accept the first set of equations (that is, (2)–(3)) or the other sets (that is, (7)–(10)); and thirdly, whether in any given set to accept the first or the second regression line, or a combination of the two.

5.14. The only kind of statistical error which might be a source of bias in the estimates of the coefficients is that caused by not adjusting value added to a more satisfactory conceptual basis. Of the omitted adjustments, which are enumerated in 5.3, the most important is the failure to deduct miscellaneous overhead expenses, such as advertising, local rates, etc. As the necessary empirical information is not available, two alternative assumptions might be made. First it could be assumed that the items to be deducted are about the same per employee in different industries, or, at any rate, are not correlated with the level of assets and with value added. In that case the estimate of b would be unbiased but that of a would exceed the theoretical value by the average per employee of the items to be deducted, which is about £70–80. Secondly, it could be assumed that the items to be deducted are proportionate to value added, or at any rate are correlated with it and with assets per employee. In that case both a and b would be overestimated by about 10 per cent. A consideration of the nature of the items to be deducted suggests that the first assumption is more plausible.* Whatever bias there may be in the estimates, however, does not affect the choice between the different sets of equations.

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* If the relationship between the variables is linear, aggregation does not cause a bias in the estimates of the coefficients.

5.15. Evidently wages, as well as gross profits, are correlated with the level of assets; in other words, part of the higher value added which is associated with a higher level of assets goes to wages and part to gross profits. This may be due to one of two causes. It is possible that a higher average wage is associated with a higher level of assets, and in that case clearly b should be estimated from (9)–(10). Or it is possible that the rate of pay for the same type of labour is associated with the level of assets and in that case b should be estimated from (2)–(3). This estimate would represent the marginal product of capital irrespective of whether it is distributed as the remuneration of capital or of labour. The simple correlation between w and p is naturally significant but the coefficient of partial correlation, eliminating the effect of k, is $r = 0{\cdot}09$ which is not significant. There remains, however, the fact that the more capital intensive industries are also, on the whole, the expanding industries and may pay better wages to attract labour; they certainly pay higher average wages because shift working is more common than it is among the less capital-intensive industries. The capital-intensive industries may, of course, be in a better position to pay higher wages, as profits *per employee* are higher even if the rate of profit on capital is the same as in other industries. Without further research it is difficult to say which of these two explanations is correct. The likelihood is that they both contain some truth, the first probably more than the second. For this reason the true value of b is somewhere between the values given by (2)–(3) and (9)–(10). But the difference between the two sets of equations in this respect is only about $0{\cdot}02$, which is much smaller than the difference between (2) and (3′), *or* between (9) and (10′).

5.16. The practical problem is not so much which set of equations to take but rather which elementary regression equation to take within a given set. The question of a unique regression line which would describe functional relationships was investigated by Allen (1939). Following Allen, the error term is related to the standard deviation of the variable which gives a measure of the "intensity of deviations" in the variable. If there is only a moderate positive correlation between the deviations in the two variables—and this can be assumed in the present case—the true regression line lies between the two elementary lines and nearer to the one where the explanatory variable has the smaller intensity of deviations.

5.17. There are two broad reasons for the deviation of variables from true values; errors of measurement, and deviations in the variable to be explained by effects other than those of the explanatory variables. The variables v, p and k are each subject to errors of measurement. Although the statistical error proper is likely to be greatest in k, errors due to deviations from the theoretically correct definitions, which were noted in 5.3 above, are more important in v and p. It is even more important that the abstract conditions which make the functional relationship in (4) possible are absent from the real world and therefore k is insufficient in itself to explain variations in either v or p. Hence the intensity of deviations is much greater in v or p than in k, and the true regression line should lie nearer to the first elementary line in either set, though somewhat in the direction of the second line. If we look at Chart I (or Chart II) the new line is obtained by rotating the line with the less steep slope round its point of intersection with the other line in an anti-clockwise direction. I have divided the *angle* between the two lines (in both charts) in the proportion of 1 to 5, which appears to me reasonable in the light of the argument above.† This division is, however, subjective—apart from the requirement that the new line ought to be nearer to the first than to the second regression line—and other workers may adopt a different solution.

5.18. The "true" regression lines thus obtained are

$$v = 458 + 0{\cdot}19\,k \qquad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (11)$$

and

$$p = 72 + 0{\cdot}16\,k \qquad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (12)$$

The following properties of these lines should be noted. First, each of the coefficients in (11) and (12) differs from the corresponding coefficient in (2) and (9) approximately by the standard error of the latter. Thus (11) and (12) are within the fiducial limits of (2) and (9) respectively. Secondly, line (12) does not pass through the origin, like the theoretical line in (6), but cuts the vertical axis at a point (£72) roughly corresponding to the average per employee of the expenditures not deducted from value added. Similarly, the coefficient of £458 in equation (11) can be regarded as composed of the same amount (£70–£80) and of a "basic" wage (£380–£390). The latter is consistent with the relationship in (7). To sum up: value added per employee consists of a "basic" wage of £380–£390, *plus* a premium for extra skill above the average, *plus* 16–19 per cent. of the replacement cost of assets, *plus* £70–£80 for the overhead expenses mentioned above. It should be emphasized that these findings are provisional as regards both methods and statistical sources used.*

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* cf., however, my answer to the discussion.

† For the purpose of this division, the variables were represented on an identical scale. In the charts, however, £5 on the horizontal scale corresponds to £1 on the vertical one.

5.19. It is possible to estimate roughly the net return on capital employed for manufacturing as a whole. Depreciation is likely to be 2½–3½ per cent. of the replacement cost new of fixed assets so that net return on *gross* capital comes to 13–17 per cent. But net return should be related to net capital; not so much to its actual value but rather to some value corresponding to the equilibrium age-composition of assets. As fixed assets withdrawn from use have some scrap value, and often still represent a substantial proportion of replacement cost new, and as the rate of interest also enters the calculation, the net (that is, written down) value of fixed assets throughout life might average approximately two-thirds of replacement cost new. In that case return on net capital amounts to 18–23 per cent.

5.20. The data collected for this inquiry are not sufficient to say how far the rate of net return varied from industry to industry. It is not even possible to foresee how far a refinement in the concept and measurement of value added would lead to closer relationships between v (or p) and k. Nevertheless the deviation of some industries from the ruling relationships shown in Charts I and II is significant. In both charts the textile trades (other than man-made fibres) are very much below the regression line and the cement, tobacco and drug and toilet preparations trades very much above it.† For textiles the employment of female labour is one part of the explanation and the depressed state of the industry another. The tobacco and the drug and toilet preparations trades are among the heaviest advertisers, and advertising expenditure is not deducted from value added, which may in part explain the position of these trades. It is difficult to find a similar explanation for cement.

5.21. The capital-output ratio, which is much discussed in economic literature, is written in the terms used here as k'/v'. It is clear from the form of equation (4) that the ratio is increasing with the amount of assets employed, but the marginal capital-output ratio is constant, being $1/b$. The numerical value of the incremental ratio is about 5, compared with an average ratio of about 3 which is referred to in 5.5. This result, which is based on cross-section data, is in interesting contrast with results based on time series, e.g. by Creamer and Bernstein (1954) for the United States. The latter show average ratios of the same magnitude as, or smaller than, that given here but the marginal ratios are equal to or smaller than the average ratios. According to my analysis an investment of about £5 is needed to increase the contribution of manufacturing to the national income by £1 but according to alternative findings only about £2 is needed; I offer a possible explanation of these discrepancies elsewhere (Barna, 1956).

6. THE CURRENT RATE OF INVESTMENT

6.1. Information on gross fixed capital formation has been regularly collected by the government since 1948 and published in the blue books on national income. Hitherto it has not been possible to relate statistics of capital formation to a realistic estimate of the stock of assets of a particular industry. An attempt to do so is made in this section.

6.2. The usefulness of the information given in the blue books has improved with the 1956 issue as a larger number of industries is distinguished than before. Previously only the Standard Industrial Classification's 14 major manufacturing orders were distinguished; in addition, mineral

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[*] In particular, it should be possible to improve the estimates of gross profits by a careful reconciliation of the census of production data with those of the Inland Revenue.

[†] The larger industries may also include sections with exceptionally high or low profits but the effect of this would cancel within the industry, and therefore would not be apparent in the data.

oil refining was separated from other chemicals trades, and iron and steel from non-ferrous metals.* Textiles, for instance, are now split into man-made fibres and traditional textiles as there are reasons to assume that the movement of investment in the two sections was different. However, the additional information is available for the years 1951–1954 only and for the complete period since 1948 only the classification given in Table 6 is possible.†

6.3. Another obstacle to analysis is that capital formation financed by government expenditure and for government research and development are shown as separate categories and are not allocated to particular industries. The former consists of government armaments factories, purchases of machine tools abroad on government account, and factories built by the government and leased to industry. In the latter category the largest share is represented by assets of the Atomic Energy Authority. These items altogether account for one-tenth of post-war capital formation in manufacturing. It should also be noted that the figures on capital formation are not truly gross as sales of second-hand assets are deducted. But the balance of these deductions, equivalent to sales of assets for scrap, is small and is neglected here.

6.4. The data on gross capital formation were completed by estimates for the first half of 1955, all figures were converted to mid-1955 prices, and totals computed for the 7½ year period from January 1948 until mid-1955. These are shown in Table 6 separately for buildings and for plant and machinery (excluding vehicles). It can reasonably be assumed that assets acquired since 1948 were still in existence at mid-1955 and thus the estimates represent the amount of assets less than 7½ years old at mid-1955. 27 per cent. of all assets fall into this category, the proportion ranging from 16 per cent. to 36 per cent. according to industry, except that the mineral oil refining trade is almost wholly of post-1947 origin. The industries where the highest proportion of assets is of recent origin are the bricks, pottery, glass and cement trades (taken together); iron and steel; and the chemicals and allied trades. At the opposite end of the scale are the vehicles trades; leather and clothing; and the textiles trades. The proportion is industry by industry consistently higher for plant and machinery than for buildings.

TABLE 6

Gross Fixed Capital Formation (Excl. Vehicles) from Jan., 1948, *to Mid-*1955, *United Kingdom Manufacturing*

	In 1955 Prices, £ million.			*In Percentage of 1955 Stock*		
	Buildings	*Plant*	*Total*	*Buildings*	*Plant*	*Total*
Government financed expenditure	91	159	250	..	..	..
Bricks, pottery, glass, cement, etc.	45	113	158	22	47	36
Mineral oil refining . . .	51	165	216	..	..	91
Other chemicals and allied trades	108	378	488	29	32	31
Iron and steel	90	327	417	30	33	33
Non-ferrous metals . . .	13	55	68	11	34	24
Engineering, shipbuilding and electrical goods . . .	146	381	527	16	23	20
Vehicles	70	200	270	13	22	19
Metal goods n.e.s., precision instruments, etc. . . .	46	133	179	18	32	27
Textiles	85	323	408	7	22	16
Leather, leather goods, fur and clothing	22	54	76	9	28	17
Food—drink and tobacco . .	116	331	447	14	35	25
Manufactures of wood and cork .	16	39	55	12	35	22
Paper and printing . . .	46	183	229	11	25	20
Other manufacturing . .	20	74	94	15	31	25
Government research and development	122	73	195	..	..	..
Total manufacturing . .	1,087	2,991	4,078	19	32	27

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* Tobacco was also shown separately before but is now taken together with the drink industries. Two major orders which account for only a small share of capital formation in manufacturing—leather and fur, and clothing, &c.—are now taken together.

† Except that I have shown as one item metal goods n.e.s. and precision instruments, etc.

6.5. Capital formation consists, of course, partly of replacement and partly of additions to assets. It must be remembered in interpreting these figures that assets which are less durable need to be replaced more frequently. This explains why the percentages shown are lower for buildings than for plant and machinery. On the whole, however, the share of recently installed assets appears highest in the industries where they probably last longest; if all industries were to expand at the same rate, this would not be so. The relatively unfavourable position of the engineering and vehicles trades will be noted. Perhaps their position would improve if account were taken of their share in government financed capital formation. But it is also possible that advances in, e.g. the aircraft and motor vehicles trades were offset by lack of progress in the other vehicles trades (railway rolling stock, mine wagons, prams and carts). Lastly, it should be borne in mind that the comparison of recently installed with old assets is in terms of replacement cost new, and a comparison in terms of some measure of physical capacity might yield somewhat different results.

6.6. For manufacturing as a whole gross fixed capital formation (excluding vehicles) is shown for each year since 1948 in Table 7 in current as well as in 1955 prices. A rough indication is given of gross capital formation as a proportion of the corresponding stock of assets in existence. Apart from the setback in 1952–1954, this proportion rose from 3¼ per cent. in 1948 to 4¾ per cent. in 1956. The last figure is based on investment plans as ascertained in the summer of 1956 and probably represents the highest rate since the beginning of this century. These are of course not rates of growth as no deductions are made for the scrapping of assets.

TABLE 7

Gross Fixed Capital Formation (Exc. Vehicles), U.K. Manufacturing

	Current Prices £ *million.*	1955 *Prices* £ *million.*	*Percentage of Current Stock*
1948	322	428	3¼
1949	368	481	3½
1950	434	559	4
1951	505	603	4¼
1952	534	574	4
1953	523	545	3¾
1954	557	578	4
1955	687	687	4½
1956 forecast*	810	770	4¾

* Based on *Board of Trade Journal*, August 4th, 1956.

6.7. The forecasts for 1956 are analysed in more detail in Table 8. Data are given for nine sectors of manufacturing, expressed in current prices and related to the corresponding stock of

TABLE 8

Privately Financed Gross Fixed Capital Formation (exc. Vehicles) in Manufacturing: Forecast for 1956*

S.I.C. Order		£ *million*	*Percentage of Current Stock*
III	Non-metalliferous mining products	29	5·9
IV	Chemicals, etc.	125	6·2
V	Metal manufacture	106	6·0
VI, VIII, IX	Engineering and metal goods (exc. vehicles)	144	4·0
VII	Vehicles	99	6·2
X, XI, XII	Textiles and clothing	69	2·1
XIII	Food, drink and tobacco	96	4·9
XV	Paper and printing	86	6·7
XIV, XVI	Other manufacturing	32	4·6
	Total	786	4·7

* Based on *Board of Trade Journal*, August 4th, 1956.

assets. The rate appears highest in paper and printing (6.7 per cent.), and next in chemicals, vehicles, metal making, and non-metalliferous mining products (about 6 per cent.). It is lowest in the textiles and clothing trades (2 per cent.). The figure for the food, drink and tobacco industry, in the same way as in Table 6, is higher than that for the engineering trades. Although no further breakdown of the figures is possible, a study of statistics of capital formation in 1953 and 1954 suggests that the rate of growth was probably much greater in man-made than in traditional textiles; greater in the motor and aircraft than in the other vehicles trades; and greater in the food than in the drink and tobacco trades; but that it was about the same in paper and pulp as in paper products and printing.

6.8. In the absence of empirical data on the life of assets, it is difficult to convert these figures into rates of growth in particular industries. It would not be wrong, however, to assume that the average life of assets in each of the broad industrial categories shown in Table 8 is between 22 and 45 years, and thus in the long-run 2 per cent. to $4\frac{1}{2}$ per cent. of the stock of assets would be replaced each year. But as the stock of assets has been growing, and therefore its average age is less than that corresponding to a stationary equilibrium, actual replacement needs are somewhat below the long-term needs. At a guess, one might say that in 1956 the stock of assets distinctly increased in the paper and printing, chemicals, vehicles, metal making and building materials trades, but that in the textiles and clothing trades it probably slightly declined. The increase in the engineering and metal goods trades, if any, must have been small.

7. Conclusions

7.1. The experience of this inquiry yields two types of conclusion, those regarding methods of research and sources of data, which are relevant for future work in this field, and those which may be of practical importance for economic analysis and policy, but at the same time may point to the need for further research.

7.2. The data used in this paper were derived largely from fire insurance valuations or some other estimate of replacement cost. An inquiry like the one described here could be undertaken from time to time, but need not be done frequently since the quantity of assets changes only slowly. Although with the experience already gained the technique of investigation could be improved, inherent difficulties make an inquiry into capital values more complex than the usual census-of-production type inquiries. One of these difficulties is that not all business firms can give a satisfactory answer to the questions. Perhaps business firms will improve the valuation of their assets, primarily for their own use, as many of the largest firms are already doing. However, in this field it is better to be satisfied with voluntary answers than risk the inclusion of compulsorily obtained but meaningless data. Another difficulty is that the data collected require interpretation by those who furnish them. Uniformity does not exist and cannot be expected to exist in this field and therefore it is not possible to employ routine techniques of analysis.

7.3. The data on fire insurance values could theoretically be obtained either from the insured or the insurer.* In fact they were obtained exclusively from the insured and this appears the preferable method in practice because the insured are in a better position to interpret the data or, if necessary, give alternative and more suitable estimates. Fire offices could not disclose information without the client's consent, although this might not apply to totals for particular industries. The technical difficulties of extracting information are also greater unless fire offices could be persuaded to reorganize their records so as to throw up useful summary information. In spite of these obstacles, I feel that an analysis of the records of fire offices would be a valuable supplement to data obtained from other sources. One might perhaps prefer to take only a sample of the records and look at the policies in more detail. Some further light might also be thrown on the question of under-insurance by a study of reports on actual fire damage.

7.4. Each of the different principles of valuing assets is suitable for its own purpose, and the replacement cost basis was adopted in this paper as it is significant for a number of important economic investigations.† But the figures collected on this basis, and in the particular circumstances described here, cannot be used to answer all questions which economists may raise. For instance, the data on assets per employee indicate average relationships and need not apply to equipment currently installed. Perhaps one might assume that with current techniques more assets per employee are required. But output is also higher and it is difficult to predict whether the new asset-output ratio will be higher or lower than the old. Again, there is no reason to assume that these data are applicable to other countries. In a less industrialized country it might pay to utilize assets more fully than in the United Kingdom but the extent to which this is possible will vary from industry to industry. Fuller utilization might, however, be offset by lack of adequate skill and asset-output ratios would not necessarily be lower.

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* Insurance offices make an annual return to the Board of Trade showing revenue and expenditure but not values insured.

† For the different principles of valuing assets, cf. Bronbright (1937).

7.5. Industrial capital is admittedly a vital element in economic progress, especially in an internationally competitive economy, but our knowledge of it from an overall quantitative viewpoint is still rudimentary. Compared with demographic statistics, statistics of capital are in their infancy. Since 1948 the government has assumed responsibility for collecting and publishing information on the birth of assets and since 1955 has even ventured into forecasting births by a sample survey of intentions. This paper presents the results of a sample inquiry into the population of assets. But information on mortality rates and on life tables of assets is still gravely lacking: the next step in this field must be one towards a study of the age distribution, the decline in efficiency with age and the retirement from useful life of assets in different industries.

7.6. The following is the summary of conclusions which emerge from this inquiry:

(1) It has proved possible to find a basis for valuing fixed assets in manufacturing industry which is economically meaningful, and this is replacement cost new. The concept of written-down replacement cost, which is also meaningful, has not been further explored here but the possibility of establishing measurements on that basis also exists.

(2) Almost all firms can supply information on the value of assets for purposes of fire insurance and on the nature of the valuation. These figures roughly correspond either to replacement cost new or to written-down replacement cost. Large firms, especially in heavy industry, may be able to supply estimates of replacement cost even if their fire insurance valuation is incomplete or unsatisfactory.

(3) On the basis of information obtained from manufacturing firms and relating to establishments employing some 1,100,000 persons, the replacement cost new of fixed assets in manufacturing, excluding vehicles, is estimated at £15,300 million for mid-1955. There is a very wide range of variation in assets per employee, from about £600 in china and earthenware to £13,000 in mineral oil refining. The average for manufacturing is £1,800 but one-tenth of the labour force is in industries with more than £3,000 per employee.

(4) For manufacturing as a whole the new estimates indicate an amount of assets approximately 50 per cent. larger than the comparable figure based on Redfern's estimates which make use of the perpetual inventory method. The most plausible explanations of the discrepancy are that assets last much longer than is implied in income-tax depreciation rates and that assets are restored, improved or extended by expenditure which is not recorded in statistics of capital formation.

(5) A fairly close relationship is established between assets per employee, including inventories, and value added per employee by industry. Value added can be estimated as equal to a basic wage in manufacturing plus 16 per cent. to 19 per cent. of the replacement cost of assets. There is not sufficient information to estimate the net rate of return on capital employed in particular industries but it is suggested that in the cement, tobacco, and drug and toilet goods industries the rate is likely to be above the average and in the textile trades below it.

(6) Relating post-war statistics of capital formation to the new data on the stock of assets, it is estimated that at mid-1955 27 per cent. of assets were less than 7½ years old, with variations from 16 per cent. to 36 per cent. by industry (except mineral oil refining which is almost wholly of post-war origin). The rate of capital formation reached a new record level in 1956 when gross capital formation came to 4¾ per cent. of the gross stock of fixed assets. With the exception of textiles and clothing, there was a net addition to assets probably in most industries and particularly in paper and printing, chemicals, metal making, and vehicles.

(7) It is suggested that the present inquiry be repeated from time to time, preferably on a voluntary basis. Its coverage and scope could be usefully extended, especially with the co-operation of industrial firms and insurance companies.

8. APPENDIX—MATHEMATICAL NOTE ON THE PERPETUAL INVENTORY METHOD

8.1. There appears to be a similarity between population mathematics and the mathematics required for the analysis of capital assets. Investments correspond to births and the stock of assets to population. Similarly to concepts used in population mathematics, the life of assets *of a given type* is determined by the probability distribution $f(x)$, for positive values of x, where

$$\int_0^\infty f(x)\,dx = 1$$

8.2. The concept of a "life table" of assets is important. $F(l)$ denotes the proportion of assets of a given type which survive age l, that is

$$F(l) = 1 - \int_0^l f(x)\,dx = \int_l^\infty f(l)\,dl$$

$F(x)$ is of course a monotonic decreasing function of x.

8.3. If we write $y(t)\,dt$ for investment as function of time, the stock of assets at point of time T is

$$G_T = \int_0^\infty y(T-x)\,.\,F(x)\,dx$$

8.4. With the perpetual inventory method the stock of assets at point of time T is calculated as

$$G_T' = \int_0^L y(T-x)\,dx$$

where L is the "normal" length of life of assets.

Hence

$$G_T' - G_T = \int_0^L y(T-x)\,\{1 - F(x)\}\,dx - \int_L^\infty y(T-x)\,F(x)\,dx$$

8.5. The nature of the difference between the calculated value G_T' and the true value G_T is made clearer in Fig. 1 which shows an illustrative life table function for assets of a given type. Up to a point $x = L$ the areas above the line are shaded and beyond this point the areas below the line. These shaded areas are in the nature of positive and negative weights which are to be applied to the investment function $y(t)$ in order to measure the difference. This difference is the product-moment of the weights illustrated and investment measured *backwards* from a given point of time.

8.6. It is easily seen that one cannot make a general statement about the direction of the difference $G' - G$. This depends on (*a*) the shape of the life function, (*b*) the location of the point $F(L)$ on the life function, and (*c*) the shape of the investment function. As a simplification it may be said that the difference $G' - G$ depends on (*a*) the relative sizes of the two shaded areas in Fig. 1 and (*b*) on the trend of the investment function, which can be taken as the linear trend. An increase in L increases the size of the first shaded area in relation to the second and tends to increase the difference $G' - G$. An increasing trend in investment through time also tends to increase this difference.

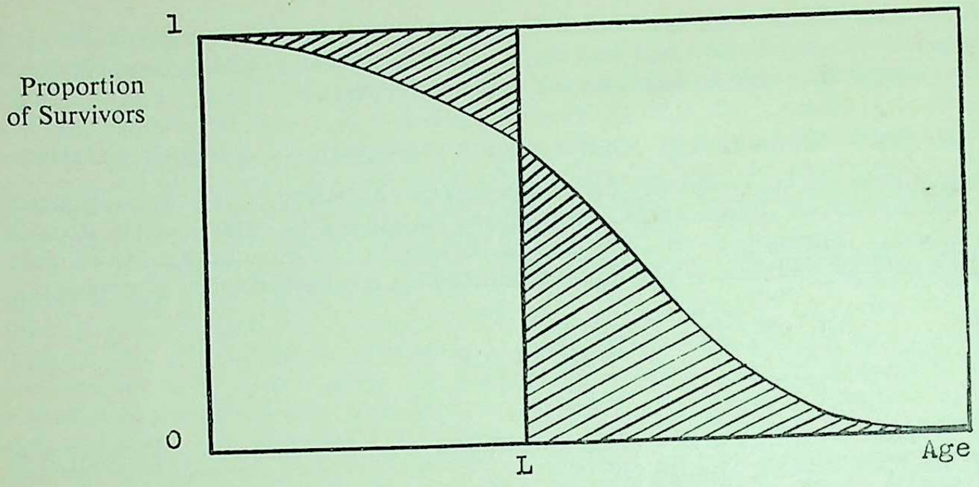

FIG. 1.—Illustrative life table of assets.

8.7. The question is what interpretation should be placed on L, the "normal" life of assets. It is presumably one of the averages derived from the function $f(l)$ and is likely to be the mode of the distribution, that is, the point of inflexion of the integral function shown in Fig. 1. Now, in so far as "normal" life is taken to be the mean life, the two shaded areas in Fig. 1 would be equal in size. In that case, $G' - G$ is likely to be positive with an increasing and negative with a decreasing trend in investment. In so far as L is equivalent to the modal value of life, $G' - G$ is likely to be positive with an increasing trend in investment *and* a negatively skewed life function (where the mode exceeds the average); or likely to be negative with a decreasing trend in investment *and* a positively skewed life function.

8.8. Unfortunately the life function of assets is not known, but from the little empirical evidence which we have it appears to be positively skewed though only moderately so. As investment has shown an increasing trend over the past half century, this tends to offset the effect of positive skewness on the difference $G' - G$. Hence no definite statement can be made about the likely direction of this difference in the actual circumstances. Experiments with illustrative figures indicate that the difference is likely to be of relatively small order of magnitude, say 5 to 10 per cent. of G.

8.9. This note does not deal with the point that the life table of assets itself may change through time.

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DISCUSSION ON DR. BARNA'S PAPER

Mr. P. REDFERN: As in his earlier paper to the Society, "The Interdependence of the British Economy" (this *Journal*, 1952, 29–77), Dr. Barna has broken fresh ground so far as the United Kingdom is concerned. By giving us the first direct estimates of the fixed capital employed in the principal manufacturing industries, he has made a notable contribution to our statistical knowledge in a hitherto neglected field of study.

I should like to make one general observation on the method of approach. The estimates which Dr. Barna has derived rest entirely on the asset valuations provided by the firms approached in the enquiry. Now valuation is a process which does not lead to a unique answer even if the basis of valuation—that is replacement cost new—is laid down in precise terms. An element of judgment must enter in, so that two different valuers may be expected to arrive at somewhat different answers no matter how expert they may be. In this respect valuation is an essentially different process from that of measuring the area of a piece of land or recording the wages and salaries paid by a given factory in the year 1955.

Furthermore, Dr. Barna has frankly set out the reasons why the valuations he has been able to collect have not always been of the quality he would have wished. For example, he says that in times of rising prices some firms do not adequately and promptly revise upwards the values of reinstatement policies. Consequently he has had to make corrections—presumably rather rough corrections—for this and other shortcomings. We must recognize therefore that, quite apart from the sampling errors, the results will differ by an unknown margin from the "true" answer—the "true" answer being defined in this context perhaps as the answer which would have been obtained by employing the most expert valuer and giving him very precise instructions on the basis of valuation.

Dr. Barna and the National Institute of Economic and Social Research are to be congratulated in having had the courage to go to industry with a questionnaire on a difficult subject and then to cross-question firms on the basis and shortcomings of the answers they have given. In the circumstances I feel that the response rates achieved are satisfactory.

Turning to the results, it would perhaps be appropriate to comment on the discrepancy between Dr. Barna's results and those set out in my paper to the Society in 1955 (this *Journal*, 1955, 141–182). In the first place, the estimates of capital stock given in my paper were related to the costs of production of the capital assets—that is, capital was measured by the quantity of factors of production embodied in it. Some of my critics argued that capital should instead have been measured by reference to its productive capacity—that is in terms of its output. My choice of the former concept was, to a large extent, dictated by the available data, but I argued in my reply to the discussion that the measurement of capital in terms of its cost of production was more interesting and useful. On the other hand, Dr. Barna's estimates seem to be related more to productive capacity, since he says that "the art of evaluating replacement cost in fact consists of finding that amount of modern assets which can be regarded as the equivalent of the old in the economic rather than in the engineering sense." This difference of concept between Dr. Barna's estimates and mine, although of some importance theoretically, is unlikely to explain the discrepancy between our respective figures. If anything it widens the gap between us.

I think Dr. Barna has analysed in some detail the possible sources of difference between his figures and mine, and I agree with his diagnosis that the most likely sources are errors in the lengths of life I assumed and deficiencies in the basic figures of fixed capital formation in past decades. In my paper, I was at pains to point out how heavily the estimates of capital stock depended on the assumptions about length of life of assets. Moreover, my researches showed that for road vehicles the statistics of numbers of vehicles on the road in 1952, coupled with statistics of the annual intake of vehicles to the home market in preceding years, implied a length of life substantially longer than the 10-year life implicit in the Inland Revenue depreciation rates. Subsequent researches have shown that the average life of ships is also several years longer than the Inland Revenue life.

A conclusion of my paper was that the estimates of gross capital stock given in it were of doubtful accuracy, and I was not therefore unduly dismayed to note the divergence between Dr. Barna's figures and mine. Indeed it was even a relief to note that Dr. Barna's answer, expressed in £s, was a number of eleven digits and that my answer was of eleven digits too.

My paper was, of course, mainly concerned to measure depreciation—or capital consumption as it is now called—rather than capital stock. And, as Dr. Barna has acknowledged, the errors in length of life have a much smaller influence on capital consumption estimates than on estimates of capital stock.

I should like to see Dr. Barna's work extended in two directions. Firstly, I would support his proposal for an inquiry into the age distribution and life tables of various classes of assets.

I would hope that this would lead to a closer reconciliation between the estimates of gross fixed capital derived by the perpetual inventory method and the direct estimates derived from fire insurance valuations. Such a reconciliation would, if it succeeded, give us added confidence in both methods, and I should then like to see Dr. Barna's methods applied outside manufacturing industry. It has always seemed to me that an unduly large share of statistical effort has been, and is still, devoted to manufacturing industry in relation to other industries and services. This I suggest is largely because statisticians find information is easier to come by in manufacturing and not because manufacturing is fundamentally more important. But the scarcity of data on other industries, for example, distribution, transport and service industries, should be a challenge to us, and I hope we shall in time get estimates of fixed capital in these industries by Dr. Barna's approach.

Dr. Barna's paper may be summed up in the author's own words as "an experiment in method which, in the event, was successful", and from which "enough information was obtained to establish economically significant estimates". The Society should be grateful to Dr. Barna and the National Institute for presenting to us the results of his researches, and I am therefore glad to propose the vote of thanks.

Professor SEWELL BRAY: I welcome this very much needed piece of research and congratulate Dr. Barna on the progress he has been able to make with it. Naturally I tend to look at the subject with an accountant's interest, and despite a background of theoretical economics I have never been happy about the practical application of a concept of capital which is formulated in terms of the discounted value of future income streams—de Paula encountered similar difficulties many years ago. I am glad to find that this hesitancy appears to be well-founded, since Dr. Barna regards the concept as so general that only in rare instances can it be used in practical problems. Indeed, he makes it plain that one important aim in measuring industrial capital is to evaluate the contribution it makes to production. Certainly to an accountant this is a principal aim, and it is therefore reassuring to learn from an applied economist that for this purpose a "replacement cost" concept of capital appears suitable but a "discounted future income stream" concept clearly is not.

A second point I wish to stress concerns what Dr. Barna describes as the most important contribution of this inquiry, namely, that in fact it is feasible to obtain information from business firms on the replacement cost of fixed assets. I think that in the long run this may have important consequences for the development of accounting practice. As Dr. Barna observes, "in general, replacement cost gives a more objective basis to the valuation of assets than any alternative method since almost all assets used in industry are replaceable". In the past there has been a tendency for accountants to say that there is no objectively dependable basis for measuring the replacement cost of fixed assets and therefore such a basis of valuation is suspect when taken on to accounts. I hope that Dr. Barna's discussion on the nature of replacement cost will bring forward the time when real assets can be taken on to accounts at written-down replacement cost, a term which Dr. Barna prefers to the more ambiguous "net" replacement cost. In this context I should add that in my view replacement cost new is just as much a primary concept for accounting theory as for statistical purposes. Dr. Barna explains the fundamental distinction between replacement cost new and written-down replacement cost when he says that "no doubt replacement cost new is less an economic and more an engineering concept than written-down replacement cost but it may be held to be a more relevant concept when examining the relationship of assets to current output, and certainly more relevant in forecasting requirements of incremental capital". As was mentioned in the report of a committee on accounting ratios, of which Dr. Barna was a member " . . . for purposes of comparison of operating efficiency it may be found more useful to include fixed assets in 'fixed capital employed in operations' at current replacement cost before deduction of provisions for depreciation. There is a difference of opinion concerning the deduction of depreciation. On the one hand it is asserted that to deduct accumulated provisions is to prejudge the declining efficiency of fixed assets which it is the purpose of the ratios to disclose and measure, and provisions for depreciation are largely a matter of policy, in which uniformity is not to be expected: their deduction from the investment in fixed assets inevitably leads to lack of comparability in inter-firm comparisons and is a source of some misconceptions in comparisons within individual firms. On the other hand it is felt by some that the logic of including fixed assets in operating capital employed at current cost as new is open to some doubt in those industries where an active second-hand market in used assets exists. The usefulness of approximate tests based on gross current costs where the fixed assets have long effective working lives can be seen. But the argument is perhaps not so convincing when the fixed assets have relatively short lives because of technological change". Again, in the present paper Dr. Barna clearly sums up the accounting issues when he says "it appears that an increasing number of firms is interested in possessing information on the replacement cost of assets quite apart from the need for such information for fire insurance. These figures are used for estimating depreciation in excess of normal balance sheet depreciation and for assessing the profitability of the business in relation to real capital employed. Only a few of the firms possessing this information did revalue assets in the balance sheet".

My third and last point is to draw attention to the great significance in this inquiry of relating value added to assets, and I have been especially glad to see the breakdown of total assets into fixed assets and stocks. This is a pioneering achievement which I shall hope to see perfected and I hope that in due time there will be enough information to estimate the net rate of return on capital employed in particular industries. I was particularly interested to learn that a fairly close relationship is established between assets per employee, including inventories, and value added per employee by industry. Once accountants grasp the economic concept of value added there is much in Table 5 to interest them. I can only hope that the present inquiry will be repeated and developed from time to time.

I am glad to have this opportunity of seconding the vote of thanks.

The vote of thanks was put to the meeting and carried unanimously.

Sir ROBERT SHONE: I should like to congratulate Dr. Barna on a most interesting and stimulating paper and in the short time available to comment on three points. First, in the steel industry and the coke-oven industry (two of the classes given in the paper) we have been making independent estimates of the replacement cost new of the capacity of the industry. This is a very important question for price policy purposes. We have taken the view in the Iron and Steel Board that the crucial test for prices must be that they give a reasonable return on capital expenditure at a new plant operating with the full efficiency possible at such a plant. In connection with this determination we have had to make close estimates of replacement capital cost on the basis of new modern plant, and also of the operating cost possible at such plant.

We have used the figures of actual expenditures currently being undertaken or recently undertaken at steel works in various parts of the country, and where there is a gap of a year or so we have made appropriate adjustments to bring the figures up to current price levels. This gives a total figure for replacement costs new, and I find that our figures closely confirm those of Dr. Barna's for the steel industry.

For coke ovens his figure is £178 million and ours £188 million for the same capacity.

Secondly, I was very interested to see from Dr. Barna's equation the following result obtained by the difference between equations 11 and 12:—

$$\text{Wages per employee } (w) = £385 + 0\cdot03 \times \text{capital per employee } (k)$$

This would, I presume, be his guess at the theoretical relation underlying his equations (7) and (8′). In other words, the wage and salary level in an industry equals a basic wage plus 3 per cent. (or thereabouts) of the capital per employee in that industry. Thus, in a high-capital industry Dr. Barna's data show that we do on the whole find high wages. The reasons, no doubt, are those mentioned in his paragraph 5.15, but I am rather bothered about the basis of his equations. The wage element and numbers employed relate to the average of a whole range of existing plants. The capital, however, relates to that at a hypothetical new plant which, if it were really operating, might use an amount of labour very different from that actually reported.

Thirdly, Dr. Barna talks about depreciation and the possibility of measuring the decline in efficiency of plants with age. I do not really think it is right to think of depreciations occurring primarily because efficiency falls. The efficiency of a plant may well remain substantially constant throughout its life, or indeed increase if the managers find better ways of operating it. However, new and more modern plants come along which are relatively more efficient. With new inventions and new plants real wages in the economy generally go up, so an existing plant as it gets older is faced with progressively increasing real wage costs until these squeeze away the margin for depreciation and profit. The history of depreciation thus depends mainly not on a decline in efficiency within plants, but on technical progress which brings in competing new plants of successively higher efficiency. I am glad that Dr. Barna is going to carry further the consideration of depreciation.

Mr. G. PAINE: I should like to make one or two comments about the implication of Dr. Barna's estimates upon the official estimates of capital consumption.

In Table 4 are set out two estimates of the gross replacement value of fixed assets (excluding vehicles and land) in the manufacturing industry of the United Kingdom at the middle of 1955.

The first is derived from the simple inquiry into fire insurance value, the second from Mr. Redfern's estimates using the perpetual inventory method. I have made a later estimate by the perpetual inventory method using the more up-to-date figures and reach almost exactly the same figure as Redfern's. Whatever Mr. Redfern may say, the size of the discrepancy is startling, at any rate to the uninitiated. When I first saw this comparison I was not so much surprised as dismayed by its size. The more so because the gap would be even bigger if Redfern had been trying to measure what Dr. Barna calls the economic, as opposed to the engineering, concept. Now the official estimates of capital consumption that are published in the Blue Book are based on the perpetual inventory method developed and described by Redfern. So Dr. Barna's findings seem at first sight to discredit the accuracy and usefulness of the official estimates. A little reflection shows there is not so much cause for dismay as appears at first sight. This does not mean that I have been able to reconcile the two estimates of gross replacement value.

Dr. Barna has described the ways in which he overcame difficulties of under-insurance. He has specifically ignored the possibility of over-insurance. It is quite possible that over-insurance does occur in spite of the economic disincentive; if so, this would lead to some over-estimation of the replacement value of the stock of assets.

I have no new points to add to what Dr. Barna himself has said in paragraphs 4.17 and 4.18, I would only lay a little emphasis on some of his remarks. In the first place Redfern was trying to estimate capital consumption and not the value of the stock of capital. Using the perpetual inventory method errors in the assumed lengths of lives lead to much larger errors in estimates of gross and net value of the stock of capital than in the estimates of net capital formation or capital consumption. There is nothing new in this. Redfern gives a illustration of it in his paper (pp. 146–147). Barna acknowledges it in paragraph 1.10.

As Dr. Barna said, there is little doubt that many of the assumed lives are too short and, as he implies in the last sentence of paragraph 4.18, one reason why this is so is that a good deal of expenditure on repairs and maintenance (which is not part of gross fixed capital formation) does prolong the lives of the assets. It thus increases the number of assets in use at any one time and hence increases the value of the total stock of assets above the corresponding estimates by the perpetual inventory method.

I do not agree with Dr. Barna that such expenditure should be included in gross capital formation, nor that in any particular case it necessarily increases the gross replacement value of the asset in question; it may simply increase the length of its life. However, such expenditure does make a positive contribution to net capital formation and so perhaps a better way of treating it would be not as fixed capital formation as Dr. Barna implies at the end of paragraph 4.18, nor simply as current expenditure as at present, but simultaneously as current expenditure and as negative capital consumption. Information is not at present available to be able to do this. In the existing official statistics such expenditure is treated as an expense on revenue account and therefore it should not at the same time make any contribution to capital consumption. So while this does lead to an underestimate of the value of the stock of fixed assets by the perpetual inventory method, any error in the estimate of capital consumption cancels out over a period.

I agree that there is some other expenditure charged against revenue that could more properly be included as fixed capital expenditure. This includes all expenditure on extensions and improvements to existing assets that masquerades as repairs and maintenance, and expenditure on small tools, both of which are charged against revenue. The exclusion of these from fixed capital formation would lead in the perpetual inventory method to a further under-estimation of the gross fixed capital stock. But, once again, this expenditure has already been charged against revenue and consequently it would be inconsistent to let it also contribute to capital consumption.

For these reasons I do not believe that the official estimates of capital consumption are discredited by Dr. Barna's findings. On the other hand we cannot be content until we have found a reconciliation between these two very different estimates of the gross capital stock.

Mr. LYDALL: I should like to support what has been said in favour of Dr. Barna's work. However, I regard it more as a useful experiment and the proof of a valuable technique than as providing us with results that we should use widely or without the greatest caution. One cannot help being struck by the fact that all this enormous structure depends on the answers from 90 companies. As they cover 38 industries we can presume there are not more than perhaps four or five companies in any given industry. I think it would have been useful if Dr. Barna had given us the figures of the actual number of companies within each of the 38 industries on which he based his analysis.

Moreover, these companies are by definition those at the top of the scale within each industry. Dr. Barna said he looked at the results and found very little variation in the ratios of assets per man. But since he is looking at only the top slice represented by three or four companies it is difficult to judge the degree of variation throughout the scale. I would say, therefore, that one should be extremely cautious in using these estimates for the whole industries, and that it is not surprising that there should be discrepancies between these results and those produced by Redfern.

Although there is no proof that larger companies have higher assets per man employed, it is a very generally held view that this is so; and certainly in many industries value added per man increases very substantially at the top end of the scale.

It may be said that this is all answered by what Sir Robert Shone said about the steel industry, where there is a most remarkable coincidence between his estimate and Dr. Barna's. If you look at Barna's figures, however, you will see that his estimates for iron, steel, and non-ferrous metals are based on six companies. How many are non-ferrous I do not know, but presumably one or two, and that leaves us with four or five steel companies covering 33 per cent. of employment in this industry. It is possible that, as a result of Sir Robert Shone's efforts, the steel companies now have a standard method of estimating the replacement cost of assets per man, and hence the companies in Dr. Barna's sample simply gave him the standard answer.

The second point on which I wish to comment is the use of regression analysis in this paper. I am very disturbed by the methods used. To start with, there is no discussion as to whether one would expect the scatter diagram to be randomly distributed round a straight line, and there is no reason to think that most of the variation between industries is due to random errors. On the contrary, the differences between industries are likely to be due to specific industry effects, as Barna himself says at one point in his paper.

I would suggest that one can interpret this distribution as being a set of points relating to quite different industries which happen to have been drawn on one piece of paper. There are three major sources of difference between industries in value added per man. The first is different wage rates, the second differences in the amount of advertising and the other outside costs which are included in value added, and the third differences in profit rate on capital. This profit rate will depend on the degree of monopoly, the extent to which there are barriers to new entry into the industry, and so on.

I think it unlikely that any industries will fall below a certain level. With full employment they will all be earning enough profit to provide a "normal" return on capital invested. That is the minimum and all the rest will be above it. I think, therefore, that any of the conclusions that Dr. Barna has drawn about the number of £s of capital required to produce an addition of £1 to the national output can be very misleading. Moreover, there is a danger that this can be confused with the normal capital coefficient, which is usually defined as the amount of capital required to add £1 to the national output, assuming that the other factors are available in the quantities required by the given state of technique. It would be more realistic to infer from Dr. Barna's results that the marginal efficiency of capital, as he defines it, is of the order of one-tenth than to accept his figure of one-fifth.

Mr. J. L. NICHOLSON: I should like to make a few comments on the techniques employed in section 5, which is concerned with the relation between value added, or net output, and the level of assets.

There is first the question of whether the per head figures for each of the 36 industry groups included in the regression analysis should be weighted or not. Dr. Barna expresses a preference for the unweighted figures on the grounds that they are nearer to a normal distribution. A small point is that the figures for each industry group, if calculated in the ordinary way, would automatically reflect the relative importance of different industries and trades within the group. Unless per head averages were calculated for each firm, the figures would be weighted within each of the 36 industry groups, but not between these groups; which seems slightly inconsistent.

One surely ought to decide whether to use weighted or unweighted figures without looking to see which set of figures is closer to the normal distribution. Otherwise, since the grouping of industries is arbitrary, they could be regrouped in a large number of different ways and one could select the groups which produced (unweighted) figures nearest to the normal distribution. This would allow too much rope to the statistician. As a matter of economics, the larger industries are more important than the smaller ones and so it is the weighted figures in which we are primarily interested. The fact that the unweighted figures are nearer to the normal distribution is not relevant. Oddly enough, when Dr. Barna comes to interpret his regression lines, he adopts the Bowley-Allen approach, which is independent of the assumption of a normal distribution.

Dr. Barna finds a linear relation between v (value added) and k (total assets), of the form $v = a + bk$. To explain this relation, he sets up the hypothesis that the price of labour and the rate of return on capital are the same in all industries. It is not clear why he chooses this hypothesis, since one would expect industries with more capital to employ more skilled labour, which is paid higher wages; and we know that wages vary a good deal from one industry to another.

The regression analysis shows, in fact, a strong correlation between w (wages) and k (assets). One is then faced with the choice between finding some other explanation for this correlation and abandoning the hypothesis; Dr. Barna does not seem to make the choice, but apparently retains the hypothesis since he considers the possibility of estimating a from the equation connecting w and k alone, and b from the equation between p and k alone. He considers these equations as alternatives to the equations connecting v directly with k.

Value added (v) is subdivided between wages (w) and profits, interest and rent (p); $w + p \equiv v$ identically. If w, p and v are each expressed as the same type of function of k, e.g.

$$w = a_1 + b_1 k, \quad p = a_2 + b_2 k, \quad v = a + bk,$$

and if the regression equations are obtained by minimizing the sums of squares of deviations in w, p and v, it can be shown* that the regression co-efficients in the equations for w and p will add up to the corresponding regression co-efficients in the equation for v ($a_1 + a_2 = a$; $b_1 + b_2 = b$). But the regression equations obtained by minimizing the sums of squares of deviations in k will not have the same "adding-up" property. Barna's equations (7) + (9) = (2), but equations (8′) + (10′) ≠ (3′). Thus (7) and (9) are merely subdivisions of (2) and do not lead to different estimates of a and b. If equations (8′) and (10′) are summed, we get

$$v = -44 + 0{\cdot}387\, k,$$

which seems to be the only alternative (among those considered) to equations (2) – (3). If, however, it is the relation between v and k in which we are interested, there seems no real justification for splitting v into two parts, w and p, finding the regression equation between each part and k and then adding the two together.

Passing to the interpretation of the two elementary regression equations, the author refers to a very useful paper by Professor Allen, building on the work of Sir Arthur Bowley. He quotes Allen as saying that if there is moderate positive correlation between the "errors" of two variables which are positively correlated, the true relation lies within the acute angle formed by the two elementary regression lines.

In fact, Allen's conclusions are rather different. He shows that, on certain assumptions, only if the correlation between the errors is zero, or of opposite sign to the correlation between the two variables, can we definitely say that the true relation lies within the acute angle of the two elementary regression lines. He also shows that, on the same assumptions, if the errors have a substantial correlation of the same sign as the correlation between the two variables, the true relation is likely to lie *outside* the acute angle formed by the two elementary regression lines. If the errors are positively correlated, which Barna says is probably the case, we are very much in the dark about the true relation between v and k.

Despite these criticisms of one section of the paper, I would like to congratulate the author on successfully carrying out an ambitious project.

Mr. BOUND: I should like to follow the speaker who said that a very ambitious structure has been raised on a very small sample. I have been looking at the response rates. Dr. Barna said these were very good considering the difficulties, but I wonder whether they were good enough. They are all around 50 per cent. In my own work, dealing with the results of sample inquiries, I would say any sample inquiry results based on a rate as low as that must be regarded with a certain amount of doubt. This response rate does weaken the very important arguments that have been raised.

I wonder whether, if Dr. Barna is considering repeating this inquiry, he might perhaps improve the field methods. I was rather struck by the points made in paragraph 3.15 on companies not giving information. The original letter was followed up by a second, but I wonder whether an answer might not have been obtained by more active field work. We see that five companies promised information but it was not received. Might a more energetic follow-up have produced such information?

It is necessary that the actual field work should be regarded as important because, as a previous speaker pointed out, one has to contend with a small sample to start off with, and if one is going to throw further doubt on the sample by suggesting the responses might be from only a biased part of the population one is tending to discredit what is undoubtedly a very valuable exercise.

Mr. CARY: I appreciate Dr. Barna's difficult task in the field work. Dr. Barna asked us a question and I gave him an answer. He promptly replied that that did not agree with part of the data and I had to make another estimate. In my company we have a very fine works but we also have half a dozen others. How can we get a replacement value for them? One can have only a rule-of-thumb method of valuing. I eventually gave Dr. Barna another figure which I considered was the replacement value. If this inquiry went further I think other companies would be more interested and would make a real attack on that question.

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* See J. L. Nicholson, "The General Form of the Adding-up Criterion" (page 84 of this *Journal*).

Mr. CLARK: If it is really true that it takes five units of capital to produce one additional unit per annum of gross product this is the worst news we have heard for a long time. Indeed, if our capital requirements are as high as this, we should most of us do better making our living out of agriculture, rather than trying to develop industry further. In paragraph 5.3 Dr. Barna sets out to relate true income produced to "value added". He makes a deduction of 10 per cent., but the analogous figures in the Blue Book appear to work out at something more like 16 per cent. He puts depreciation at 5 per cent., whereas I should make it over 7 per cent. The result is that Dr. Barna's table very seriously over-states gross profits and, furthermore, it differentially over-states them as between industries.

Mr. C. T. SAUNDERS: May I make one suggestion? I do hope that now or later Dr. Barna will explain what the marginal capital coefficient derived from cross-section data in paragraph 5.21 really means, and whether it has any relevance whatever to capital coefficients based on time series.

The following contribution was received in writing after the meeting:

Mr. H. WARD: Dr. Barna is to be congratulated on a very real contribution and his excellent grasp of the insurance problem.

Balance sheet values of plant are almost meaningless for statistical purposes of this kind. Insurance values are more real. He had the problem of forming a view, without specific inquiry, of the value of foundations and how far they are or are not insured. An average figure of 10 per cent. might be modified in any future inquiry. Foundations may often cost 25 per cent. in those industries which use substantial buildings.

In a mature industrial country, such as the United Kingdom, thousands of allegedly ancient machines, perhaps forty years old, may still be as useful as or even more efficient than a modern machine. Perhaps the only ancient part is the casting. Rolls, gears and ancillary equipment may have changed many times. There is no wear on the casting and if it is in use another fifty years it is no reflection on efficiency and no guide as to new capital required year by year for plant. The machine may still carry the latest electronic gear.

The speed of development makes average figures of capital per person very wide of the mark. Fifty per cent. of all workers are commonly engaged on various services, internal transport, lifts, stores, maintenance, window cleaning, painting, etc., and this must vary with locality and company policy. Even a large factory may in effect be a jobbing factory with complicated problems of preparing drawings, invoicing, planning, progressing. Ancillary workers must be tailored to the exact work undertaken by the company. Thus one would expect a wide range of variation of capital per employee in companies of equal efficiency, depending on the nature of the business even within one industry.

Dr. BARNA: I will reply to the detailed points in writing. There is, however, one important issue which ought to be settled now. I think Mr. Redfern and I agree on the possible factors which may explain the difference between our estimates, but we cannot attribute quantities to these factors. It is true that if all the difference is due to errors in the length of life assumed by him, then the capital consumption figures in the blue book are subject only to a small error. (It is not necessarily also true that the relative error is small in the net capital formation figures since the latter can be around zero.) If, on the other hand, there is error in the nineteenth century capital formation figures, which enter the calculation, then of course there would not be any compensating factors to save the estimates of capital consumption. To state the position precisely, I do not think that we can either approve or disapprove the blue book capital consumption figures at this stage: further research is required in the direction which I indicated before anything more accurate can be said on the subject.

On the defects of my method, I am aware that (*a*) the sample was not fully representative, and (*b*) the response rate was not high.

On the first point, raised by Mr. Lydall, I am satisfied that the sample was good in most industries where capital matters. In those industries there are no small firms. Incidentally, I did not take the top slice of companies: the public company sector from which I sampled includes probably 60 per cent. of manufacturing activity, and the size distributions of public and private companies very much overlap. I mentioned the industries for which my sample is weak, and I attached margins of error to the estimates to take care of this.

The response rate is admittedly not high but my problem is not the same as in inquiries into savings where one may suspect that people who do not answer save more than people who answer. We have some knowledge on companies other than that obtained from the sample and those companies which did not answer show no clear differences from those which answered.

Dr. BARNA subsequently replied in writing as follows:

I fully agree with Mr. Redfern that the valuation of capital assets is not a process which yields a unique answer, even if different valuers follow the same procedures. I also agree with him that we are far from possessing good valuations of all assets. I am glad that he and other speakers approved of my method of approach, in spite of its imperfections, and I am particularly encouraged by the practical applications of this approach indicated by Mr. Sewell Bray, Sir Robert Shone and Mr. Cary. I am sure that greater interest by British industry in the realistic valuation of assets will also improve the response rate to similar inquiries in the future.

Since reading the paper, I found that government-financed capital formation in commercial factories during the war is omitted from Mr. Redfern's estimate of the stock of assets. The large public expenditure under this heading is given by M. M. Postan (*British War Production*, p. 448) but it is difficult to say how far it represents assets which continued to be useful after the war. By taking account of the subsidy element in private capital formation, the difference between Mr. Redfern's estimate and my own, which is shown in Table 4, might be reduced by up to one-fifth.

Sir Robert Shone raises a very real issue when he points out that the replacement cost new of existing assets is hypothetical—it is not the cost of assets which would in fact be installed if the need arose, nor are the existing assets in fact new. I also agree with his comments on the nature of depreciation but we do not yet know whether similar considerations apply to other industries besides steel.

Mr. Paine is perhaps unduly concerned with over-insurance. On balance, under-insurance is a far greater worry and in the interpretation of the data which I obtained I tended to err on the conservative side. The following table gives further information on the "editing" of the returns:

Basis of valuation	*Number of companies*	*Value returned by company* (£*mn.*)	*Final estimate* (£*mn.*)	*Difference* (£*mn.*)
Replacement cost new or equivalent . .	68	1,815	1,851	36
Written-down replacement cost or equivalent .	8	233	331	98
Partly new, partly written-down replacement cost	14	407	521	114
Total	90	2,455	2,703	248

In the first category there is a slight element of written-down values. Subsequent to sending the return, some companies reported minor upward revisions in valuation, the value of omitted foundations, uninsured assets and under-insurance; but most of the additions are on account of uninsured foundations for buildings. In the second category, more than one-half of the difference due to "editing" was confirmed by two companies, and in the third category two-thirds of the difference by six companies.

Overestimation is possible only through a bias in the sample and I have so far not found evidence of this. The sample is weak only in a few industries but the margins of error in Table 3 are intended to take care of all kinds of error. As I stated, it would not be meaningful to give the number of companies or of returns on which individual estimates are based as the scope of the individual returns varies greatly and also because some industries are more homogeneous than others.

Mr. Lydall states as a "very generally held view" that the larger companies have more assets per employee than the smaller ones. Mr. Lydall in part confuses companies with establishments. My sample did contain companies of fairly small size, and, of course, even the largest companies sometimes operate small establishments. As far as I am aware, the view (which is not generally held among experts) that capital-intensity is associated with size is based on United States data derived from balance-sheets filed with the income-tax authorities. I am not willing to accept balance-sheet values for this purpose, mainly for the reasons stated in my paper and also because

I believe the defects in the data to be greater in small firms. But Mr. Lydall's statement is also refuted on purely technical grounds in a recent article by H. S. Davis (*The Review of Economics and Statistics*, 1956, p. 286). My own more detailed research on the furniture industry does not accord with Mr. Lydall's thesis and I hope that on another occasion he will disclose the relevant findings of his large-scale inquiry at Oxford into small firms.

The steel industry is very far from practising a "standard method of estimating the replacement cost of assets", as Mr. Lydall believes. I had four steel companies in my sample: for one my estimate is based on capital formation as shown in the balance-sheets, for one on the distribution of the original cost of assets by age and the company's own index of construction costs, for one on a similar method using *The Economist* index, and for one (which exceptionally insures almost all assets) on fire insurance values which were, at my request, adjusted from a written-down basis to replacement cost new. (A fifth company insures only a proportion of its assets and does not know the replacement cost of uninsured assets.) Assets per head differ greatly between these companies but the law of averages must have come to my aid.

The analysis in section 5 is only a by-product of the main inquiry and, as I said, it is experimental and provisional as regards both data and methods. The rest of my reply is in answer to comments on that section.

I gave more reasons for using unweighted data in the regression analysis than the one stated by Mr. Nicholson. I do not agree with him that weighting is "a matter of economics" since weights also ought to reflect the relative error in the estimates. However, I recalculated, as an example, equation (2), using employment as weights. From this

$$v = 490 + 0 \cdot 136\, k,$$

with $r = 0 \cdot 734$.

Mr. Nicholson also criticizes the assumption underlying my equation (4) but in fact equations (5) to (10) are explained by an alternative assumption which must have escaped his attention. It would appear that Mr. Nicholson does not see that equations (2), (7) and (9) are interdependent in terms of theoretical variables but that interdependence is disturbed when empirical variables are taken. On the other hand, he does not see the arithmetical connection between (3), (8) and (10) and the new equation which he derives is not based on the least squares assumption.

Paragraph 5.16 is too condensed and I failed to make a clear distinction between the conclusions of Professor Allen and my application of his conclusions to the specific case under discussion. Professor Allen shows that the position of the true regression line, in relation to the elementary lines, is determined by the configuration of deviations from true values in the variables. In his equations (5) and (6) the slope of the true regression line is given in terms of the two intensities of the deviations (that is, the ratios of the error term to the standard deviation of the variable), the correlation between the deviations in the two variables, as well as the observed correlation between the variables. He then proceeds to show that if the correlation between the deviations in the two variables is zero, or unity with a sign opposite to that of the correlation between the variables, the true line is always between the two elementary lines. In all instances, however, a solution is possible if numerical values are inserted in Professor Allen's equations.

In the instance which is the subject of section 5 of my paper, there is no reason to assume correlation between the deviations in the two variables; if anything, there may be a small positive correlation between them in so far as the valuation of assets is influenced, for psychological reasons, by the profitability of the firm. It can be also assumed, as argued in paragraph 5.17, that the intensity of deviations is much larger in one variable than in the other but that nevertheless the other is also subject to some error. In these circumstances the true regression line lies between the two elementary lines and nearer to the one where the explanatory variable has the smaller intensity of deviations. The results would be uncertain (which is Mr. Nicholson's contention) only if the two intensities of deviations were of the same order of magnitude.

I do not share Mr. Lydall's anxiety about my regression analysis. What does it mean that "the differences between industries are likely to be due to specific industry effects"? I do not see why the comparison of outputs between more and less capital intensive industries raises different problems from a comparison of, say, savings between rich and poor. It is possible that what Mr. Lydall means is that differences in the degree of monopoly introduce a bias in the regression. But this is true only if the degree of monopoly is correlated with capital intensity and presumably Mr. Lydall had in mind the hypothesis of some economic theorists to this effect. In the circumstances of British industry in 1954, however, no evidence can be found to support that hypothesis and, on the whole, industries suspected of monopolistic tendencies did not make exceptional profits, nor did the rate of profit rise with capital-intensity.

Mr. Clark does not say how he calculates the various deductions which should be made from value added. My estimate for total value added in 1954 is £6,280 million and the corresponding blue book figure is £5,884 million. The difference is 6 per cent. but I mentioned 10 per cent. in paragraph 5.3 because the coverage of manufacturing in the blue book is somewhat wider than in the census of production. Depreciation of manufacturing assets, excluding vehicles, can be put at £300 million on the basis of Redfern's estimates.

Mr. Reddaway suggested, in a private communication, that I could partially correct the Board of Trade value added estimates by extrapolating the relevant items specially collected in the 1948 census of production. The resulting calculation shows that of the two assumptions discussed in paragraph 5.14, I should have adopted the second instead of the first. With the adjusted value added figures both coefficients of equation (2) would be 10 per cent. lower. Surprisingly, the correlation coefficient does not change with these adjustments.

Mr. Clark is thus right in so far as he had in mind that deductions from value added for "outside payments" vary with capital intensity. (A similar variation of depreciation is already implicitly allowed in the procedure of paragraph 5.19). But a correction on this account, or the adoption of Mr. Clark's other suggestions, would lower the rate of return on capital and increase the capital coefficient even beyond the figure which Mr. Clark already finds excessive.

An attempt to explain the difference between my marginal capital coefficient and those derived from time series—to answer Mr. Saunders's question—is made in an unpublished paper referred to in 5.21. Apart from differences arising from the differing nature of empirical data, in my analysis, based on a cross-section of industry, the labour force and techniques of production are given. (A line similar to my regression line is discussed by M. Bronfenbrenner in *Econometrica*, 1944, p. 35.) But the labour force, the structure of industry and capacity utilization may change through time, and techniques of production always change. While the accumulation of capital tends to yield diminishing returns, this tendency is offset by the effect of inventions which increase the efficiency of a given amount of resources embodied in capital assets. On balance the capital-output ratio may change in either direction and thus the marginal coefficient can be higher or lower than the average coefficient. In static conditions the marginal coefficient is always higher than the average. The simplest explanation is that my marginal coefficient is just a new-fashioned name for the inverse of the rate of return on capital.

I estimated the net rate of return on capital employed in manufacturing, very tentatively, as of the order of 20 per cent. per annum. Mr. Lydall believes that the rate must be of the order of 10 per cent. On the other hand, Mr. Clark thinks that if the rate were as low as 20 per cent. we ought to be in agriculture as, presumably, farmers are earning much more than 20 per cent. on capital employed. I disagree with both.

As a result of the ballot taken during the meeting the candidates named below were elected Fellows of the Society:

Tangirala Venkata Avadhani
Edward Maurice Backett
Kali Shankar Banerjee
Harold Edward Barry
Grace E. Bates
Wilfred Bell
Norman Charles Berry
Bernard Cyril Bishop
Philip William Peter Browne
Tony Reginald Burbridge
Roshan Lal Chadda
Michael Leonard Chambers
Norman Spencer Christian
Howard Collings
Richard Melville Cormack
Colin John Cornwall
John Henderson Coultate
Douglas Critchley
Peter John Cropper
John Jeffrey Dodds
Derlwyn Howard Edwards
Herbert John Evans
Iorwerth Gwyn Evans
Cecil Geoffrey Fenn
John Richard Fowling
Jonathan Herbert Frimpong-Ansah
Alan Gardner
Anila Graham
Jacob Jack Graneek
William George Gray
Francis Eugene Guaschi
Olga Gunewardena
Robert Charles Hazell
Dennis Hemingway
Joan Mary Hewson
Walter Ernest Hindmarsh Hickox
Colin Barclay Hill
Kenneth Richard Hutchings
Peter Kay
Ronald Smart Keir
Kenneth Walter Kemp
Sidney Kessler
Hanus Kraus
Rex Anthony Leaver
Jean Lepere
John Hyde Linnell
Cornelius Mack
Zbigniew Konstanty Mackowiechi
Charles Alexander Marr
Henry William Messenger

Stuart Love Morrison
Rosemary Janet Morton
Richard Geoffrey Newbery
Francis Richard Oliver
Wyndham Somerville Paige
Robert Ernest Peake
Kenneth Eifion Penny
Colin Albert Perry
Antoni Konrad Pokora
Ludwig Nahman Posener
Narahari Umanath Prabhu
Mansa Ram Puri
A. Gopal Rao
Basil Robarts
Lothar Sauer
Jack Arshavir Shaljean
Prasad Sheo Shankar
Ronold Ernest Shadrack
Martin Simmons
Diana Margaret Smith
Victor Frank Soul
David John Spurrell
Bevan B. Stein
T. K. Sundaresan
Evelyn Margaret Symon
Gim Hock Teoh
John Ethelbert Thomas
John William Ivor Thomas
Eric John Thompson
Glyn Thompson
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MEASLES PERIODICITY AND COMMUNITY SIZE

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1. *Retrospect*

A CRITIQUE of the statistical theory of epidemics arising from the transmission of infection from person to person, with particular reference to the theory of recurrent epidemics, was attempted in a recent paper presented at Berkeley in 1955 (Bartlett, 1956). Explicit consideration was given to measles epidemics, on which a good deal of previous investigation, both theoretical and observational, has been made (see References). It was suggested in my paper that the simple assumptions made in the Hamer-Soper model (see Soper, 1929) of measles, while leading to gross discrepancies with observed statistics, might be rehabilitated if formulated in proper stochastic (i.e. probabilistic) terms. It should be explained that statistics in a fairly large-scale sense, as obtained, for example, from the Registrar-General's reports, are being referred to. The detailed study of infection is also important in elucidating the values of constants occurring in theoretical models, but this problem, which has been studied by R. E. Hope Simpson (1948, 1952), N. T. J. Bailey (1955, 1956) and others, is not being considered here.

The two chief theoretical points may be recapitulated as follows:

(i) The deterministic form of the Hamer-Soper model leads to damping of the epidemic oscillations (this damping is reduced but not eliminated by a shorter period of infectivity), in contrast with the quasi-stationary character of observed epidemics (cf. Wilson and Worcester (1945*a*)). On the other hand, the stochastic formulation leads to a stationary time-series with a pattern of oscillations depending on the community size. The theoretical size of these oscillations depends in a complicated way on the "fade-out" effect referred to in (ii) below, especially when the extension of the community in space is included in the model.

(ii) The recurrence of epidemics is made possible by the continual influx of susceptibles (in the case of measles, the continual growing-up of children into the critical age-period), but in the absence of "carriers" may be prevented by random extinction or "fade-out" of the infection. This is the more likely the smaller the community. This seems well-known to field epidemiologists, at least in the case of measles epidemics for small village or island communities, but a more quantitative observational study, linked to predicted theoretical relationships, seems desirable, and is one of the main objects of this paper.

It is perhaps some justification of the present approach that, in spite of the inevitable idealization of the mathematical model, preventing any exact quantitative comparison, two important characteristics of the pattern of measles epidemics were surmised theoretically and then checked observationally. The first was a statistical relation between the mean period between epidemics and the size of the community, the second was the existence of a critical community size above which "fade-out" of infection was unlikely. In order to assist the theoretical discussion of the latter phenomenon in the absence of detailed mathematical solutions, artificial realizations of epidemic series for fair-sized communities have been generated with the aid of the Manchester electronic computer, and (while this part of the investigation is still incomplete) may be compared with actual epidemic statistics.

2. *Observational Material*

It has been common in previous studies of the periodicity of measles epidemics to quote the statistics for large urban areas, London, Glasgow or Manchester, and the biennial measles periodicity often observed (in times of population stability) thus merely refers to such large units. It has been noted, for example, by Hope Simpson (in the discussion following Bailey (1955)) and by Bartlett (1956), that much longer periods are observed for smaller communities. A typical small town epidemic series, for Ffestiniog in Wales, was shown in my 1956 paper. In order to study the dependence of the epidemic pattern on town size more systematically, twenty towns of varying sizes were chosen from an A.A. handbook. The important requirement for the smaller towns was reasonable isolation, for obviously a satellite district near a larger town would merely reflect the characteristics of the larger community.

TABLE 1

Nineteen Towns in England and Wales Ranked According to Population (Average from 1940)*

Town	*Population (Thousands)*
(1) Birmingham	1,046·0
(2) Manchester	658·0
(3) Bristol	415·2
(4) Hull	269·4
(5) Plymouth	180·0
(6) Norwich	113·2
(7) Barrow-in-Furness	66·4
(8) Carlisle	65·0
(9) Bridgwater	21·8
(10) Newbury	18·1
(11) Carmarthen	11·7
(12) Penrith	10·5
(13) Ffestiniog	7·14
(14) Brecon	5·62
(15) Okehampton	4·02
(16) Cardigan	3·51
(17) South Molton	3·07
(18) Llanrwst	2·59
(19) Appleby	1·74

* London was originally also included, but was dropped owing to the inconvenience of no over-all figures for London (except for the Administrative County) being quoted in the Registrar-General's weekly return.

The weekly measles notifications for the towns in Table 1 were then extracted from the Registrar-General's weekly returns of births, deaths and infectious diseases in England and Wales. It is hardly feasible to reproduce such statistics, or graphs of them, here* in full, but their chief characteristics will be summarized. The gradual change in the epidemic pattern from the continuous epidemic peaks and troughs in the case of the large towns like Birmingham and Manchester to the more spasmodic and of course smaller epidemics in the smaller towns was quite apparent. Two of the smallest towns considered, Cardigan and Llanrwst, were especially remarkable in experiencing only two epidemics each over the entire period for which statistics were available, 1940 to the beginning of 1956. A further feature, which is not unexpected if the greater spatial extent of the bigger towns is borne in mind, was the more "drawn-out" character of some of the epidemics in the large towns.

In order to investigate the time between epidemics, a precise definition of an epidemic is required. This is rather arbitrary, but it is advisable to adopt a definite rule throughout. In the one adopted, weekly notifications had to rise above one case per 4,000 population, except in small towns for which just a few notifications would give this frequency. In the latter case, a minimum of 8 notifications in at least one week were required, with some further cases in previous or succeeding weeks. (This last rule is perhaps rather too stringent and might tend to introduce a slight spurious relation between community size and epidemic period, but from inspection of the complete series of notifications no genuine epidemics seemed excluded by this rule in the smaller towns.)

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* That is, in this paper. A complete set of graphs was displayed at the meeting.

It is known that epidemics in different towns, while necessarily having some linkage, do not always remain in phase. Some subsidiary investigation of this point was made by a deliberate choice of four further Welsh towns situated roughly on the rim and centre of a "wheel", with Ffestiniog and Llanrwst forming two of the towns on the rim. The remaining towns were Bala, Bethesda, Dolgelly and Portmadoc. These towns are too small for any very systematic epidemic pattern to be discernible, and some tendency to remain in phase and some tendency to get out of phase both seemed to be present. Thus in 1942 all towns were in phase except for Llanrwst, which missed altogether. Beginning in 1946, phase differences were considerable, Bethesda and Ffestiniog being in phase, Portmadoc not getting an epidemic until 1947, and Bala and Dolgelly until the beginning of 1948. Llanrwst again missed. The next general outbreak began with Llanrwst at the end of 1950, occurred in Bethesda, Portmadoc and Dolgelly in the middle of 1951, in Ffestiniog at the beginning of 1952, and again latest in Bala, at the beginning of 1953. Some of these phase differences are of course much too large to be compatible with direct infection from one town to another, and a much more detailed tracing of individual infections would be needed before a complete picture was available. This is rarely feasible except for very small communities.

3. *The Observed and Theoretical Relation between Measles Periodicity and Community Size*

A rough theoretical model of the distribution of times between epidemics when fade-out of infection occurs after major epidemics was referred to in my Berkeley paper. From equation (5.7) of that paper it was noted that the mode of this distribution is of the form $a + b/\sqrt{\varepsilon}$, where ε is the immigration rate for new infection. The mean of the distribution has a less simple mathematical expression, but may be taken approximately to be of the same form. If we now make the further rough assumption that immigration of infected persons will tend to be proportional to the town size N, it follows that the mean period between epidemics should for small towns be roughly of the form $a + c/\sqrt{N}$. This relation will, however, be inapplicable to large towns, for when a town is large enough for fade-out to be negligible, the mean period should conform more to the value given by the deterministic Hamer-Soper model, and in so far as the characteristics and communicability of the disease remain constant from town to town, the period will be constant within this range.

To compare the observed times between epidemics with these theoretical predictions, it was first necessary to obtain estimates of the mean periods. In the case of towns for which the weekly notifications formed an unbroken series, separate outbreaks were required to have an intervening "trough" of less than 1 notification per 20,000 for two consecutive weeks. The mean period was calculated from the complete periods observed in all cases, except in the case of the smallest towns, for which only one or two epidemics occurred. In such cases the evidence on the mean period provided by the time intervals at the beginning and end of the total time of the available records was important, for an alternative estimate is obtained by taking this total time and dividing by two more than the observed number of epidemics. Such an estimate is obviously biased, but *negatively*. When such an estimate still exceeded the first estimate, it was substituted.

It was interesting to find that the evidence for fade-out, judged by three or more consecutive weeks showing no notifications, went strictly with the order of towns given in Table 1, Birmingham and Manchester showing no fade-out, Bristol, Hull and Plymouth occasional fade-out, and all the smaller towns listed invariable fade-out. These three groups are divided by the vertical dotted lines shown in Fig. 1, in which a plot of mean period against $1/\sqrt{N}$ has been made. The mean periods there depicted are recorded in Table 2 and of course are subject to greater error in the case of the smaller towns, as both the dispersion of the individual periods is greater and there are less observed periods to calculate the average from. With this *caveat*, the observed relation, while naturally imprecise, seems entirely compatible with the predicted relation. For completeness the "linked" Welsh towns were also added, and again these fitted with the broad relation observed from the original nineteen towns chosen.

It would be possible to discuss to what extent apparent departures from a strict relation between mean period and town size might be real. For example, Cardigan gives a somewhat large period for its size, and Carmarthen a somewhat small one. This may well reflect a higher

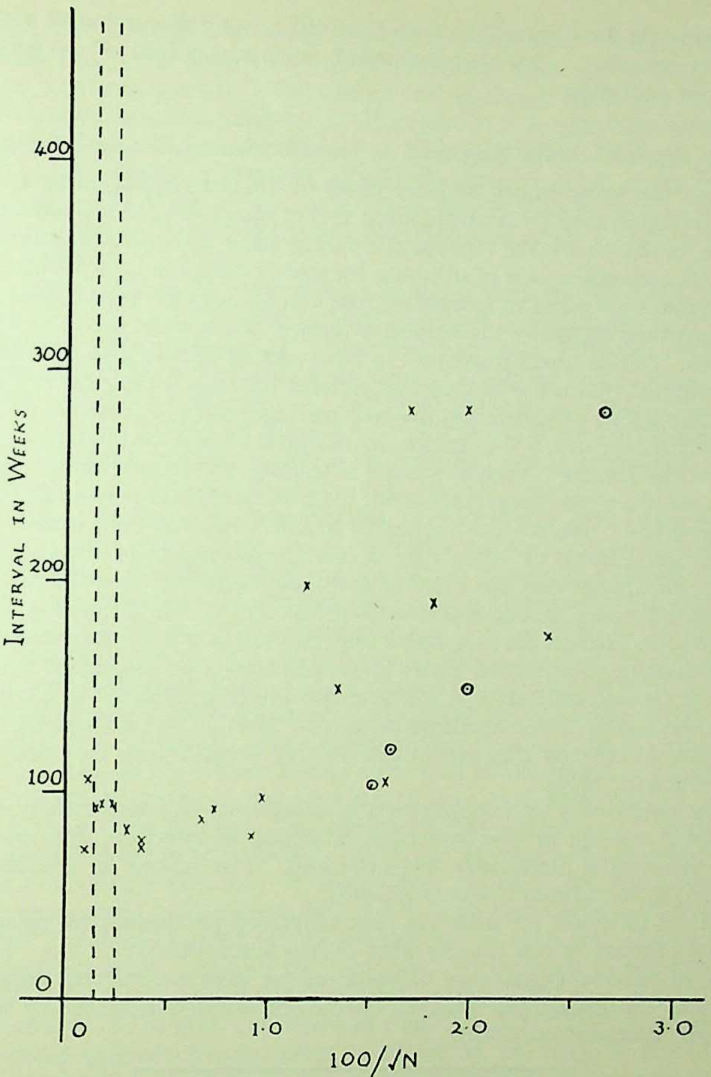


FIG. 1.—Observed relation between epidemic period and population (*N*).

TABLE 2

Mean Period Between Epidemics

Town	*Period*
(1) Birmingham	72·7
(2) Manchester	106·0
(3) Bristol	92·1
(4) Hull	93·0
(5) Plymouth	94·3
(6) Norwich	80·4
(7) Barrow-in-Furness	74·0
(8) Carlisle	75·2
(9) Bridgwater	86·4
(10) Newbury	91·6
(11) Carmarthen	79·0
(12) Penrith	97·6
(13) Ffestiniog	199·0
(14) Brecon	149·0
(15) Okehampton	104·6
(16) Cardigan	284·3*
(17) South Molton	191·3
(18) Llanrwst	284·3*
(19) Appleby	175·2
(20) Bala	284·3*
(21) Bethesda	104·0*
(22) Dolgelly	150·0
(23) Portmadoc	121·9*

* Based on total time interval.

relative immigration rate for Carmarthen than Cardigan; but a discussion of such possible local differences was not intended. If it were attempted, further appraisal of the proximity of other towns would also be necessary.

4. *The Artificial Series Generated on the Manchester Electronic Computer*

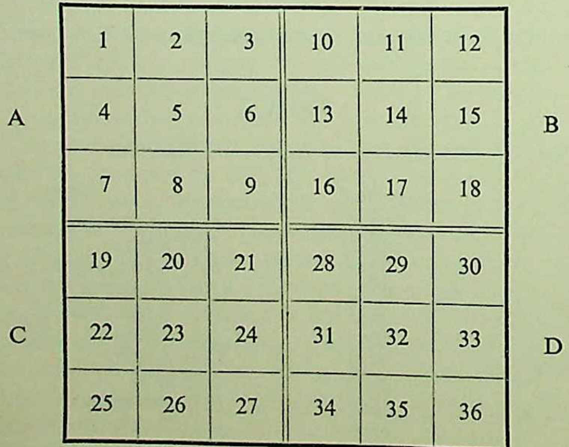
Some discussion was given in my Berkeley paper of the likely effect on an epidemic series of the movement of infection over an extended area; but in the absence of a theoretical solution it is hardly possible to proceed very far without generating some artificial realizations based on the assumed model. A convenient way of allowing for spatial diffusion is to set up a "grid" of cells or "wards", such that movement of infectives from contiguous cells is possible.

Even on an electronic computer the number of events taking place increases very rapidly with the number of cells. While it was important to have enough cells to give a reasonable statistical picture, it was reckoned that a 6 × 6 grid was perhaps the largest that was feasible. The basic model used and the method of generating the artificial series were indicated in my previous paper, the only modification here being due to the spatial grid. Some of the simplifications may be felt to make the model academic, such as the use of transition probabilities homogeneous in space and time, and the use of a continuous removal rate of an infected person giving an average infectious "lifetime" of a fortnight. No seasonal variation in infectivity has been inserted. It should be recalled that observed seasonal variation may be a secondary aspect of school assembly and dispersal, and it may be argued that the effect of such heterogeneities in space and time should be allowed for (cf. Hope Simpson, in discussion following Bailey (1955)). However, it is again stressed that it is the large-scale features that are under investigation in a semi-quantitative way, and that if these appear promising these further elaborations can always be considered if required.

The use of a migration coefficient ε of infectives between cells separated by a common boundary was preferred to the use of a non-localized infectivity function (corresponding to movement of an infective to another cell and then returning), and for simplicity no movement of susceptibles was allowed (cf. Bartlett, 1956).

In view of the desirability of not making the community size larger than necessary, a size expected to be small enough to give invariable extinction of infection after one epidemic cycle (starting with a reasonable amplitude) was first tried. The scheme of coefficients and initial conditions, referred to as Scheme I, was as follows:

$\lambda = 0\cdot01$ (rate of infection per week per infected person per susceptible person in one cell);
$\nu = 0\cdot75$ (rate of entry in one cell per week of new susceptibles);
$\mu = 0\cdot5$ (rate of removal or recovery of infectives per week per infected person);
$\varepsilon = 0\cdot25$* (rate of migration per week per infected person in one cell to any one neighbouring cell with common boundary).



* This value for ε is believed to be reasonably representative (corresponding to the rough diffusion estimate 0·05 miles/$\sqrt{}$week suggested in Bartlett (1956) p. 103, provided the population density is that for the heart of Manchester); but it would be of interest to investigate the effect of changes in ε.

In cells 1 to 9 (A quarter) and 28 to 36 (D quarter), each cell had 3 infected persons initially and 75 susceptible persons. The other two quarters B and C also began with 75 susceptibles per cell, but had no infectives initially. This choice was of course somewhat arbitrary, but for the infectives was intended to reflect some initial heterogeneity. As the mean number of susceptibles in the deterministic Hamer-Soper model is $\mu/\lambda = 50$, it will be seen that the initial value of "f", the factor I have used elsewhere (Bartlett, 1956) to indicate the size of the epidemic swing, was of the order of $\frac{1}{2}$*. The value of $\sigma = \lambda\nu/\mu$, which largely determines the period in the deterministic model, was $66\frac{2}{3}$, and was sufficiently near the value of 68·2 used by Soper and adopted in my Berkeley paper.

Over a long period of time the mean number of susceptibles for the entire grid under Scheme I should be about $50 \times 36 = 1{,}800$, and the mean number of infectives 54. If we note that for Manchester the mean number of weekly notifications was about 170 from 1917–51, and the mean number m of infectives in the model is twice this figure, i.e. 340, we find a ratio of m to total population size of about 1 : 2,000 for Manchester. Using the same ratio, an equivalent total population size is obtained of about 100,000 for Scheme I. With this size (as is discussed more fully in §5) infection faded out after one epidemic cycle, as expected both from the actual measles statistics and from the rough theoretical calculations in my previous paper.

In Scheme II the population size was doubled by doubling ν. The value of λ had then to be halved in order to keep constant $\sigma \equiv \lambda\nu/\mu$. In order not to increase the number of cells, the value of ε was halved; this implies the same population density in a community double the size, or $\sqrt{2}$ times the linear dimensions, as the diffusion or migration standard deviation is proportional to $\sqrt{\varepsilon}$. With Scheme II the equivalent population size is of the order of 200,000, which is getting into the observed critical region. On the non-spatial model assumed for previous rough calculations, it was still not large enough to maintain recurrent epidemics (at least for $f = \frac{1}{2}$), but it was suspected that differences in phase within the community would help to avoid infection fade-out. It was thus hoped that Scheme II, which corresponded to about the maximum population size that could be tolerated on the present basis,† even with an electronic computer, would begin to display the continuity of epidemics characteristic of very large communities.

In Scheme II the average number of susceptibles is 100 per cell, so the initial number was put at 150. The initial infected were as before. (It should be mentioned that owing to an oversight Scheme II was first started with only 75 susceptibles per cell. As this is below the threshold density, of course no major epidemic resulted and the infection began to fade out straightaway.)

The "weekly" numbers of infected and susceptibles in each cell, together with the weekly notifications, constituted the output of the computer. In addition to the total numbers for the whole community, the totals per quarter were summarized. The total weekly notifications, and those for each quarter of the grid, are shown in Figs. 2 and 3. The notifications for quarter D showed a somewhat anomalous drop before finally rising, so much so that the epidemics in B and C caught up with D. This seemed due to some peculiarity in the pseudo-random sequence used to generate the Monte Carlo sequence. Again, owing to an oversight, the sequence of "random" numbers used in Scheme II was begun as in Scheme I. This led to some similarity in behaviour at first, but this connection of the same numbers with the stages of the epidemic sequence finally disappeared of course as the sequence progressed. The notifications for Scheme II are recorded, as far as they were available when this paper was prepared, in Figs. 4 and 5.

5. *The Observed and Theoretical Critical Community Size*

The immediate and rather gratifying property with Scheme II is the tendency to avoid fade-out, manifested by the persistence of infection during the aftermath of the first epidemic. This is clearly sufficient, from the results recorded in Figs. 4 and 5, to begin a new epidemic,‡ even although infection at one stage faded out of all three quarters B, C, D and persisted only in A. It

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* A more relevant value of f is calculated at the "trough" of the epidemic; this turned out to be $\frac{3}{4}$.

† It is likely that the use of further approximations, in which the numbers of susceptibles and infectives are treated as continuous variables except at critical stages, would enable larger community sizes to be handled more efficiently, but the present scheme was most direct and was preferred at this stage.

‡ A graph showing the latest results available, including a substantial part of the second epidemic, was displayed at the meeting.

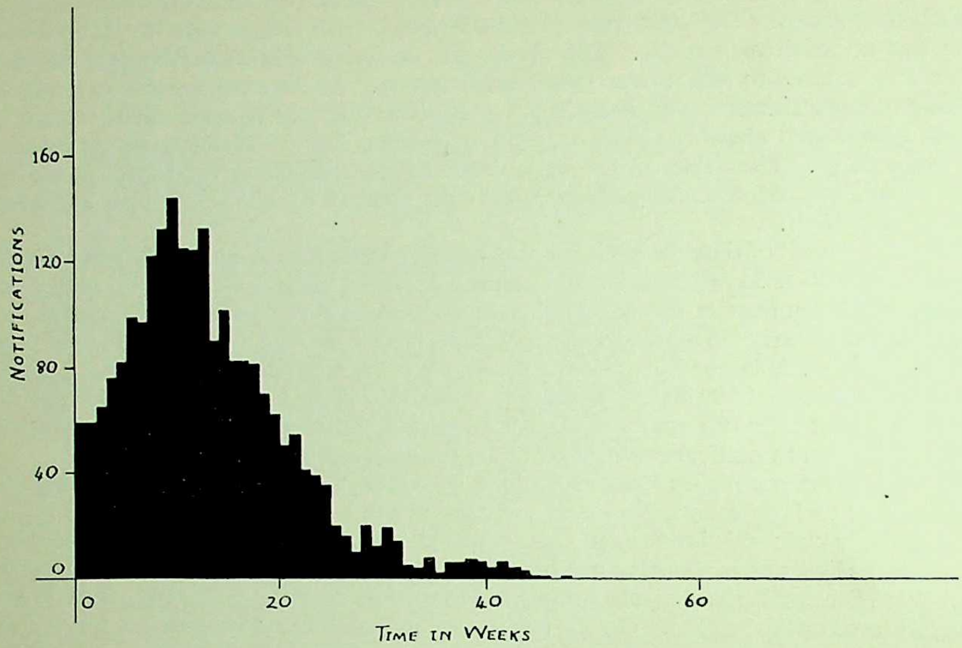

FIG. 2.—Artificial epidemic series, Scheme I. Total notifications per week until "fade-out".

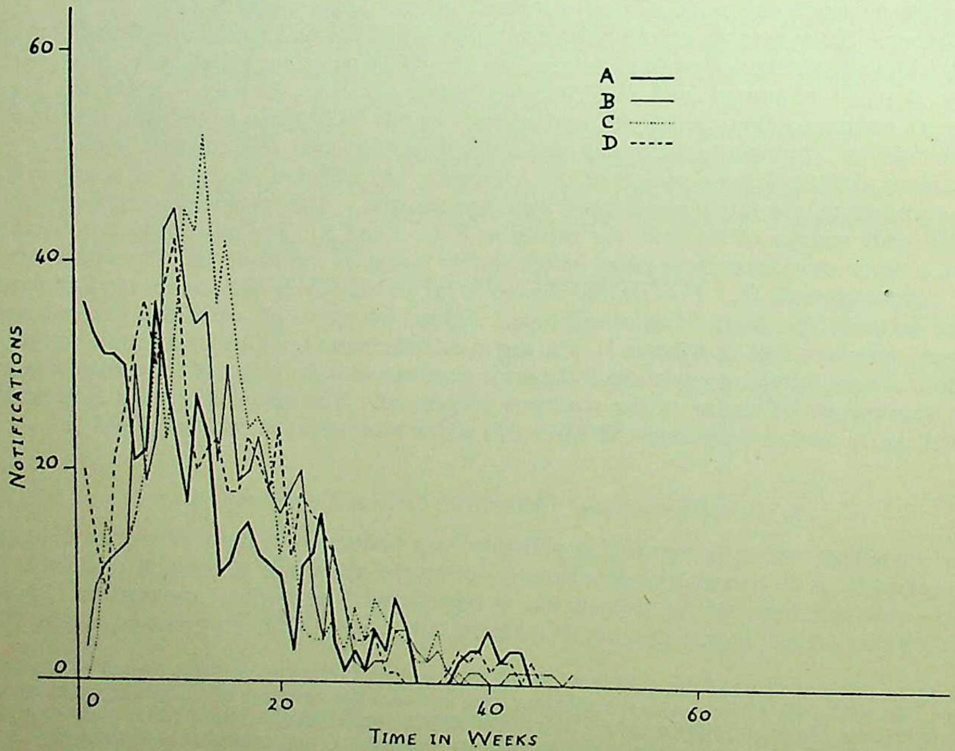

FIG. 3.—Artificial epidemic series, Scheme I. Weekly notifications separately for each quarter of the total area.

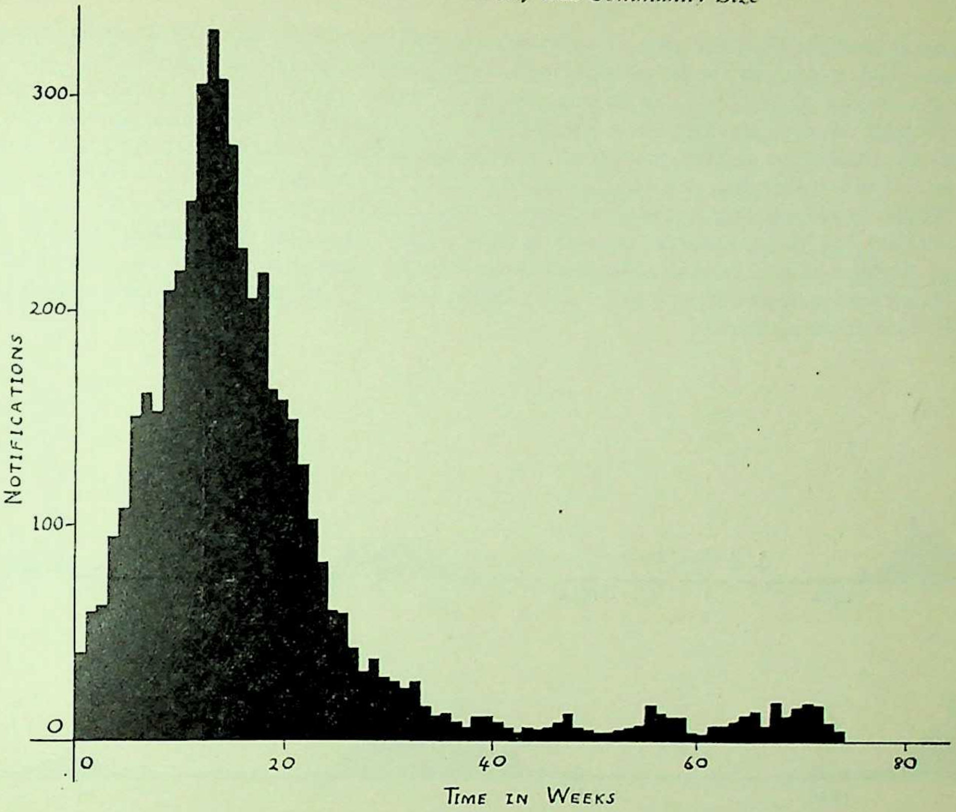

FIG. 4.—Artificial epidemic series, Scheme II. Total notifications per week (to be continued).

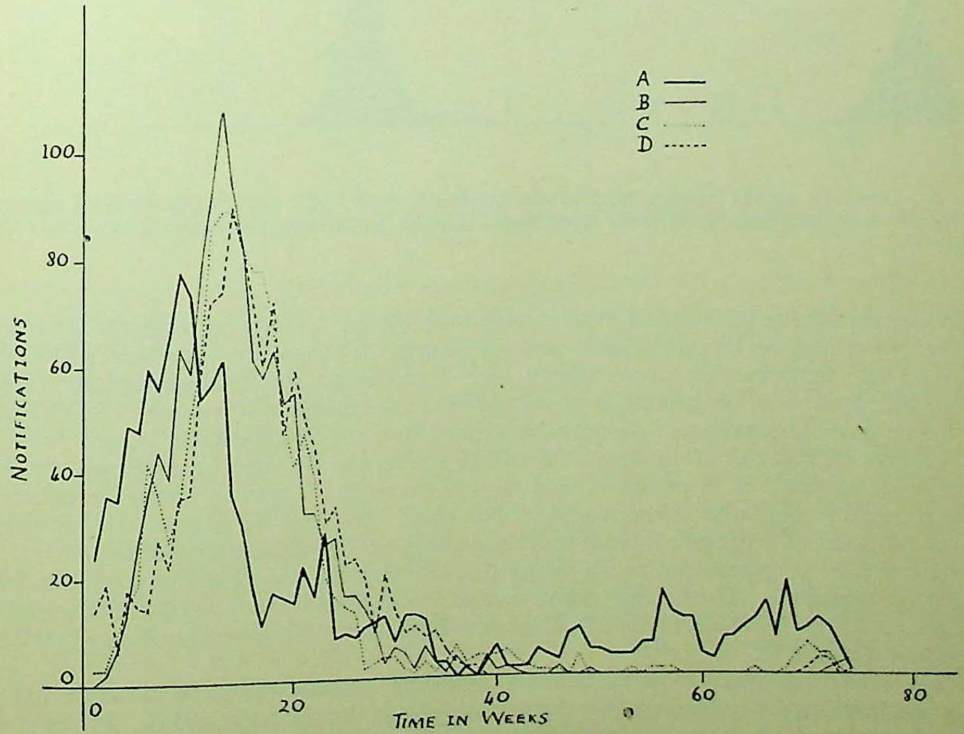

FIG. 5.—Artificial epidemic series, Scheme II. Weekly notifications separately for each quarter of the total area.

is probable that the simplification of a continuous removal rate of infectives tends to favour persistence, partly owing to the increased deterministic damping to an equilibrium level, and partly because fade-out of infection can be less abrupt. There is also the difficulty that the stochastic "equilibrium" level of fluctuations is not known (the occurrence of local fade-out certainly complicates the theoretical picture, and no satisfactory method of calculating the amplitude of oscillations allowing for this fade-out effect is yet available); the artificial sequence should be allowed to continue, to see whether in the absence of fade-out a reaonsably stable series has been reached, and this point is being checked as soon as practicable. It should be recalled that the critical average number of infectives as calculated in my Berkeley paper dropped by a factor of 10 when the f factor was increased from $\frac{1}{2}$ to $\frac{2}{3}$; and a higher effective f in actual communities could reduce the critical size considerably.

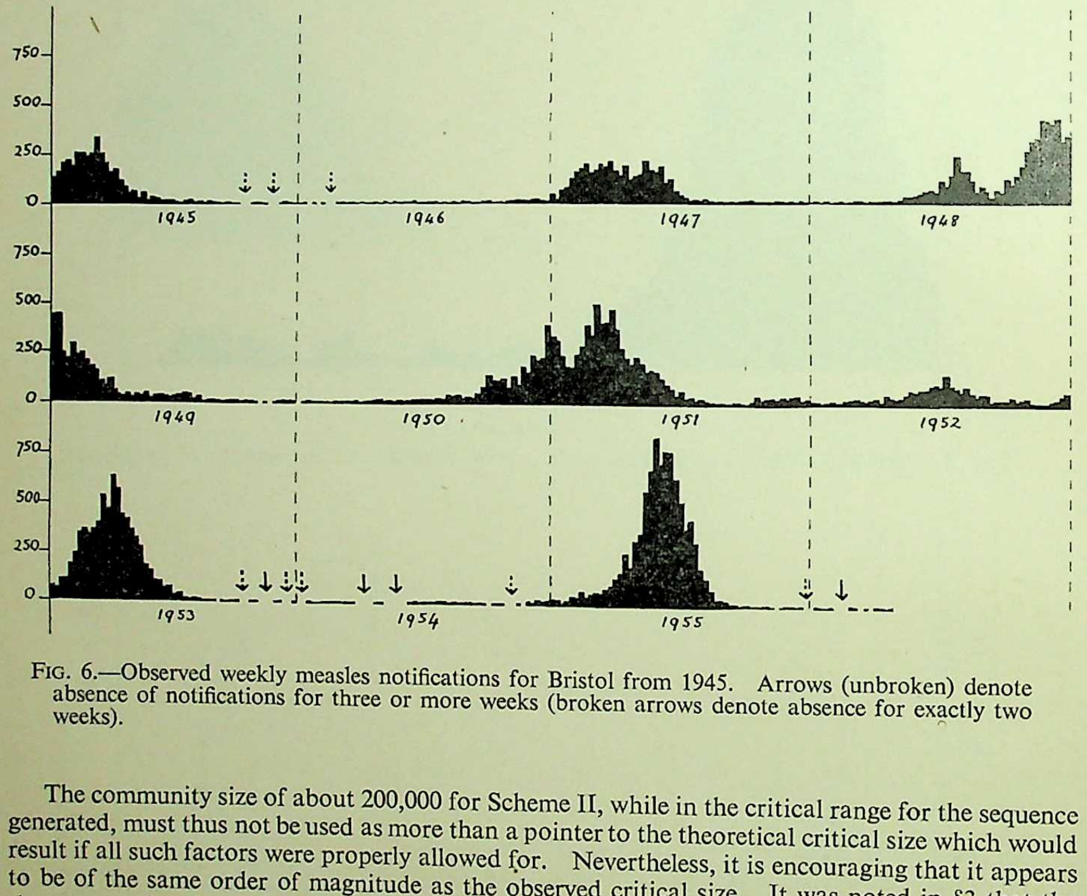

FIG. 6.—Observed weekly measles notifications for Bristol from 1945. Arrows (unbroken) denote absence of notifications for three or more weeks (broken arrows denote absence for exactly two weeks).

The community size of about 200,000 for Scheme II, while in the critical range for the sequence generated, must thus not be used as more than a pointer to the theoretical critical size which would result if all such factors were properly allowed for. Nevertheless, it is encouraging that it appears to be of the same order of magnitude as the observed critical size. It was noted in §3 that the three towns showing occasional fade-out were Bristol, Hull and Plymouth (the Bristol series from 1945 is shown in Fig. 6). If we define the critical size as one for which the chance of fade-out after a major epidemic is 50 per cent., then the two towns in the relevant range are Bristol and Hull (Plymouth giving rather a higher chance than this). The *observed* critical size is thus of the order of 300,000. It is of some interest that the average weekly notifications for Bristol and Hull were 67·0 and 57·0 respectively, rather nearer together than the corresponding total populations would have suggested. The corresponding critical size arrived at via the Manchester statistics would be 62 × 2 × 2,000, i.e. about 250,000, somewhat closer to the corresponding figure for the sequence of Scheme II.

It was considered whether an independent empirical estimate would be available from the weekly notifications for individual Manchester wards from 1925 (Linnert, 1954). By the pooling of contiguous wards, a decreasing probability of fade-out should be obtained. Unfortunately, the theoretical functional form of the fade-out probability with the community size N (if the effect of size on the factor f is ignored) is e^{-aN}, and as the pooled wards are still unsegregated from the remaining wards, a further probability factor arising from the chance of immigration of infectives should be allowed for, as this can hardly be negligible. If the assumption made previously is retained, that the immigration rate is proportional to N, this extra factor would also be of the form e^{-bN}. Hence it would not be possible without independent information on the immigration rate to obtain by this means an estimate of the critical size of an *isolated* community. Of course the same effect must operate to some extent for entire towns like Bristol and Hull, giving an observed critical size somewhat less than the correct value for an entirely isolated town, but the effect here is probably not very great.

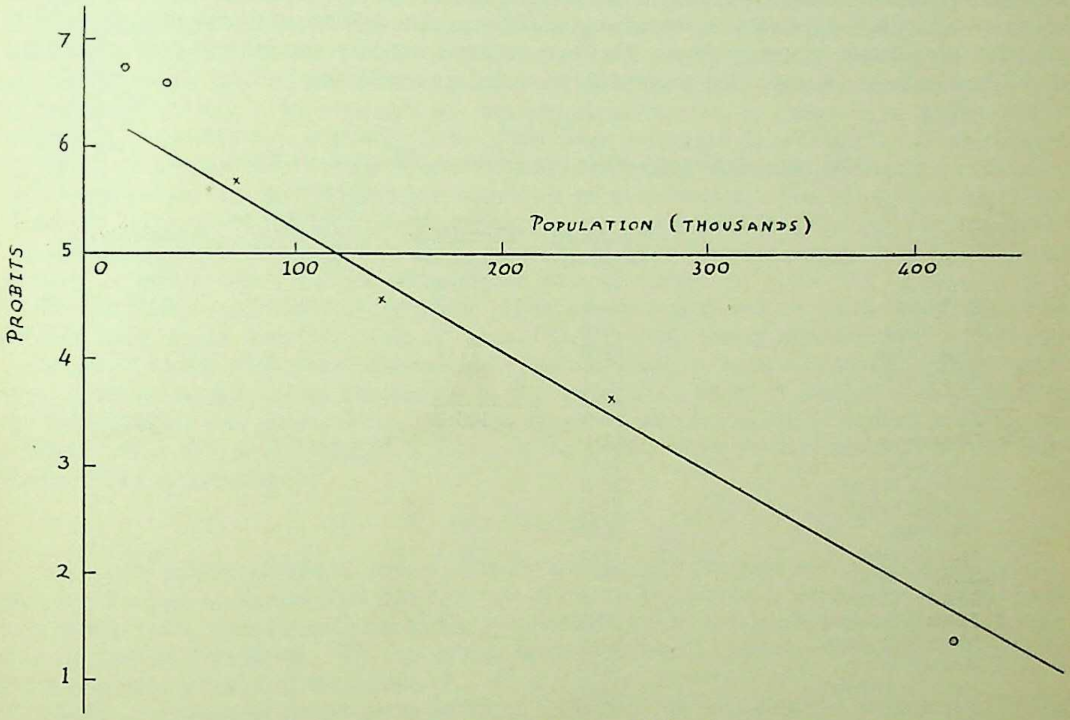

FIG. 7.—Observed relation between "fade-out" for area of Manchester and population of area.

However, in spite of the above limitation it was thought worth-while summarizing the information in the Manchester records. A cumulative set of wards was accordingly selected, with total populations ranging from about 20,000 for the smallest group (one ward only) to about 400,000 for the entire set available. In the period 1925–50 inclusive*, thirteen major epidemics occurred in Manchester, the first at the beginning of 1926 and the last at the end of 1950. Hence we have twelve epidemics (omitting the last) for checking fade-out effects. The information so obtained is shown in Table 3. In spite of the suggested theoretical form of the extinction probability, it was empirically more convenient to use a rough probit analysis to obtain the 50 per cent. point for fade-out. The successive observed probabilities are of course not independent, and no fit beyond the first visual line was attempted (except to note that the "working probits" for 0 per cent. or 100 per cent. probabilities would not greatly affect the estimate of where this first line cuts the 50 per cent. probability, this being at about $N = 120{,}000$—see Fig. 7). Remembering that this is necessarily an underestimate of the critical size of an isolated community, owing to immigration effects, we have reasonable consistency with the previous estimate from separate towns, of about 250,000. The difference suggests a rather high "immigration" effect between the Manchester wards, though the figures are too roughly determined to justify any more elaborate discussion of this point. It might be remembered that any investigation on observed fade-out must be affected by notification omissions.* Moreover, no attempt has been made to obtain figures for the populations at risk, and information on the numbers of susceptible children would have been useful. In the absence of such information no indication of the size of the f factor is available. Evidence from other sources suggests that for large towns at least the swing in numbers of susceptibles may be relatively low; Hedrich (1933), for example, gives an estimated range for Baltimore (U.S.A.) of 30–50 per cent. of the total child population under 15, equivalent to an f factor of 30 per cent./40 per cent. or $\frac{3}{4}$. If this were a representative figure, the theoretical critical population size would be comparatively small (cf. p. 53). However, the complication that the peak incidence occurs not in the youngest children, but in children of early school age (see Wilson *et al.*, 1939) stresses that the availability of the total susceptible population is not uniform, and may imply a smaller "effective" population of susceptibles. This may be perceived from the extreme case when, say, all children under 5 are supposed completely protected from infection.

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* Although measles notifications were made compulsory in Manchester from 1917, alterations in ward boundaries make the year 1925 a preferable starting point.

TABLE 3

Observed (Aggregate) Fade-out Effect in Manchester Wards

Ward	*Population (Thousands)*	*Cumulative Population*	*Number of Epidemics followed by Fade-out*	*Probability of Fade-out*
Ardwick	18·4	18·4	12	100%
St. Mark's	19·8	38·2	12	100%
St. Luke's	20·6			
New Cross	13·0	71·8	9	75%
All Saints	13·1			
Beswick	22·1			
Miles Platting	14·9	140·8	4	33%
Openshaw	18·9			
Longsight	26·1			
Gorton North	22·4			
Gorton South	27·0			
Bradford	26·0			
St. Michaels	11·8	254·1	1	8%
St. George's	16·5			
Medlock Street	15·8			
Moss Side West	19·8			
Moss Side East	17·1			
Rusholme	20·6			
Newton Heath	21·3			
Collyhurst	12·6			
Harpurhey	18·9			
Cheetham	22·6	419·3	0	0%

As a further check on the "fade-out" estimate for non-isolated urban areas, the detailed measles figures for St. Pancras given by Stocks and Karn (1928) were examined. A similar analysis was made as with the Manchester wards, aggregating the squares into which these authors divided the total area they were studying. As, however, only two major epidemics occurred during the three year period available, March, 1924–March, 1927, no more than a very crude check was possible. The results are summarized below, two consecutive 10-day periods without further notifications being taken as the fade-out criterion.

TABLE 4

Aggregate area*	H16	+J16	+H15 +K16	+G15, 16 +J17	Total area†
Total number of notifications	787	1,616	2,834	4,628	6,468
Number of epidemics followed by fade-out	2	1	1	1	0

* For key, see Table 9, Stocks and Karn (1928).

† Excluding "fringe" squares where less than 50 notifications occurred, viz. D15, F14, 17, K15, L17.

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* For statistics of notification omissions, see, for example, Chope (1940). [I am indebted to Dr. Hope Simpson for this reference.]

It will be seen that the median area giving 50 per cent. fade-out had 2,834 notifications, compared with the figure 6,547 for the St. Pancras district, or 43 per cent. The total child population (under 15) was 54,600 in 1924, so for the median area it was about 23,500. Using the Manchester conversion ration of child to total population of 4·43 (Linnert, 1954), the equivalent (Manchester) total population was 110,000, compared with the critical figure of 120,000 obtained from the Manchester wards. (The agreement is closer than we have any right to expect, bearing in mind the 3 : 1 population range in the middle three entries of Table 4.)

Summary and Conclusion

To sum up the results presented in this paper, these have been concerned with the confirmation of two phenomena predicted from the stochastic model for measles previously put forward. The first is a relation between measles periodicity and community size, provided the communities are comparatively isolated and do not exceed a certain critical size. The second is the existence of such a critical size. For the model this has been demonstrated, by means of an artificial series generated on an electronic computer, to be of the order of magnitude of 200,000 total population (in terms of Manchester figures), at least if the initial swing of susceptibles proves sufficiently representative of the quasi-equilibrium amplitude of such swings. The observed critical size (again in terms of Manchester figures) appears to be about 250,000. For non-isolated urban areas, the critical size is less, due to the immigration of infection from surrounding districts, and appears to be only about half the critical figure for entire towns.

Provided the main features of the present model appear reasonable, one of the points requiring further study is the effective swing of susceptibles, in both theory and practice. Admittedly the idealized model put forward still contains various limitations, some of which have been noted. It is, however, suggested that no previous (e.g. deterministic) model of measles could cope with the large-scale type of data collected and here presented, whereas by contrast characteristic properties of these data were predicted by means of the model. Such predictions have been broadly confirmed from the statistics.

Acknowledgments

I am very much indebted to Mrs. L. Linnert for the help she gave me in this investigation. Her own interest in the collection and collation of the various statistics, especially (with the kind help of Mr. Hart, of the Manchester Public Health Department) the detailed data for Manchester, was particularly stimulating. My thanks are also due to Miss C. Caley for further help, including the preparation of most of the graphs.

I wish to express my indebtedness to Mr. J. C. Gower for preparing the programme for the Monte Carlo work on the Manchester electronic computer; also to Dr. D. C. Gilles for his help in putting this programme on the computer.

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DISCUSSION ON PROFESSOR BARTLETT'S PAPER

DR. REID: It gives me great pleasure to propose a vote of thanks to Professor Bartlett for this important contribution to theoretical epidemiology. In the discussion after a paper by the late Professor Greenwood, Professor Kendall asked whether this sort of approach to epidemiology was better made by a doctor with some taste for mathematics or a mathematician more fully competent to deal with the analytical complexities involved. As a medical teacher of epidemiology who lacks that competence, I for one welcome Professor Bartlett to this field. Although Jonathan Swift in his book published in 1720 on *Precedence between Physicians and Civilians* described himself as "a master of the stochastic art" he did not say it was a distinguishing feature of the professional; and I shall be singularly relieved if others deal with the mathematics and leave me with the measles.

Measles has always had obvious attractions for the epidemiological theorists. Diagnosis is reasonably certain and the disease is generally notifiable throughout the country. But here we come at once to the question which haunts these theoretical studies—are the basic assumptions reasonable? I hope that Professor Bartlett's illusions about the noblest profession will not be shattered by the cynical comment in a recent *Lancet* leader. Discussing age differences in reported measles attack rates the *Lancet* points out that the trends observed "might be related in some way with the 1948 increase in the fee from 1*s.* to 2*s.* 6d. for notifying measles. It is not an uncommon experience elsewhere to find that both measles and whooping cough notifications showed a definite increase as practitioners realized the advantage. Many said quite frankly that they would not bother to write out and post a certificate for a shilling, when all the other diseases were worth half a crown". As Mr. Bailey has shown by his demonstration of the positive correlation between tonsillectomy and circumcision rates in Cambridge undergraduates and the fees of the public schools they attended, the dollar sign has not yet been taken out of medicine. My point is not entirely frivolous, for the decision of what to call an epidemic in small towns of less than 5,000 population may be affected by considerations of practitioner habits and pressure of work and the probability of under-reporting. A Poisson expectation may not be entirely appropriate to a localized outbreak of an infectious illness but it would seem to me that if the standard rate is 1 case per 4,000 population Professor Bartlett has been unduly strict in demanding at least 8 notifications in a week in small towns of 5,000 or less before counting the end of an inter-epidemic interval. Since under-reporting is a serious risk, the spurious correlation between community size and inter-epidemic period may be even greater than Professor Bartlett fears.

In section 3 of his paper, Professor Bartlett shows, however, that even in the large town the mean period between epidemics is negatively associated with population size. In his discussion of this result, he rather implies that any inconsistencies observed may be the result of differences in the rates of migration into these communities. Certainly a good fit is obtained between observation and expectation based on the theory expressed in the formula (5.7) of his Berkeley paper which gives the mode of the distribution of intervals as $a + b/\sqrt{\varepsilon}$ when ε is the immigration rate for new infection. But in practice this rate is not estimated directly but simply taken as proportional to N, the size of the population. This in turn means that, as far as migration is concerned, the goodness of fit may be almost entirely coincidental. In Frost's original formulation of the measles epidemic problem, frequency and effectiveness of contact between infective case and susceptible plays a dominant role in determining the epidemic pattern. In larger towns then, differences in the manner of life with multiple opportunities for contact might well prevent "fade-out" quite apart from any immigration of infective cases and immunes from outside. I suggest, therefore, that the term "migration" should, in the present state of our knowledge, be interpreted in the widest possible sense.

The problem of movement into and within populations comes up again in the study of the spatial distribution of disease incidence. In some early work on the spread of infection from smallpox hospitals, the occurrence of cases outside but close to the hospitals raised the possibility of widespread aerial dissemination from the hospitals. Field studies soon showed, however, that such cases were more likely to arise from contact with patients who refreshed themselves at one of the local hostelries just before admission to the hospital. The realities of the situation should not be too lightly dismissed. In measles epidemics in circumscribed urban areas, the relative importance of chance factors in contact makes stochastic models even more appropriate than in the study of epidemic behaviour in time in large populations, but it is in just these circumstances that the heterogeneities in space and time pointed out by Hope Simpson may bulk so large. A diagram of the epidemic spread in an outbreak among children in 6 Southall schools some years ago may make these points clear. Infection spread from area to area largely through the movement of susceptible children from neighbouring sectors to the corresponding schools and at school the duration of infectious exposure before removal is inevitably much shorter than a fortnight. For the larger sectors which Professor Bartlett had in mind, such considerations may not be so apposite, but he might earn the gratitude of epidemiologists if he would return to the attack on the problems of small-scale investigations such as the recent survey by Dr. Nicol in the parishes of the Isle of Wight.

A few days ago I was gratified and flattered to receive a request from Professor Kendall's department for any reprints which I might still have available. The sting was in the tail: "papers on the practical aspects of statistics", it said "are not required". Professor Bartlett may wish to be reassured that his paper is not going to be dismissed on the score of immediate application in epidemiological practice; but he should be warned that if he continues in this way the value of the stochastic outlook in epidemiological forecasting will be appreciated by practical men sooner than he now anticipates.

Mr. N. T. J. **Bailey**: It gives me great pleasure to have the privilege of seconding the vote of thanks to Professor Bartlett.

The application of stochastic processes to the mathematical theory of epidemics gives rise to several inter-related difficulties. In the first place there is the purely mathematical aspect: one is liable to arrive very quickly at an intractable mass of algebra. Indeed, merely to write down the equations for a continuous-infection process in a two-dimensional continuum uses up quite a lot of paper. Secondly, there is the difficulty of hitting on a model that is sufficiently close to the epidemiological realities, without being too complicated to handle either verbally or mathematically.

Professor Bartlett's previous work has already contributed considerably to our understanding of the behaviour of recurrent epidemics. When the spatial element is absent considerable progress can be made along purely mathematical lines; but in the extension to planar processes the difficulties are formidable. One way out is to use the Monte Carlo method of building up mock epidemics in the way that Professor Bartlett has done. The work described in this paper is undoubtedly a landmark of major importance in the advancement of epidemic theory.

As with all good applied mathematics, the purely abstract development must be constantly checked against experiment and observation. It is therefore particularly gratifying to see how Professor Bartlett has managed to establish a broad correspondence between his theoretical results and actual epidemiological data. This is a particularly difficult thing to do in the field of large-scale phenomena; the corresponding procedure is much easier when we deal with epidemic processes in small groups like families or school classrooms.

One of the signs of the times is the use of an electronic computer to handle the Monte Carlo experiments. Provided they are not made an excuse for avoiding difficult mathematics, I think there is great scope for such computers in biometrical work. To speak only of the epidemiological field, there are several applications that come to mind, apart from the one made here. In this one wants to study the large-scale behaviour of epidemic processes in time and space under a wide variety of conditions.

In small-scale studies, on the other hand, we are more concerned to fit theoretical models in detail. We want to deal with the distributions of latent periods and infectious periods, and this involves the estimation of several parameters—not less than four in the simplest case, and appreciably more in models that are only slightly more realistic. We may before long, especially if further data become available, be faced with the problem of estimation on a considerably increased scale. It will certainly be worth seeing what help can be obtained from electronic methods of computation in applying, say, standard maximum-likelihood scoring techniques.

Another aspect I should like to mention is the perennial dream of epidemiologists, mathematical and otherwise, of being able to predict the course of an epidemic from its initial stages. Statistically speaking, this is in general likely to be unsatisfactory because of the inherent lack of information in the data. Nevertheless, when records for the first few weeks, of, say, a polio epidemic have been accumulated, the kind of predictions *potentially* available must become more precise. If there were sufficient knowledge of the epidemiological state of a community when an epidemic started it should be possible to specify the course of the outbreak in terms of probability statements, which became progressively more accurate. It would, of course, be necessary to utilize a great deal of detailed social, clinical and epidemiological information, but such a procedure would, if it could be developed, be of considerable value to Medical Officers of Health, for instance.

I look forward to the development of these and many other applications suggested by the contents of Professor Bartlett's paper.

The vote of thanks was put to the meeting and carried unanimously.

Colonel W. BUTLER: Measles more than any other disease has been the plaything of the statistician. Perhaps this is due largely to the fact that its events, long recognized as in some way periodic, recur frequently and in high numbers.

I am not quite clear—beyond what is inferential from known facts—what Professor Bartlett's thesis is as to the effect of size of population upon the periodicity of measles epidemics. Certainly of far greater effect upon the character and periodicity of measles outbreaks than the size of the community attacked is the susceptibility-immunity constitution of any such unit of population. When measles attacks primitive races which for generations have been insulated nearly everyone succumbs; it is virtually universal in its incidence and it kills at a rate which is never experienced among inured peoples. Whole native tribes have in the past been completely eliminated by epidemics of measles. From that extreme of virtually universal susceptibility to the immunity constitution of population, such as is represented in this country, is a wide range mainly, but not wholly, determined by this circumstance. Of varying immunity and age-constitution, small and sheltered communities, isolated villages and sleepy towns will, of course, give developmental and periodicity patterns of epidemicity different from those of larger groups of population, densely packed and with a higher degree of intercommunication and commingling of persons.

Some 50 years ago I had during a number of years the opportunity of observing the incidence of measles in a suburban population in relation to its immunity constitution.* Out of 14,000 or so persons definitely exposed to infection by its outbreak in the home it was found that about 78 per cent. at all ages and 97 per cent. of persons aged 15 and upward had already suffered the disease. Of those at all ages who were exposed who had already suffered from it less than 1 per cent. were attacked; of those who gave no history of previous attack 66 per cent. succumbed. That of those not already protected by a previous attack 66 per cent. only failed and 34 per cent. escaped when exposed to the infection, contrasts with the almost universal incidence where infection is first introduced among aboriginal peoples, or returns only after a long interval, as in the Faroes, where in 1846 it reappeared after an absence of 65 years.

If you have already suffered from measles you have had conferred upon you in nearly all cases, a protection from future attack. If you have not had measles after six months of age—under six months you do not get it—until the end of life you are susceptible to attack if exposed.

Over 5 years of age, as the years advance the liability to attack gets rather less. I have suggested, and it has since become an almost accepted theory, that those not observed previously to have suffered, who have not, when known to have been exposed, been attacked, have acquired an active immunity by minor non-critical doses of the infection and, have built up at least a temporary mmunity.

Passing from the observed facts determinant of the nature of measles prevalence to description of its incidence and the periodicity of its recurrences in a population habitually subject to its presence, we enter a field of observation obscured in the past by lack of statutory notification.

As Professor Bartlett rightly says, "in order to investigate the time between epidemics", precise definition of what we mean by epidemic is required. Epidemic incidence is sharply defined when identified with "weekly notifications [which] had to rise above one case per 4,000 population except in small towns". But the definition, admittedly arbitrary, is *sui generis* and so inapplicable to the epidemicity of other diseases of markedly lower prevalence. Above all, it is fundamentally defective in that it fails to include commencing and ending phases of epidemic incidence, essential data, obviously, in any statement of inter-epidemic periodicity.

In an investigation of the epidemic features disclosed by statutory notification of measles, I examined at the time the returns of the first 8 years of its operation, 1940–47. As defining epidemic I took "the succession of phases in a continuum of growing, enduring and declining incidence which from a commencing rise develops a prevalence, for a time, above the mean of its weekly serial incidence and thereafter from its peak declines to sustained low levels or, it may be, an enduring zero".

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* "Measles", *Proc. Roy. Soc. Med.* (1913), 6, Sect., Epidemiology and State Medicine.

From the analysis of 8 years of measles notification, there emerged repetitive patterns of incidence the cycles of which conformed generally to well defined types.

Graphs* of the continuous incidence of measles, epidemic and sub-epidemic, during this period when for the first time this was possible, showed 4 completed epidemics, each of some 20 months duration and separated by a low level of sub-epidemicity in the intervening four months before commencement of another epidemic. Each succeeding cycle occupied completely the 24 months of the respective two-yearly periods. Beginning in January of one year, an epidemic reached its peak in March of the next and by September following had fallen to low levels definitely sub-epidemic and virtually of uniform incidence until the commencing rise in January of the following year. The duration of each phase of incidence is very different from what formerly was taken to be the case, and from what would be given by graphs of epidemic incidence as defined by Professor Bartlett. Instead of brief inter-epidemic periods of some 4 months, sub-epidemicity of over a year's duration would be shown and in lieu of the lengthy periods of developing, mature and declining epidemicity, high peaks of an intensive shortened incidence would alone be shown. The cycle of biennial periodicity would be apparent in either graph, but the recurrencies would be of wholly different phenomena.

In the more inclusive survey, the periodicity disclosed is richer in repetitive coincidences. Characteristic phases, the summer hump and the autumn notch, in the first year of developing epidemicity and other features throughout share in the rhythmic recurrences of now modified major events and their inter-relations. Generally the pattern was the same for England and Wales, London and the Great Towns, counties such as the West Riding, Durham and Middlesex, and great cities such as London and Manchester.

As in a composite photograph, composite graphs of two or more epidemics emphasized the more periodic and outstanding features.

Examination on similar lines of smaller communities was not undertaken, but it is assumed that in them only the most rudimentary, if any, conformity to type would be detected.

Dr. J. O. IRWIN: I should like to express my admiration, not only for this paper, but for all that lies behind it. Professor Bartlett seems to me to be the only person who has got near to accounting on theoretical grounds for the observed features of measles epidemics. In this particular paper he has concentrated on two points; a relation between measles periodicity and community size and the critical size below which this relation holds.

This particular work encouraged me to look at the paper which he read to the Symposium at Berkeley in California on the theory of recurrent epidemics and I was amazed to see how much had been done. For the first time I felt I really understood the relation between Soper's original deterministic model for measles epidemics and the new stochastic model and how the newer theory can account for the limitations of the old. The difficulty is best described in the quotation from Wilson and Worcester made by Professor Bartlett himself in the Berkeley paper: "It must be admitted that the phenomenon of recurrent measles epidemics gives no clear evidence of any damping. This creates something of a difficulty with the theory in respect to the prediction of damping and throws some doubt on the reality of periods; it is possible that measles simply dies out and then returns and under such a hypothesis there would seem to be no reason to expect either definite periods or damping to be observable by comparing successive epidemics."

Bartlett has got us out of this difficulty by analysing the effect of the more complete stochastic formulation and considering the spatial or topographical factor, that is, allowing for the fact that communities are not in fact closed. I have not yet fully understood all the intricate mathematical technique connected with the spatial problem, I hope to do so in time; but I did understand, I think, his treatment of the extinction problem and I did realize the skill with which his mathematics is directed to practical ends.

I was stimulated to a few general reflections on statistical method. There is always a tendency for statisticians to become slaves of their own techniques. It does not matter, provided we have the ability, how apparently recondite are the mathematical methods, if they are directed towards ends which are important and are finally put to the test of comparison with observation. It is very easy to overestimate the importance of significance tests; after all, it matters little if an effect is significant if because of its magnitude say, or on other grounds, it is unimportant. Too great an obsession with significance tests is apt to empty out the baby with the bath. This work on measles illustrates the point very well. It is to understanding the nature of the epidemic phenomenon that all this work is directed and tests of goodness of fit might only impede progress at this stage.

* *Bulletin of the Ministry of Health*, April 1946, April 1947, May 1948, and paper on "Whooping cough and measles", *Proc. Roy. Soc. Med.* (1947), **40**, Sect. Epidemiology and State Medicine.

I had the curiosity to turn up the discussion on Soper's original paper, read to this Society in 1929, at which I was present. Sir William Hamer proposed the vote of thanks. Among other things he said: "My poor little Jemima has grown beyond all recognition, if she be in any way related to the lady whose portrait Mr. Soper has unveiled this evening. The transformation he has effected can only be compared with that accomplished by Mr. Bernard Shaw's Pygmalion, the Professor of Phonetics who fashioned the Covent Garden flower-girl, Eliza Doolittle, into one of the most peerless Galateas that ever stepped off a pedestal." My imagination boggles at trying to suggest what Sir William Hamer would have said Bartlett had done with Galatea.

Mr. J. C. **Gower**: I should like to describe in a little more detail the programme for the Manchester computer which has produced the results that Professor Bartlett has been discussing.

A description of the computing procedure required in this type of problem can be found in Bartlett, *Applied Statistics*, **2**, No. 1, 1955. The epidemic model investigated there is much less elaborate, but is similar in principle to the model discussed to-day. The basic computing cycle is divided into two parts, first to calculate the time to the next event, which is assumed exponentially distributed, and secondly to decide which of the 228 possible events has occurred and to act accordingly. For both of these steps a random number is required. Owing to the fact that the computer makes not infrequent mistakes and in view of the apparent impossibility of getting an overall check on the accuracy of this type of calculation, it is necessary to repeat the calculations. For this purpose it is convenient to use one of the methods for producing pseudo-random numbers. In addition, the calculation is programmed in steps taking about a minute each which are then repeated and checked automatically. If the check fails the previous minute's calculation is repeated again, and checked and so on until two consecutive repeats agree.

Some difficulty was experienced in deciding which of the many available methods for producing pseudo-random number sequences would be the most suitable. In fact I carried out an investigation into three of the more simple methods, paying particular attention to the dependence of the successive groups of digits in the resulting sequences. Any undue bias of this sort could lead to rather peculiar results, in extreme cases probably producing an epidemic only in the lower right hand ward. These investigations indicated that the well known mid-product process was adequate for the purpose. In this method two numbers N_1 and N_2 are chosen at random, both of d digits. The product N_1N_2 is found and N_3 is equated to the middle d digits of the resulting $2d$ digits. The process is then repeated on N_2 and N_3 etc. The method has the disadvantage that it can get into a loop, repeating the same numbers in sequence, and in particular of looping on zero. To minimize this danger I adopted the following scheme:

The random numbers are produced in batches of 64. Each batch is tested for divergence from the expected number of unit digits. If the test fails a new batch is produced and tested. If three successive batches fail the machine stops and hoots continuously. As a further precaution for each run of the programme, a new pair of random numbers is used to start off the mid-product process.

Only once in the sixteen months during which the programme has been running have three successive batches of numbers failed. The random numbers x produced are assumed uniformly distributed between 0 and 1 and the exponential time interval t is obtained by the formula

$$t = -\log x,$$

allowance being made for the mean of the distribution.

Finally some figures concerning the time taken to carry out the calculations. It should be remembered that the computer at Manchester is quite old for an electronic machine and more modern equipment would be considerably faster. Furthermore some improvement could probably be obtained by re-coding parts of the programme.

	Epidemic Time	*Machine Time*
Scheme I . . .	49 weeks	40 hours
Scheme II . . .	105 ,,	159 ,,

These figures are possibly a little inaccurate as I have not been running the programme myself and have had to estimate them from the number of occasions when I have received results. In an hour's computing time the machine can deal with some 2,000 events.

Mr. D. G. **Kendall** (amplified in writing): Professor Bartlett's fascinating study of the development of an epidemic in space and time has encouraged me to collect some fragmentary results on the behaviour of a similar but much simpler deterministic system. Consider an infinite uniform two-dimensional population for which σ denotes the number of persons per unit area.

We shall count as members of the population only those individuals who are old enough to catch and transmit the disease (thus, if the "disease" is a rumour, we shall exclude those children who have not yet learnt to talk), and the problem will be further simplified by neglecting the effects of spatial migration, the recruitment of fresh susceptibles from lower age-groups and mortality from other causes. There will accordingly be no question of the epidemic's recurrence in time, but it is still possible to ask non-trivial questions about its spatial development, the extent to which an initially local infection can ultimately leave its mark on the whole population, and so forth. These are questions which must necessarily be asked in any thorough study of spatio-temporal epidemic behaviour although they are rather different in character from the questions with which Professor Bartlett has been concerned in his present paper.

If dS denotes an element of area then we may follow Kermack and McKendrick* and classify the σdS individuals resident in dS as follows:

$x\,\sigma\,dS$ "susceptibles",
$y\,\sigma\,dS$ "infected-and-infectious",
$z\,\sigma\,dS$ "removed",

where as usual the third class (of "z-men") contains isolated infected cases, persons who have died from the disease and persons who have recovered from it and are now immune. (It is the separation of the second and third classes which distinguishes the present model from that of Rushton and Mautner.†) The classification is supposed exclusive and exhaustive, so that

$$x + y + z = 1 \qquad \qquad (1)$$

at all times and at all places (each term on the left-hand side of this equation can vary with the time t and with the position P in the plane). We now write down a set of differential equations analogous to and modelled on those of Kermack and McKendrick. They will represent the joint effects of (i) *removal* (converting y-men into z-men) acting at a rate proportional to y and (ii) *infection* (converting x-men into y-men) acting at a rate jointly proportional to x and to a (spatial) moving average of y :

$$\left.\begin{aligned}\frac{\partial x}{\partial t} &= -\beta\sigma x\tilde{y},\\ \frac{\partial y}{\partial t} &= \beta\sigma x\tilde{y} - \gamma y,\\ \frac{\partial z}{\partial t} &= \gamma y.\end{aligned}\right\} \qquad \qquad (2)$$

Here β and γ are constants; β is an intrinsic infection rate and γ an intrinsic removal rate, and the explicit factor σ in the term $\beta\sigma x\tilde{y}$ is needed to keep β free from density-dependence. The quantity $\tilde{y}$ is obtained from y as follows:

$$\tilde{y}(P, t) = \iint \lambda(PQ)\, y(Q, t)\, dS. \qquad \qquad (3)$$

Here dS is an element of area located at Q, and the non-negative coefficient $\lambda(PQ)$ weights the contributions of the y-men at Q to the infection of susceptibles at P. The integration at (3) is to be extended over the whole plane and by the suitable adjustment of β we can arrange that‡

$$\lambda(r) \geqslant 0; \iint \lambda(PQ)\, dS \equiv 2\pi \int_0^\infty \lambda(r) r\, dr = 1. \qquad \qquad (4)$$

Let us now apply the operation "$\sim$" to the third of equations (2) to get

$$\frac{\partial \tilde{z}}{\partial t} = \gamma\tilde{y},$$

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* For a summary of their work see my paper in the *Third Berkeley Symposium*.

† *Biometrika*, **42**, 126–132.

‡ It is reasonable to require the integral at (3) to converge when y is a constant, and this makes it possible to effect the normalization (4).

and divide this into the first of equations (2). It will then follow that, for a fixed point P of the plane,

$$\frac{dx}{d\tilde{z}} = -\sigma x/\rho \qquad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (5)$$

where $\rho \equiv \gamma/\beta$. Thus, if initially

$$x(P, 0) = 1 - \varepsilon, \qquad y(P, 0) = \varepsilon, \qquad z(P, 0) = 0 \qquad . \quad . \quad . \quad . \qquad (6)$$

(when ε is a function of P) ,we shall have

$$x(P, t) = \{1 - \varepsilon(P)\}\, e^{-\sigma\tilde{z}(P,\, t)/\rho}. \qquad . \quad . \quad . \quad . \quad . \quad . \qquad (7)$$

On combining this with (1) and with the last of equations (2) we get the equation

$$\frac{1}{\gamma}\frac{\partial z}{\partial t} = \varepsilon + (1 - \varepsilon)(1 - e^{-\sigma\tilde{z}/\rho}) - z \qquad . \quad . \quad . \quad . \quad . \qquad (8)$$

which, with the initial condition $z(P, 0) \equiv 0$, is to determine z for all P and t. The other dependent variables x and y will then be determined by (7) and (1).

An adequate treatment of the equation (8) presents some difficulty, even if (as we shall) we choose a very simple initial condition such as

$$\left.\begin{aligned} \varepsilon(P) &\equiv \alpha > 0 \qquad (0P < a), \\ \varepsilon(P) &\equiv 0 \qquad\quad\;\; (0P > a). \end{aligned}\right\} \qquad . \quad . \quad . \quad . \quad . \quad . \qquad (9)$$

Some progress can be made if we assume that the λ-function satisfies the condition,

$$\left.\begin{aligned} \lambda(r) &> 0 \qquad (0 \leqslant r < b), \\ \lambda(r) &\equiv 0 \qquad\quad\; (r \geqslant b). \end{aligned}\right\} \qquad . \quad . \quad . \quad . \quad . \quad . \qquad (10)$$

It is clear from (2) that x decreases and z increases when $t \to \infty$, and both are bounded so that (using (1)) we must have $x \downarrow X$, $y \to Y$ and $z \uparrow Z$, where $X + Y + Z = 1$. Again, (2) shows that $Y \equiv 0$ and so $X \equiv 1 - Z$; thus only $Z(P)$ need be found if we wish to study the ultimate effect of the epidemic. Now, from (8),

$$Z = \varepsilon + (1 - \varepsilon)(1 - e^{-\sigma\tilde{Z}/\rho}) \qquad . \quad . \quad . \quad . \quad . \qquad (11)$$

and

$$\tilde{Z} = \frac{\rho}{\sigma}\log\frac{1}{1 - Z} \qquad (\mathrm{O}P > a). \qquad . \quad . \quad . \quad . \quad . \qquad (12)$$

It is clear that $Z(P) > 0$ whenever $\varepsilon(P) > 0$ and so, in particular, Z is positive when $OP < a$. Suppose *if possible* that $Z(A) = 0$ for some point A of the plane; then $\varepsilon(A) = 0$ and $\tilde{Z}(A) = 0$. But now (10) implies that $Z(P) \equiv 0$ almost everywhere inside the circle centred at A and of radius b, and on continuing this argument in the direction AO we should arrive at a contradiction in finitely many steps. We conclude therefore that $Z(P) > 0$ for all points P; that is, *the effects of the epidemic ultimately extend over the whole plane.*

To make further progress let us *assume* (*this ought to be studied further*) that $Z(P)$ decreases steadily as OP tends to infinity, at least so long as P is sufficiently far removed from the "focus", O, of infection. Then $Z(P) \to \zeta \geqslant 0$ as $OP \to \infty$ and

$$\zeta = 1 - e^{-\sigma\zeta/\rho}. \qquad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (13)$$

Now (13) always has the root $\zeta = 0$; it has a (unique) positive root if and only if $\sigma > \rho$. The quantity

$$\zeta \equiv \lim_{OP \to \infty}\; \lim_{t \to \infty}\; z(P, t) \qquad . \quad . \quad . \quad . \quad . \qquad (14)$$

is obviously of considerable interest; we shall say that there is a *pandemic* when $\zeta > 0$; this will mean that ultimately the fraction of individuals succumbing to the disease will be ζ or greater, even at indefinitely great distances from the focus. It will be convenient to call ζ the *severity* of the pandemic. If we accept the assumption of the ultimate monotony of the Z-function we now have the following "Pandemic Threshold Theorem":

(i) There will be a pandemic if and only if the population density σ *exceeds* the threshold density $\rho \equiv \gamma/\beta$.

(ii) If there is a pandemic then its severity ζ will be the (unique) positive root of the equation (13).

For example, the severity ζ will be 10 per cent. if $\sigma = 1{\cdot}05\rho$ and 90 per cent. if $\sigma = 2{\cdot}56\rho$.

The proof is quite trivial save for the demonstration that $\zeta \neq 0$ when $\sigma > \rho$. This is shown as follows. Suppose $\zeta = 0$ and choose θ so that $\rho/\sigma < \theta < 1$; then from (12) we know that

$$\tilde{Z}(P) \leqslant \theta\, Z(P)$$

for all sufficiently large values of OP, say for $OP \geqslant c$. Iteration now gives

$$\tilde{Z}^n(P) \leqslant \theta^n\, Z(P)$$

provided that $OP \geqslant c + (n-1)\,b$, where $\tilde{Z}^n$ denotes the effect of applying the operation "$\sim$" n times to Z. Choose n so that $\theta^n < \frac{1}{8}$ and then fix it. We now consider the two circles

Γ_1: centre O, radius OP;
Γ_2: centre P, radius nb.

If OP is large enough then their region of overlap will be almost a semicircle of Γ_2 and it will at least include a quadrant of Γ_2; also, in this region of overlap, $Z \geqslant Z(P)$. Thus

$$\tilde{Z}^n(P) \geqslant \tfrac{1}{4}\, Z(P) \text{ for all sufficiently large } OP$$

and yet

$$\tilde{Z}^n(P) < \tfrac{1}{8}\, Z(P) \text{ if } OP \geqslant c + (n-1)\,b,$$

and $Z(P) \neq 0$. The desired contradiction has now been obtained, and it follows that $Z(P)$ must converge to the *positive* root of (13) when $\sigma > \rho$.

The threshold theorem just described presents a close similarity to the threshold theorem of Kermack and McKendrick for epidemics in closed populations. The stochastic equivalent of the older threshold theorem is now well known,* even if it has still to receive an exact formulation, and it is tempting to speculate about the stochastic analogue of the pandemic threshold theorem. In doing so one new feature deserves to be treated with special caution. As soon as a large epidemic has got under way in a closed population it is quite reasonable to expect the deterministic model to yield a good qualitative approximation to its stochastic equivalent. With the "open" population considered here this is no longer true; even in conditions of pandemicity the y-frequency will be small in a critical annular region advancing with the pandemic wave and so stochastic fluctuations in amplitude will continue to be of importance throughout the whole history of the pandemic. At first one may think in terms of a critical annulus of active y-ness surrounding a disc of passive z-ness and expanding into an infinite sea of susceptibles with a velocity which can be calculated (at least on a deterministic basis). A little reflection suggests, however, that the critical annulus will eventually break up into an increasing number of disjoint arcs (because of spatial "fade-out"), and an important question is whether some one of these arcs will escape fade-out altogether.

I have already indicated that this analysis may be applicable to the spreading of rumours, and in conclusion it may be of interest to note as a third example the obvious application to forest fires.

Dr. P. G. Moore: I found Professor Bartlett's paper extremely illuminating in the light that it throws on the periodicity of epidemics, although to savour it to the full I found it necessary to make some study of the previous paper given at the Berkeley symposium. I should like to make a few comments on the relationship between periodicity and community size discussed on p. 50. The mode and the mean of the period given there are derived from (5.7) of the 1955 paper which is

$$f(T) = r(T-1)\,T^{r-1}\,e^{-r(T-1)}$$

where

$$r = \varepsilon\mu/\lambda v$$

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* See the contributions by Bartlett and myself to the discussion following Bailey's recent *Journal of the Royal Statistical Society* paper and to the *Third Berkeley Symposium*. See also P. Whittle, *Biometrika*, **42**, 116–122.

the symbols being defined on p. 52. The mode of this distribution is $1 + 1/\sqrt{r}$ and the mean a little greater. In plotting, as Professor Bartlett has done in Fig. 1, the average period between epidemics against the value of $100/\sqrt{N}$, N being the size of town, we are assuming that $\varepsilon\mu/\lambda v \propto N$ or if we say that $\varepsilon \propto N$ we assume that $\mu/\lambda v$ is constant for all the towns. This would seem to be a strong assumption if we compare, say, an isolated inland town and a seaside resort. It is difficult to see from Fig. 1 that there is a linear relationship, and experiments with other functions, such as $100/N$, give better straight lines so that more information would be needed to establish the relationship conclusively.

The distribution of T, for small r especially, is very skew and not at all symmetrical. There are large variations in the observed periods about the mean period, particularly on the positive side. Any tests employed would have to bear this form of departure from normality in mind. A few specimen calculations give

r	Mean (ξ)	Standard deviation (σ)	$V = \frac{\sigma}{\xi}$
$r = 2$	$= 2\cdot2500$	$= 0\cdot8292$	$= 0\cdot37$
3	$1\cdot9630$	$0\cdot6174$	$0\cdot31$
4	$1\cdot8047$	$0\cdot5049$	$0\cdot28$
6	$1\cdot6291$	$0\cdot3838$	$0\cdot24$

and the coefficients of variation are fairly large by everyday standards. To examine the agreement between theory and observation it would be interesting to compare the observed and theoretical standard deviations of the periods between epidemics. At present there do not seem to be enough data available for such a comparison although the data from Ffestiniog from 1940–55 give a value of V of $0\cdot38$ which is reasonable. However, as this period contains only three epidemics it is clearly too few on which to base any firm conclusions, but as data accumulate various tests could be made to test agreement between theory and observation apart from the mean.

Dr. FINNEY (read in his absence by the Honorary Secretary): How reliable are the figures for notifications? For the last two months there have been many cases in Aberdeen. For several weeks, my daughter's school class was reduced to about 4 children, the absentees being mostly cases of true and German measles. My medical colleagues tell me that differentiation between the two diseases is far from easy, but I know of cases of reputedly true measles in families of at least two members of the medical profession. Yet, in the middle of this period, our local daily paper contained a list of notifications of infectious diseases for the previous week including "Measles: 1". I have not attempted to check for a possible printer's error, but suggest that a *prima facie* case for questioning realiability of records exists.

Professor BARTLETT (in reply): While I am gratified that people have on the whole been rather kind, I think it fair to say that the more theoretical have been somewhat kinder than the medical people. This is as it should be. The actual facts and data and the assumptions made in the theory obviously need very careful consideration from the medical point of view.

Dr. Reid and Colonel Butler raised the question of the definition of an epidemic. I agree that my own definition was quite arbitrary, and no doubt could be improved, but I would reiterate a point I made in my introductory remarks at the beginning of the meeting. I should be the last to deny the importance of the medical aspects in all this work, but one cannot ignore the statistical aspects, and in particular the definition of an epidemic does seem to me to be essentially one that must be put in statistical terms, that is in "crowd" terms. It is not, I think, out of the question that the actual statistical theory will help in suggesting what one does. In the case of simple and closed populations where there is an epidemic, one knows from recent theoretical work that there may be a minor outbreak if the level of susceptibles is below the threshold, or a minor outbreak *or* epidemic if the level is above the threshold, and there may be something in between which one may not know whether to call an epidemic or not. There is an essential arbitrariness about it.

Again on the formula for measles periodicity and community size I hope I did not give a wrong impression in claiming I was establishing very precise results. Whatever the fate of the theory that is being put forward, it has certainly helped me to try to understand what is going on, if only to encourage me to look at the data to see how they conform with the theory. Any data put forward in the paper should be considered on their own merits, quite apart from the theory. Take the existence of a critical size. If one does not accept the theoretical reasons suggested, one has still to explain the phenomenon in terms of some other theory one may wish to put forward.

All this is perhaps a little relevant to some remarks made by Colonel Butler. For example, the question of virulence of an attack in a place where measles has not existed for a long time is quite undeniable; but, coming to less extreme cases, one finds that a certain proportion of the population is attacked and then the epidemic will die down. The percentage of attacked is not determined necessarily because the rest of the population are more immune but because that is the statistical consequence in that sort of population.

I was interested in the detailed map which Dr. Reid displayed; it seemed to me to be something intermediate between the detailed work Mr. Bailey has been interested in and the sort of large-scale work I have been interested in here. Perhaps at some time I might have a closer look at the data.

Again in answer to Colonel Butler, I think I would agree with what Dr. Irwin suggested, that the object of this work was primarily to understand, or to try to understand, what was going on. We have a long way to go, but I submit that study from this angle, as well as from the medical angle, is at any rate of some relevance to the understanding of epidemiological phenomena.

Professor BARTLETT subsequently replied in writing as follows:

To add a little more to my reply to Col. Butler, I hope that he and I will not remain too much at cross-purposes in our approaches to epidemic theory. In my verbal reply I suggested that the data should at least be considered on their own merits, quite apart from any explanatory theory. However, I would not wish one general theoretical point to be ignored—namely, that complex, and certainly not obvious, consequences flow from even the simplest model. This means that observational data often cannot be adequately considered without the investigator having some awareness of these complexities. As a possible example (not perhaps a very good one, but it happens to arise from the discussion), consider the estimates by Dr. Nicol, referred to by Dr. Reid, of the percentage of children in the Isle of Wight ultimately succumbing to measles. In addition to the more obvious questions such as the completeness of notifications, the theoretical concept of critical size seems to me to have some relevance to such investigations; for if semi-isolated populations are below the critical size, the chance of a child being attacked at some stage must (though it is not immediately obvious to what extent) be affected.

There seems to me no incompatibility of such theoretical points with detailed studies of actual epidemic patterns and recurrences. Dr. Reid has noted the more extended and variegated interpretation of simple epidemiological concepts and assumptions that may be necessary before they can be very relevant to real life; and certainly there was no implication that my simple model was complete. To recall a complication noted in my paper, assembly and dispersal in schools is an important aspect contributing to seasonal variation in incidence, and even sometimes disrupting an epidemic and starting it up again after the summer holidays. (Should such an interrupted epidemic count as one epidemic or two?)

At one point in Col. Butler's remarks, he raised another possible complication of "at least a temporary immunity". This kind of hypothesis is also found in the 1928 measles study by Stocks and Karn. That I have not included any such assumption in my own model does not preclude its inclusion later if found necessary, but its importance, even if it has some medical basis, seems limited. It does not appear any longer helpful as an "explanation" of measles' biennial periodicity, nor do the observed attack-rates of 80 per cent. or so in some schools allow the production of such immunes to be anything like the three or four to one ratio of new cases that Stocks and Karn suggested at first. In a letter to me in 1954, Dr. Stocks modified the hypothesis to one of a differential immunization, with less effect in cases of close contact and heavy infection; in this more complex form, the hypothesis will unfortunately be even more difficult to verify.

The problem of incomplete notifications was mentioned both by Dr. Reid and by Dr. Finney (and also, as noted in my paper, by Dr. Hope Simpson, who I regret was unable to be present at the meeting). Some quantitative adjustment for this might be attempted, but it could hardly disturb my main thesis. It would have the effect of making the critical size estimated from actual data somewhat smaller, to allow for the chance of apparent "fade-out" being a consequence of incomplete notifications. The rather rudimentary quantitative estimates given in my paper are, however, in my opinion still too crude for such adjustment yet to be worth while.

Dr. Moore noted that the information available was hardly sufficient to establish my proposed linear relation between measles periodicity and $1/\sqrt{N}$. I quite agree. But bearing in mind that I was using the simplest assumptions, I consider the absence of contradiction encouraging enough. I would point out that my relation (for towns below the critical size) was augmented by the critical size concept, and the broad compatibility of theory and observation over the whole range of town size is by no means trivial.

Mr. David Kendall's (amplified) remarks represent a valuable contribution to the spatial theory of epidemics, and I hope its mathematical precision (coming at the end of a paper which, in spite of its theoretical flavour, was largely concerned with observed phenomena) will not be overlooked by serious students of epidemic theory. Mr. Kendall has given the spatial generalization of his own exact solution (see his Berkeley paper) of the Kermack and McKendrick model of an epidemic in a closed population. Perhaps, regarding the associated stochastic problem raised at the end of his contribution, I might draw attention to the relevance of equation (9.5), and the immediately ensuing equations, in my own Berkeley paper.

Finally, I should like to record my grateful thanks to all the speakers for their various contributions.

As a result of the ballot taken during the meeting, the candidates named below were elected Fellows of the Society:

Jane Frances Arnold.
Henry Moore Baker.
William John Fox Benton.
Derek Thomas Beeston.
Allan Birnbaum.
Frederick Ernest Bonner.
Robert George Bradley.
Leslie Brown.
Maude Caudwell.
William Horace Chapman.
John Welham Clarke.
Derek Edward Cook.
Reginald James Williams Crabbe.
Oliver Douglas Cumming.
Paul Omodara-Adeniyi Dada.
Dennis John Gerald Farlie.
Peter Raphael Francis.
Dennis Frank Gilley.
Joseph Hamilton-Jones.
Philip Horace Hammon.
Carola Herbert.
Percy Francis Hooker.
David Frank Kerridge.
Peter Thomas McIntosh.
Pinhas Paul Naor.
Emmanuel Noi Omaboe.
Alan Rowland Rogers.
Brian Frederick Sagar.
Norman Havelock Shears.
Arthur Gribble Simons.
Joseph Andrew George Taylor.
Sridhar Tripathy.
Charles Frederick Trustam.
Francis Herbest Wales.
Leonard Ainslie Williams.

SHIP WASTAGE RATES

By J. R. PARKINSON

Introduction

THE primary object of this study is to estimate wastage rates of mechanically-propelled merchant ships in the present century. Section I deals with the period 1885–1914; Section II with the periods 1922–38 and 1949–53. Some of the factors affecting the service life of ships are discussed in Section III, and in Section IV the bearing of the figures on trade cycle theory and the demand for ships from United Kingdom shipyards is briefly indicated.

The most complete study of wastage rates is that of Einarsen (1938), but his investigations were confined to the Norwegian fleet and need to be extended to other countries. For the United Kingdom there have been a number of scattered estimates of the life of ships on the British register but they vary considerably in content and reliability. In 1914 Robertson (1914, p. 166) quoted the opinion of a shipowner that the life of a ship on the British register was 20 years though his own estimate suggested that 17 years was a better figure, and Allen (1933) also put the normal life at about 20 years. Some doubt has been cast on the validity of these assumptions by Jones (1955, p. 45) who appears to imply in his calculations a normal life of about 50 years.

The divergence between these estimates and the paucity of information about other countries and the world merchant fleet suggest that a more detailed examination of the facts is desirable. In the practical field a knowledge of the longevity of ships is of use in assessing market prospects, estimating supplies of steel scrap and drawing up investment requirements; and in economic theory it is relevant to studies of the trade cycle, economic growth and demand.

There are considerable difficulties in measuring wastage rates of merchant ships. In the first place, the available data are incomplete and not always in a form suitable for analysis, and secondly, considerations of time limit the amount of detailed work which can be undertaken. There are also theoretical difficulties. Wastage rates are affected by a large number of factors including among others the period considered, the size of vessels, the country of ownership and the type of vessels. Any attempt to isolate all these variables would lead to the consideration of classes too small to yield reliable results. But practical considerations in any case severely limit the amount of subdivision of data which can be undertaken in order to reduce heterogeneity to a minimum. Inevitably we are faced with the examination of groups of vessels aggregated by their gross tonnage and distinguished only partially by age, ownership, and period of operation. Such groups have other features in common: the vessels included, for example, are for the most part constructed of steel and all are mechanically propelled. But their differences are also considerable. Perhaps the major defect of the subdivisions is that it has not been possible to distinguish between the main types of vessels. Passenger liners, dry-cargo ships, tankers, fishing boats etc. are all grouped together, although there is reason to suppose that their wastage rates vary as well as does the proportion they represent of the various tonnage groups considered. This defect should not, however, be over-emphasized. It is likely to be most important for tanker and non-tanker vessels. The former have had a shorter life than the latter and the proportion of tanker tonnage in the world fleet has increased from a very small figure at the turn of the century to about one-quarter at the present time. But if we take the Inland Revenue depreciation allowances of 4 per cent. and 5 per cent. per annum respectively for non-tanker and tanker vessels as an indication of their relative longevity, the change in the composition of the world fleet in the present century would have had the effect of reducing the average life from 25 years to only 23½ years. This difference is rather small compared with differences which are noticeable between wastage rates in various age groups or in various economic conditions and suggests that, in spite of the heterogeneous nature of the grouping, the main factors affecting the longevity of ships are likely to emerge in recognizable form.

It should be emphasized, however, that the estimates of the longevity of ships presented here are not to be regarded as either complete or definitive but as a step to replacing informed opinion by statistical calculation. If the estimates serve to stimulate further enquiry and to demonstrate that the statistics of the world fleet collected by Lloyd's Register can be used to throw light on some economic problems they will have served their purpose.

Wastage in the world fleet may be taken to result either from marine casualty or scrapping, as in practice transfers from naval to merchant ownership and vice versa are of negligible importance. In estimating the life of a ship on the register of a particular country, however, account must be taken of transfers of ownership arising from the sale or purchase of second-hand ships between countries. The life of a ship on the U.K. register, for example, is much influenced by the practice of disposing of ageing tonnage to other countries. The statistics which are available for estimating the life of ships on the registers of individual countries do not always enable us to distinguish between ships which have been lost or scrapped and ships which have been transferred to (or from) other countries and it is necessary to bear in mind the content of the figures used in the various calculations in this paper.

The dependence of wastage rates on age requires some emphasis for the neglect of this factor can give rise to serious error. Wastage rates are, in general, an increasing function of age and unless there is good reason for supposing that the age distribution of a fleet is normal in some sense (for example, that it has been arrived at through an invariable rate of new construction exactly making good wastage over a long period), wastage rates cannot be measured at all satisfactorily by relating losses to the total strength of the fleet irrespective of the age distributions involved. No such assumptions about normality can be made during the period considered, for it was for the most part a period of expansion in the world merchant fleets interrupted, however, by a contraction in the 1930's and distorted by the incidence of two world wars. In the present study, therefore, it has been necessary to estimate wastage rates within comparatively small age groups and calculate the expectation of life for ships along actuarial lines.

TABLE 1

World Fleet of Steamships and Motorships at Various Dates

100 Gross Tons and Above				*Million Gross Tons*	
1886	10	1911	39	1939	68
1891	14	1921	59	1947	83*
1896	18	1926	63	1954	97*
1901	24	1931	69		
1906	32	1936	64		

* Including the U.S. reserve fleet of 7½ million gross tons in June 1947 and 14 million gross tons in 1954.

Section I—Wastage Rates 1895–1914

We may start with the 25-year period from 1885 to the outbreak of the first world war concentrating attention exclusively on steamships. Most of the basic information required for estimating wastage rates is contained in the Lloyd's Registers of the period but it is not in a form suitable for easy statistical analysis. So far as I am aware no estimates are available for this period of the age distribution of the world merchant fleet. Since the age distribution must be known for some years before wastage rates according to age can be calculated, it was decided to estimate the age distribution in 1914 and in 1889 by taking a sample of Lloyd's Registers. The latter year was chosen because it is the first year of publication of the Universal Register (covering all ships) corresponding to the age groupings adopted in the analysis. There were some 10,000 steamships on the register in 1889 and some 24,000 in 1914. In both years the tonnage of the ships whose number ended in 25, 50, 75 or 100 was extracted from the register and grouped in 5-year intervals of age. The percentage age distribution of tonnage in the sample was then calculated and applied to the total tonnage on the register. The estimated age distributions of the world steamship fleets in 1914 and 1889 are shown respectively in Tables 2 and 3 below; they are, of course, subject to a considerable margin of error.

For the period 1885–1914 figures of the tonnage of steamships launched in the world are contained in the statistical section of Lloyd's Register. From these we can infer what would have been the age composition of the fleet in 1914 if no ships had been lost or broken up. By comparing these figures with the estimated age composition of the fleet in 1914 we can arrive at an estimate of the percentage surviving in the various age groups up to 30 years of age. For vessels over this age the method can be extended, using data from the estimated age distribution of the fleet in 1889.

TABLE 2

Tonnage Launched in 1885–1914 *and Survival in* 1914

Year of build	1910–14	1905–09	1900–04	1895–99	1890–94	1885–98
Age group in 1914 . . .	0–5	5–10	10–15	15–20	20–25	25–30
Tonnage surviving in 1914 (mill. g.r.t.)	12·31	9·75	8·79	6·02	3·49	2·27
Tonnage launched (mill. g.r.t.) .	13·16	11·10	10·22	7·45	5·25	3·18
Percentage surviving . .	93·5	87·9	85·9	80·7	66·5	71·3
Percentage lost . . .	6·5	12·1	14·1	19·3	33·5	28·7

Various irregularities in the figures are evident. The percentage loss of tonnage built in the years 1910–14 appears too high. The reason for this is almost certainly the omission from the register of some tonnage launched in 1914. This omission is due to the fact that Lloyd's Register is prepared over a period of some months and represents only approximately the size and composition of merchant fleets at the middle of the year; tonnage known to have been launched in the early part of the year is included and some tonnage is also included in anticipation of launching but part of the tonnage actually launched during the latter part of the year is not recorded. The second apparent irregularity is the rather high percentage loss (in relation to the other figures) of tonnage built in the years 1890–94; it is impossible to say whether this is a reflection of fact or the product of the statistical deficiencies of the methods employed.

Strictly speaking, each age group of the table should be considered as a separate statistical entity for we are not tracing a single block of tonnage through the whole of its life but various blocks through part of their lives. If there had been no change in the types of ships built during the period, no change in the composition of output between types and no change in operating conditions the table would give a consistent picture of wastage rates (within the limits of the sampling procedure) applicable to a notionally characteristic vessel. Such rigorous conditions do not, of course, hold and any attempt to infer characteristic wastage rates from the table rests on the assumption that there was sufficient similarity between the variable factors during the period to justify generalization. Without further statistical evidence no generalization can be made with assurance, but the progression of the figures is suggestive of wastage rates averaging about 1 per cent. per annum of the fleet for tonnage under 30 years of age. The assumption of a steady loss rate may appear to be at variance with the usual tendency for wastage rates to increase with age, but the main cause of wastage during the period (in a fleet in which few vessels were approaching the end of their economic life) was marine casualty, which may have been fairly even in its incidence. Between 1902 and 1914, about three-quarters of the total loss of tonnage of all kinds was due to marine casualty, and more detailed figures which are available for the later period 1929–38 do not suggest that there was a very marked variation in the incidence of marine casualty on vessels up to 20 years of age in the world fleet.

It is clear that if a more complete picture of the life of a ship is to be given, wastage rates must be established for tonnage aged 30 years and over. Although a complete age distribution of tonnage in 1914 can be obtained from our sample, figures of the tonnage of steamships built in the world before 1885 are not readily available. This difficulty can be overcome by considering the age distribution of the fleet in 1889 as determined from the sample of Lloyd's Register for that year.

TABLE 3

Age Distribution of the World Fleet of Steamships in 1889 *and the Tonnage Surviving in* 1914

Year built	1880–84	1875–79	1870–74	1865–69	1860–64
Age in 1889	5–10	10–15	15–20	20–25	25–30
Age in 1914	30–35	35–40	40–45	45–50	50–55
Estimated tonnage in 1889 (mill. g.r.t.)	4·10	1·64	2·11	0·76	0·57
Estimated tonnage in 1914 (mill. g.r.t.)	1·67	0·53	0·39	0·07	0·07
Percentage lost between 1889 and 1914	59	68	81	91	88
Estimated percentage lost between year built and 1914	62	72	84	93	91

In estimating the percentage of tonnage lost between the date of build and 1914 it was assumed that 1 per cent. of tonnage was lost in each year until the age of 27½ years. The above calculations are extremely rough and even more subject to reservation than those in the previous table, but there can be little doubt that wastage rates of vessels more than 30 years old increased very rapidly in the next five years and although wastage may have proceeded more slowly thereafter relatively few vessels survived 40 years of service.

Broadly speaking, a third of the tonnage of ships launched in a particular year appears to have been lost before 30 years of service, a further third before 35 years of service and nearly all the remaining tonnage before 50 years. More detailed calculations suggest that the average life of ships during the period we have been considering was of the order of 30 years, though the result is naturally rather uncertain. It appears that ships built in the latter part of the nineteenth century had a long period of useful service in front of them. The durability of the new iron and steel ships must have contrasted with the sailing ships built in the 1880's north of Yarmouth, which Lloyd's would not classify for more than ten years (Pollard, 1950–51, p. 77), or with those first installed with steam engines, which were often lucky to complete their maiden voyage.

There is little reason to believe that the physical longevity of ships on the U.K. register was very different from that of the world as a whole (including the U.K. fleet). About half the world's ships were on the U.K. register and about 70 per cent. of the steamships built between 1885 and 1914 came from British shipyards. Quantitatively, the most important deletion of ships from the U.K. register was due to sales of ships abroad. From 1885–1914 these averaged approximately 2½ per cent. of the total strength of the British fleet compared with approximately 1¾ per cent. for losses due to marine casualty and scrapping. Tonnage sold abroad appears to have been characteristically 15–25 years of age and the proportion of tonnage over 25 years of age remaining on the U.K. register was small.

Section II—Wastage Rates Since the 1914–18 *War*

The information on wastage rates in the inter-war period is more complete than that before 1914 and in the 1930's much more detailed calculations of the importance of various kinds of wastage can be attempted. From 1922 to 1939 age distributions of the tonnage owned by a number of countries are available in 5-year age groups up to age 25 years. The use which can be made of this information is limited since it is in the age groups from 25 years onwards that wastage is most pronounced and it is at this point that the data fail. Nevertheless, the material serves for approximate calculations of the life of a ship and supplementary estimates can be made.

A measure of wastage rates can be obtained by comparing the tonnage in a 5-year age group recorded in a particular year with the tonnage in the next higher age group five years later, and so calculating the tonnage surviving the intervening years. In this way a large number of wastage rates can be calculated over a series of years for tonnages in different age groups. To extend the calculation for ships 20 and more years old it is necessary to combine the groups 20–25 years and over 25 years and compare the tonnage in these groups with that recorded five years later in the over-25 years group. The results of carrying out these calculations are shown in Table 4.

It is noticeable that the tonnage in the 5–10 year group is sometimes greater than that recorded in the 0–5 year group five years previously. As we have seen, the explanation of this is incomplete

TABLE 4

Percentage of Tonnage Surviving Five Years (World Fleet of Steam and Motorships 100 Gross Ttons and over)

	Initially in Age Group				
Years	0–5	5–10	10–15	15–20	*Over* 20
1922–27 . . .	91·7	93·6	96·6	91·5	73·3
1923–28 . . .	94·8	88·5	95·7	92·3	74·2
1924–29 . . .	102·5	89·2	96·8	92·0	75·3
1925–30 . . .	103·4	90·9	96·5	92·7	75·9
1926–31 . . .	101·1	92·9	96·4	91·6	76·7
1927–32 . . .	106·9	93·2	94·5	90·9	75·6
1928–33 . . .	105·9	93·4	91·8	89·9	71·4
1929–34 . . .	106·2	95·7	88·7	84·5	65·6
1930–35 . . .	105·9	96·6	89·6	82·6	64·0
1931–36 . . .	101·1	96·7	91·7	82·5	64·2
1932–37 . . .	99·2	96·9	92·0	85·3	66·2
1933–38 . . .	99·6	97·2	93·0	86·7	70·7
1934–39 . . .	101·5	97·9	93·0	90·3	77·9
Average for Years Beginning					
1922–25 . . .	98·1	90·6	96·4	92·1	74·7
1926–28 . . .	104·6	93·2	94·2	90·8	74·6
1929–31 . . .	104·2	96·3	90·0	83·2	64·6
1932–34 . . .	100·1	97·3	92·7	87·4	71·6
1922–34 . . .	101·5	94·0	93·6	88·7	71·6

recording of new tonnage in Lloyd's Register. The rather high wastage rates in the 0–5 group in the first two periods shown in the table may be due to scrapping (probably by the United States) of war-built tonnage; and the same explanation may hold for the 5–10 age group in later periods. The figures reveal the increase in wastage rates during the early slump years of the 1930's and a subsequent reduction when the demand for ships showed some recovery in later years.

It will be shown later that in the 1930's (and presumably in much of the inter-war period) very few vessels were scrapped before the age of 20 years and marine casualties were fairly light in the first few years of operation. We shall not go very far wrong in assuming that wastage rates were negligible in the first few years of life and that almost all tonnage survived until the middle of the 5–10 group, i.e. until an age of about 7½ years. The table shows that for the period as a whole about 94 per cent. of tonnage survived a further five years and thereafter a progressively smaller percentage survived each subsequent 5-year period. Of each 100 gross tons initially on the register about 94 tons remained at 12½ years, about 88 at 17½ years and about 78 at age 22½. Of the tonnage over 20 years of age about 5·7 per cent. left the register each year. If the tonnage over 20 years of age were evenly distributed in age up to a point at which all tonnage of a certain age was removed from the register this would imply an average life of 18 years after reaching age 20. On this assumption the average life of a ship during the inter-war period would work out at about 33 years. It is likely, however, that this figure somewhat over-states the average life of ships during this period as the tonnage of vessels over 20 years may have been rather more heavily concentrated in the lower regions of the over-20 age group than we assumed. This is, of course, to be expected with a fleet that was increasing 20 or more years earlier. We are probably safe, however, in concluding that the longevity of ships in the world fleet was not vastly different from that of the period before 1914. Tonnage broken up, however, was quantitatively a much more important cause of wastage than in the period before; on the other hand, marine casualties were much reduced as is brought out by the fact that a greater percentage of tonnage survived in the lower age groups.

Similar calculations can be carried out for the U.K. fleet; but in this case part of the reduction in fleet strength is due to the sale of second-hand ships abroad.

TABLE 5

Percentage of Tonnage Surviving Five Years

(U.K. Fleet of Steam and Motor Ships 100 Gross Tons and over)

Average for Years Beginning	*Initially in Age Group* 0–5	5–10	10–15	15–20	*Over* 20
1922–25 . . .	98·5	88·3	87·9	69·8	47·1
1926–28 . . .	100·8	91·3	85·2	75·1	42·3
1929–31 . . .	98·8	92·5	80·8	62·0	39·4
1932–34 . . .	98·7	92·2	79·1	69·6	49·2
1922–34 . . .	99·2	90·9	83·6	69·2	44·7

The table is constructed in the same way as that for world tonnage and the final line covers wastage in the whole of the period 1922–39. On the average about 90 per cent. of tonnage was still on the register at age 12½ years, about 76 per cent. at age 12½ years and about 52 per cent. at age 22½ years, while tonnage over 20 years survived about a further 9 years. On the average ships remained on the register about 25 years—probably somewhat longer than in the pre-1914 era.

A few similar calculations can be carried out for the period 1947–54 but the abnormal nature of the period and the statistical reservations which have to be made make the figures extremely unreliable. It is clear, however, that the working life of ships was somewhat extended. For the world fleet, ships over 20 years of age had probably about another 20 years of life ahead and wastage rates in the earlier age groups were also lighter. In the period 1947–54 the average time of a vessel on the U.K. register was also greater than in earlier periods, vessels over 20 years of age being retained for a further 12 years compared with 9 years in the inter-war period, but in the period 1949–54 the inter-war figures again appear to hold. The tonnage scrapped in 1953 and 1954 was much bigger than in previous years and the progressive modification of regulations governing the sale of second-hand vessels facilitated a return to the pre-war practice of disposing of ageing tonnage abroad.

We may turn now to a more detailed examination of the figures for the periods 1929–38 and 1949–53 using the detailed lists of ships lost and broken up prepared by Lloyd's Register each quarter. From 1929 onwards the date of construction of ships lost and broken up has been given in the returns. They are, however, incomplete in two respects: the dates of construction of a few ships are omitted in the quarterly returns, presumably because they are not known, and from 1929 to 1938 the date of tonnage broken up recorded in a yearly supplement to the quarterly returns is not given. The first of these omissions is of no quantitative importance but the second leads to a slight understatement of scrapping rates in an age analysis.

By grouping the tonnage of ships lost and broken up into the same age groups as are used for age distribution of fleets published by Lloyd's Register each year, various wastage rates can be calculated.

TABLE 6

Marine Casualty Rates

(Percentage of Tonnage Lost Per Year)

	Age 0–5	5–10	10–15	15–20	20–25	25 *and Over*
Ownership—			*Average* 1929–38			
U.K.	·13	·30	·34	·49	·51	1·01
U.S. (1) . . .	·26	·51	·26	·15	·49	·66
Other (2) . . .	·38	·45	·54	·68	·92	1·35
World . . .	·29	·40	·41	·49	·75	1·25
			Average 1949–53			
Other (2) . . .	·13	·35	·25	·43	·38	·88
World (3) . . .	·14	·19	·19	·33	·45	·92

(1) The figures for the U.S. are included for the sake of completeness but the irregular pattern of the figures suggests that no reliable conclusions can be drawn from them. Tonnage on the Great Lakes is excluded throughout the table.

(2) "Other" signifies of neither U.K. nor U.S. flag.

(3) The total for the world is, of course, affected by the inclusion of tonnage in the U.S. reserve fleet.

The first conclusion that emerges from the figures is that there has been a striking reduction in marine casualties. These were lighter in the inter-war period than before 1914 and a further significant reduction has occurred in the operation of the post-war fleet. Whereas before 1914 marine casualty rates of the order of 1 per cent. per annum appear to have been usual even in the lower age groups, marine casualty rates reached this level only for ships over 25 years of age. For the world fleet a reduction in marine casualty rates appears to have occurred in every age group and the same applies to tonnage sailing under flags of other nations than the United Kingdom and the United States. It is possible that casualty rates in the higher age groups understate the liability of tonnage to marine risks; it would be reasonable to expect that older tonnage is more frequently laid up than are more serviceable vessels.

It would be natural to assume that the reduction in marine casualties which has taken place is due to improvements in design and construction and better navigational aids. No doubt all these factors have contributed to greater safety at sea. But an important factor also may be an increase in the size of ships; smaller vessels appear to be more susceptible to marine casualty than larger ones; the main danger to the largest vessels is that of fire. This self-same factor may also go some way to explaining the lighter incidence of marine casualty in the U.K. and U.S. fleets before the war, though they may also have been better found and better manned.

Marine casualties are still a factor limiting the average age of ships but the major consideration is the age at which a ship is no longer economic in service and has to be scrapped. For 1929–33 and 1949–53 the percentages of tonnage scrapped in various age groups have been calculated and are shown in Table 7. Too much importance should not be attached to the details of the table

TABLE 7

Percentage of Tonnage Scrapped in Certain Years

(1) *Non-U.K., Non-U.S. Flag*

	Age Group							
Year	10–15	15–20	20–25	25–30	30–35	35–40	40–45	45–50
1929 . . .	·1	—	1·4	4·6	2·0	3·8	9·6	5·7
1930 . . .	—	—	·3	1·6	2·2	3·5	8·3	7·6
1931 . . .	·1	·3	3·3	2·5	3·6	4·9	7·0	1·6
1932 . . .	·4	1·8	2·7	5·7	10·8	6·5	3·9	4·0
1933 . . .	1·2	2·4	6·0	9·7	18·7	15·9	7·2	8·6
1934 . . .	·4	1·4	3·7	8·9	12·2	12·6	11·9	15·7
1935 . . .	·1	·5	2·2	2·4	6·4	8·8	4·9	8·8
1936 . . .	—	·5	1·7	1·3	5·0	4·4	1·7	8·8
1937 . . .	—	·1	·8	1·0	2·1	3·0	1·9	3·8
1938 . . .	—	—	·1	1·2	2·2	1·4	·9	·5
1949 . . .	—	—	·6	·4	·8	1·4	1·5	3·5
1950 . . .	—	·6	—	3·4	2·3	4·5	5·4	9·1
1951 . . .	·9	—	—	·4	1·3	2·2	2·7	12·0
1952 . . .	—	—	—	1·3	3·2	1·8	2·4	13·9
1953 . . .	—	—	·7	1·9	4·7	9·1	10·5	4·3

(2) *U.K. Flag*

	Age Group				
Year	10–15	15–20	20–25	25–30	30–40
1929 . . .	—	·3	1·1	6·1	23·5
1930 . . .	—	·7	4·1	7·3	18·9
1931 . . .	·3	·5	3·0	10·0	24·8
1932 . . .	—	2·8	5·6	15·3	24·6
1933 . . .	·4	2·2	10·1	25·0	23·6
1934 . . .	1·1	2·8	10·1	14·3	17·8
1935 . . .	2·2	2·0	4·6	13·9	21·1
1936 . . .	2·5	2·0	4·7	13·3	21·1
1937 . . .	—	1·2	1·8	6·4	14·9
1938 . . .	—	·4	2·1	4·9	7·9
1949 . . .	—	—	·1	9·4	6·9
1950 . . .	—	1·0	·7	1·4	19·8
1951 . . .	—	—	·4	2·0	4·9
1952 . . .	—	·1	1·0	3·6	18·1
1953 . . .	·2	1·9	·7	7·5	37·2

because the exclusion or inclusion of a single vessel in some of the age groups can greatly affect the percentages recorded in a single year. Figures for age groups over 25 years are particularly open to doubt because it has been necessary to estimate the tonnage existing in these age groups from a series of samples of Lloyd's Register. Samples were taken for the years 1929, 1933, and 1938 and each of the years 1940 to 1954 by extracting all the ships of 25 years or more from every fortieth page of the register and these were used to calculate age distributions. The method of sampling was not the best that could have been selected as in the alphabetical arrangement of ships in the register, ships of similar name, and often of similar type in the same ownership, tend to occur together. No samples were taken for intermediate years between 1929 and 1933 and 1933 and 1938, but an estimated age distribution was filled out for these years by proportionate interpolation.

Virtually no vessels of less than 10 years of age were scrapped and only very small tonnages occur in the over-40 years class for the U.K. and in the over-50 years class for other countries. The table can be regarded, therefore, as giving a fairly complete picture.

It is clear that the age at which vessels are scrapped is extremely variable. In any year vessels of vastly different age are scrapped and the frequency of scrapping within even a single age group varies greatly from year to year depending on economic circumstances. In addition, there may be long-term trends leading to an extension of the service life of a vessel, though these have probably been less important since the universal use of steel ships than when sailing vessels were giving way to those powered by steam.

The frequency of scrapping is low for vessels less than 25 years old, though in slump conditions inroads may be made into vessels of 20–25 years, and some in earlier age groups may also be broken up. On the other hand, there are vessels on the world register with a century of service behind them. Some vessels, mainly those employed in sheltered waters, have a very long life; in examining the registers it was noticeable that dredgers of considerable age were still in service. On the U.K. register fishing boats figure amongst the oldest tonnage but the reason for their retention is economic since their activities are not confined to coastal or inland water. All these vessels are, however, small in size and their inclusion is unlikely to affect the results of the analysis.

We may regard 1929 as the last year of prosperity before the slump began to have its reactions on the scrapping of tonnage. At that time a comparatively small percentage of vessels not sailing under the U.S. or U.K. flags was scrapped in all but the highest age groups. The figures suggest that (excluding marine casualty) the service life of these ships was about 40 years at this time. The older tonnage, however, was probably much less active than that in the lower age groups and a proportion of it was probably out of commission preparatory to being broken up. Moreover, it is noticeable that scrapping rates in the 40–50 age groups often fall below those of earlier age groups. In part this may reflect incomplete reporting of tonnage scrapped in the less well documented parts of the globe, though it may also reflect the continued survival of particular types of vessel with age characteristics different from the average.

Scrapping rates show a decrease rather than an increase in 1930 and they increased only moderately in 1931. By 1932, however, scrapping was well under way and applied with increased frequency to the lower age groups. Had the rates remained as they were in 1933 or 1934 very few vessels would have survived age 30. But these were years in which hardly any tonnage new or old could be made to pay. As trade improved scrapping subsided and by 1937 few vessels were potentially so unprofitable as to be fit only for scrap.

Post-war figures again reflect a scarcity of tonnage and the need to keep even the oldest vessels at work, but in 1952 and 1953 the tide was turning and in 1955 the service life of a ship (neglecting marine casualty) was probably reduced.

Figures for the United Kingdom show a similar progression of events but, since in the inter-war period U.K. ships tended to be sold abroad rather than broken up, scrapping rates set only an upper limit to the life of a ship on the U.K. register. As might be expected, the U.K. owners show a greater readiness to scrap older vessels than do their competitors overseas. More detailed figures than those shown in the table suggest that it is exceptional for U.K. owners to operate vessels more than 35 years old and, as we have seen earlier, there is a tendency not to employ vessels more than 25 years of age.

A similar table for the United States has been prepared but it is not included as the data are insufficient to enable a comprehensive picture to be presented. The main point of interest was the comparatively high rate of scrapping of war-built tonnage of 10–15 years in 1929–31.

Section III—Factors Affecting the Service Life of Ships

The first conclusion that emerges from a general survey of scrapping rates is that the service life of a ship is not rigorously determined by factors of a technical nature governing structural strength. Ships are built with a generous reserve of strength and furnished with engines designed to give many years of service. There are many instances of ships of riveted as well as welded construction becoming a total loss because of brittle fracture but the onset of this is unpredictable and cannot be said to put any definite age limitation on the use of the structure in the way that appears to be true for the serviceable life of certain types of aircraft. Deterioration of a ship's structure and engines is likely to be a slow though continuous process and for a long period moderate expenditure on maintenance and repair will serve to maintain efficiency.

The phenomenon most nearly akin to predictable structural failure is that of corrosion in oil tankers due to the combined effects of oil and sea-water on the metal. Some tankers have become unserviceable from this effect in as little as 15 years and the damage has been so great as to be beyond economic repair. But the amount of corrosion occurring depends on a number of factors and it is common for tankers to remain in service long after 15 years. In 1955, for example, 2½ million gross tons of tankers were over 20 years old. With the development of new means to protect the steel structure, the life of tankers is less likely to be determined by the thickness of plate surviving corrosive action and it may in future approximate more closely to that of non-tankers.

We should not look, therefore, for an explanation of the service life of ships primarily in terms of technical considerations but regard these as finding their expression in the economics of shipping operation. The decision to scrap a vessel will depend very greatly on anticipated movements of freight rates and the rising trend of repair costs with increasing age. As is well known, freight rates fluctuate widely and are more variable in the short term than costs of operation or for that matter costs of new construction. A sudden increase in the demand for tonnage at a time when new building cannot be increased rapidly is likely, therefore, to give rise to some postponement in the scrapping of those types of tonnage in demand, almost irrespective of age. A downward movement of freight rates, whether due to a fall in demand for shipping space or new building, is likely to accelerate scrapping of older tonnage. If freight rates and operation costs such as seamen's wages and the price of fuel are constant the decision to scrap an older ship is likely to depend very largely on the need for repairs. Periodic surveys of ships are carried out at 4-yearly intervals and considerable expenditure may be necessary at these times in order to conform to the classification mark, but we may reasonably suppose that repair costs will not be a serious factor for most ships until, say, 20–25 years of age. Certainly the figures suggest that ships below this age group are not scrapped at all readily in normal circumstances, which we should expect to occur if repair costs were heavy. At some later date, however, the mounting bill for repairs will become critical and will make the continued employment of the vessel uneconomic at the ruling level of freight rates.

In the long run the level of freights will depend on the capital costs and operating costs of new tonnage. The greater efficiency of new tonnage will be a factor tending to reduce operation costs and freight levels and if the improvement in efficiency is sufficiently great it will accelerate the scrapping of older obsolescent vessels and their replacement with new built tonnage.

The factors governing the decision to replace old tonnage with new are likely to be more complex than those governing the decision to scrap an old vessel. The old tonnage will bear heavier repair costs than the new and will be less efficient in operation; but it will be economical to change over only if the prime costs of operation per ton of freight carried by the older vessel exceed the prime costs of a new vessel by a sufficient margin to cover the capital costs involved in new construction.

This is unlikely to occur very early in the life of a vessel because improvements in the efficiency of operation of ships tend to be gradual and capital costs are high in relation to running costs. The percentage of total running costs represented by depreciation, interest on capital and insurance for a new vessel is a very variable factor ranging from perhaps 30 to 50 per cent. Only very large savings in running costs would justify scrapping an old vessel and incurring capital costs of this order by the purchase of a new one.

Increases in efficiency of operation have arisen in a number of ways. There have been continuing gains in fuel economy resulting from the adoptions of new types of engines and improvements of existing types; and the reduced amount of space required for fuel together with reductions in the engine weight required for a given horsepower have had the indirect effect of increasing the carrying capacity of vessels as well as reducing the number of men required to work the engines. It is difficult to form a reliable opinion of the quantitative importance of these savings in relation to operating costs in various employments. But one particular comparison of the operating costs of steamers built in 1924 and 1934 made by Galbraith Pembroke & Co. (1934, p. 411), suggests that it was likely to amount to about two-thirds of the interest and depreciation charges on a new ship.

Progress in marine engineering at this period of time may have been exceptionally rapid but the figures suggest that after about 20 years' operation many vessels in U.K. service are likely to be approaching obsolescence and the expectation of higher repair charges may result in their being withdrawn from service or sold abroad where economic use may still be found for them.

Improvements in engine efficiency, hull efficiency and similar developments in marine engineering and naval architecture are likely to be renforced in their effects by the development of new types of ships more suited to the changing needs of trade. Specialized ore carriers, sugar carriers and the like may be more economical in use than the general purpose types which preceded them.

In some cases an increase in the size of a vessel may lead to great economy in operation and older, smaller types may become uneconomical for certain trades even if no account is taken of capital charges. This is well illustrated in the design of tankers. There has been a marked increase in the size of tankers constructed over the years. In the 1930's a typical tanker was of about 6,000 gross tons; those constructed in post-war years have been about 13,000 gross tons and many larger ones have been required. As a general rule the larger a tanker the smaller its operating costs per ton mile. Figures prepared by Robinson *et al.* (1948) and figures given in the *Financial Times* (1956) of the comparative cost of operating tankers of 13,400 deadweight tons and 34,500 deadweight tons and 32,000 deadweight tons and 85,000 deadweight tons respectively suggest very strongly that the prime cost of operating the smaller size of tanker on a voyage of 6,000 miles was in each case very close to the total cost (prime cost and capital cost) of operating the larger size.

The incidence of obsolescence will, of course, affect some types and employment of vessels much more than others. It is likely to be of least importance for vessels which habitually spend a high proportion of their time in port for in these cases capital charges will represent an unusually high proportion of total costs, but for many vessels obsolescence is likely to be an increasingly important factor in deciding to scrap a vessel after a period of service of 20 years or more.

Section IV—Wastage and the Demand for New Ships

We have seen that it is unrealistic to picture ships as having a definite span of life governed by structural considerations. The life of ships cannot be considered in isolation from economic circumstances or technical progress. Low freight rates, rapid technical progress (leading to economy in operation) and low interest rates (reducing the proportion of capital to running costs) will all tend to reduce the working life of a ship.

The relationship between the working life of a ship and the demand for replacement tonnage is extremely complex. There is no automatic connection between the two. We have already noted that during a slump ships will tend to be scrapped at an earlier age than when freight rates are high. Wastage is thus accelerated during the depression when new building is at a low ebb and retarded during prosperous years when almost the whole of new building may be available for a time to increase carrying capacity. This variation in the average service life of vessels tends to subdue variations in freight rates to some extent and fluctuations in replacement demand will consequently be less severe than if technical considerations alone determined the durability of ships.

Economic models which attempt to expound cyclical fluctuations in shipbuilding in terms of the automatic replacement of tonnage within a short period of its attaining some definite age are often too oversimplified to be applicable to actual conditions as Einarsen (1938, p. 146) pointed out in criticizing Professor Robertson's (1914) calculations. We have seen that there is a wide dispersion about the average age at which tonnage is scrapped and that wastage is far from being confined to some narrow age group and so to the tonnage constructed in particular years of slump and boom. For the purpose of building economic models it is probably more realistic to adopt Kaldor's (1954, p. 59) alternative assumptions that wastage will be more a function of the size of the total fleet than of the tonnage of ships in such a narrow age band. Certainly this would have been a reasonable assumption for the period before 1914 when the need to make good losses arose mainly from the incidence of marine casualties on vessels of all ages.

With the reduction in marine losses, however, replacement demand could be more realistically taken as a function of the tonnage in the higher age groups. Cyclical fluctuations reflected in the detailed composition of these age groups would, in the main, be smoothed out and would not, therefore, be likely to give rise to irregularities in demand for replacement purposes. The size of these age groups might, however, reflect disturbances of a non-cyclical character such as the replacement of war-time losses or very prolonged periods of contraction or expansion in the past and so give rise to variations in the flow of replacement orders.

The life of a ship on the U.K. register depends, as we have seen, on the practice of U.K. shipowners selling abroad a considerable proportion of tonnage over 15 years of age. We should expect, therefore, that the replacement demand for U.K. ships would depend more on the proportion of ships in the fleet above this age than on the proportion reaching the end of their service life. The profitability of sales by U.K. owners depends on the relative prices of new and second-hand ships. In slump conditions second-hand prices are likely to fall in relation to building costs and there will be less incentive to replace ships of moderate age with new vessels. We might expect on these grounds that there would be a tendency for the average life of a ship on the U.K. register to be prolonged in a slump. There is no indication in Table 5 that this occurred in the early thirties, but the severity of the slump, leading to premature scrapping and forced sales stemming from financial difficulties, may have masked the effect. Einarsen's figures (1938, Tables 12–15) suggest, however, that for the Norwegian fleet in the period 1883–1927 owners of new tonnage disposed of a smaller proportion of their ships in slump than in boom years. This effect is likely to make it more difficult to stimulate new building in a depression. One of the difficulties of operating the U.K. scrap and build subsidies in the 1920's was the comparatively small amount of tonnage approaching the end of its useful life in the hands of U.K. shipowners.

We may now turn to considering very briefly the replacement needs of the U.K. fleet over the next ten years, on various assumptions. We may consider first what would happen if the wastage rates from all causes (including sales abroad) were the same as those ruling on the average in the inter-war period. It may be argued that these assumptions are inappropriate for prognostication but the exercise is, nevertheless, revealing.

TABLE 8

Replacement Demand for U.K. Fleet Assumption (1)

	Age Distribution in 1954. *Million Gross Tons*	*Average Wastage Rates. Percentage per Quinquennium*	*Age Distribution in* 1959. *Million Gross Tons**	*Age Distribution in* 1964. *Million Gross Tons**
0–5 . . .	4·13	0·8	3·62	4·01
5–10 . . .	4·46	9·1	4·10	3·59
10–15 . . .	5·44	16·4	4·05	3·73
15–20 . . .	1·99	31·8	4·55	3·39
20 and above . .	3·00	55·3	2·70	4·30
Total . . .	19·02		19·02	19·02

* Tonnage deleted is assumed to be replaced by new tonnage of the same amount.

The figures suggest that the demand for new vessels would average about 700,000 gross tons in 1955–59 and about 800,000 in 1960–64. In practice even if our basic assumptions were not far from the mark we should expect a considerable fluctuation in replacement orders from year to year.

We may consider next what would happen if U.K. shipowners were to abandon the practice of selling ageing tonnage abroad replacing tonnage only when older vessels were scrapped. It is difficult, indeed impossible, to know what wastage rates would apply in such circumstances. We may, however, suppose that they would be similar to the scrapping rates ruling in the years 1929, 1930, 1952 and 1953—the nearest we can get to any "normal" years from our data. The consequence of assuming wastage rates roughly corresponding to experience in these years is set out in Table 9.

TABLE 9

Replacement Demand for U.K. Assumption (2)

Age Group	*Wastage Rates Assumed for Five Year Period*	*Tonnage in 1954. Million Gross Tons*	*Wastage in 1954 to 1959. Million Gross Tons*	*Tonnage in 1959. Million Gross Tons*	*Wastage in 1960 to 1965. Million Gross Tons*	*Tonnage in 1964. Million Gross Tons*
0–5 . . .	·5	4·13	·02	1·35	·01	1·89
5–10 . . .	·5	4·46	·02	4·11	·02	1·34
10–15 . . .	·5	5·44	·03	4·44	·02	4·09
15–20 . . .	5·0	1·99	·10	5·41	·27	4·42
20–25 . . .	10·0	·78	·08	1·89	·19	5·14
25–30 . . .	35·0	1·72*	·60	·70	·24	1·70
Over 30 . . .	100·0	·50*	·50	1·12	1·12	·46
Total . .		19·02	1·35	19·02	1·87	19·02

* No sample of the age distribution has been taken for 1954 and the distribution of tonnage in these groups is notional.

Comparison of the above "numerical" exercise with the previous one is revealing. When sales of second-hand tonnage abroad are excluded wastage rates probably amount to about 3 per cent. per annum for a fleet of balanced age groups. In the previous example in which sales of second-hand tonnage were included wastage rates were nearer 4 per cent. per annum. The most striking difference however, is that in the previous example the amount of wastage was about the same in the two quinquennia whereas in the second example it is appreciably higher in the second than in the first quinquennium and would rise again if the exercise were repeated. There is all the makings of a reinvestment cycle in the second example, though it would probably be of diminishing amplitude as time went on and the oscillations were damped by the incidence of wastage in the lower age groups. We may note, however, that such a cycle would result, not from earlier fluctuations in building but from a change in the practice of U.K. shipowners in disposing of their ageing tonnage.

It is clearly impossible to make any accurate estimation of the replacement demand for the U.K. fleet in the future. Even in the long run average demand may vary between 3 and 4 per cent. per annum depending on the market for U.K. ships abroad. And from year to year much greater fluctuations are likely to occur. Nevertheless the first set of assumptions about wastage rates suggests that the possibility that replacement demand will be of the order of 4 per cent. per annum and comparatively little affected by the age distribution of tonnage on the register cannot be neglected.

If these conditions held the amount of tonnage requiring replacement would be of the order of 800,000 gross tons per year. The tonnage that U.K. shipowners may wish to dispose of in future years is, of course, no good guide to the tonnage which will be needed to replace it. But it is evident that the demand for replacement tonnage is likely to amount to little more than half the present output of 1½ million gross tons of merchant shipping from U.K. shipyards. If the difference is to be made up almost the same amount of tonnage will need to be constructed for foreign owners and the augmentation of the U.K. fleet.

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THE GENERAL FORM OF THE ADDING-UP CRITERION

By J. L. NICHOLSON

Statement of Theorem

Suppose that $x_1 + x_2 + \;\ldots\; + x_n = X$, and that we want to obtain the regression euqations of x_r $(r = 1, 2, \;\ldots\; n)$ and $X\ (= \sum_r x_r)$ on certain "independent" variables, using the same form of equation in every case. In their general form, the regression equations may be written:

$$x_r' = a_r\varphi_1 + b_r\varphi_2 + c_r\varphi_3 + \;\ldots\; + k_r\varphi_k$$
$$(r = 1, 2, \;\ldots\; n)$$
$$X' = A\varphi_1 + B\varphi_2 + C\varphi_3 + \;\ldots\; + K\varphi_k$$

where $\varphi_1, \varphi_2, \;\ldots\; \varphi_k$ are "independent" variables; $a_r, b_r, c_r, \;\ldots\; k_r, A, B, C, \;\ldots\; K$ are regression coefficients; and x_r' and X' are the values of x_r and of X, respectively, given by the regression equations.

If these regression equations are fitted by minimizing the sums of squares of the deviations $(x_r - x_r')$ and $(X - X')$, the values of the corresponding regression coefficients will "add-up" in the sense that $\sum_r a_r = A$, $\sum_r b_r = B$, $\;\ldots\;$ $\sum_r k_r = K$, and therefore $\sum_r x_r' = X'$. There is no restriction on any of the φ's.

Proof

The k regression coefficients in the equation for x_r' are determined by minimizing:

$$\Sigma\,[x_r - a_r\varphi_1 - b_r\varphi_2 - \;\ldots\; - k_r\varphi_k]^2$$

where Σ denotes summation over all observations. There are n such expressions to be minimized $(r = 1, 2, \;\ldots\; n)$.

The $n \times k$ regression coefficients must therefore satisfy n sets of k equations, of the form:

$$\Sigma\,\varphi_s[x_r - a_r\varphi_1 - b_r\varphi_2 - \;\ldots, - k_r\varphi_k] = 0$$
$$(s = 1, 2, \;\ldots\; k;\; r = 1, 2, \;\ldots\; n)$$

Summing for all values of r we obtain k equations of the form:

$$\Sigma\,\varphi_s[X - \sum_r a_r\varphi_1 - \sum_r b_r\varphi_2 - \;\ldots\; - \sum_r k_r\varphi_k] = 0$$
$$(s = 1, 2, \;\ldots\; k)$$

This is, in fact, the set of equations for determining the k regression coefficients in the expression for X, when $\Sigma\,(X - X')^2$ is minimized, with $\sum_r a_r$, $\sum_r b_r$ $\;\ldots\;$ $\sum_r k_r$ written in place of $A, B, \;\ldots\; K$. The solution for $\sum_r a_r$ is therefore the same as the solution for A; the solution for $\sum_r b_r$ is the same as the solution for B; and so on.

It follows that:

$$\sum_r a_r = A,\ \sum_r b_r = B,\ \;\ldots\; \sum_r k_r = K \text{ and hence } \sum_r x_r' = X'.$$

The sum of the regression equations for x_r $(r = 1, 2, \;\ldots\; n)$, each of which is obtained by minimizing $\Sigma\ (x_r - x_r')^2$, will equal the regression equation for X obtained by minimizing $\Sigma\,(X - X')^2$.

Applications

In the particular case where $\varphi_1 = X$ (the sum of the x_r's), the regression equations become:

$$x_r' = a_rX + b_r\varphi_2 + c_r\varphi_3 + \;\ldots\; + k_r\varphi_k$$
$$(r = 1, 2, \;\ldots\; n)$$
$$X' = AX + B\varphi_2 + C\varphi_3 + \;\ldots\; + K\varphi_k$$

On the assumption that $\Sigma(X - X')^2$ is minimized, the equations for determining the regression coefficients $A, B, \ \dots \ K$ are:

$$\begin{aligned}
&\Sigma\, X[X - AX - B\varphi_2 - \ \dots \ - K\varphi_k] = 0\\
&\Sigma\, \varphi_2[X - AX - B\varphi_2 - \ \dots \ - K\varphi_k] = 0\\
&\vdots\\
&\Sigma\, \varphi_k[X - AX - B\varphi_2 - \ \dots \ - K\varphi_k] = 0
\end{aligned}$$

Writing

$$\Delta = \begin{vmatrix} \Sigma\, X^2 & \Sigma\, X\varphi_2 & \dots & \Sigma\, X\varphi_k \\ \Sigma\, \varphi_2 X & \Sigma\, \varphi_2{}^2 & \dots & \Sigma\, \varphi_2\varphi_k \\ \vdots & & & \\ \Sigma\, \varphi_k X & \Sigma\, \varphi_k\varphi_2 & \dots & \Sigma\, \varphi_k{}^2 \end{vmatrix}$$

we obtain $A = \dfrac{\Delta}{\Delta} = 1$. And, since each of the determinants in the expressions for $B, C, \ \dots \ K$ has two identical columns, $B = C = \ \dots \ = K = 0$.

It follows from the general theorem stated above that, in this particular case, $\sum_r a_r = 1$ and $\sum_r b_r = \sum_r c_r = \ \dots \ = \sum_r k_r = 0$. There is still no restriction on $\varphi_2, \varphi_3, \ \dots \ \varphi_k$.

In this particular form, the adding-up criterion has been applied to the problem of deriving Engel curves for different commodities, the sum of expenditure on all commodities and savings being equal to income. The theorem was originally proved for one form of Engel curve, viz.:

$$x_r \text{ (the expenditure on the } r^{\text{th}} \text{ commodity)} = a_r + b_r X + c_r X^2$$

which was applied to pre-war working-class family budgets (Nicholson, 1949). Apart from the presence of X as one of the variables, the proof given did not, however, in any way depend on the form of the function (although this was not pointed out at the time). An explicit, if rather lengthy, proof of another particular version of the adding-up criterion—namely, in which $\varphi_1, \varphi_2, \ \dots \ \varphi_k$ are all functions of X and $\varphi_1 = X$—was later provided by Worswick (Worswick and Champernowne, 1954–55).

Professor D. G. Champernowne then pointed out (*ibid.*) that the adding-up criterion also holds good in the very special case in which every value of X falls on the regression line, so that $X \equiv X'$. This case is of no practical consequence, since we would never fit regression lines to the values of a known function, nor are we ever interested in regression lines which contain enough parameters to ensure a perfect fit.*

* * * * *

It should be noted that the theorem, whether in the general or in any of its particular forms, holds good only if the regression equations are obtained by minimizing the sums of squares of deviations in the direction of x_r. It does not hold good (except in the case of perfect correlation) if the regression equations are obtained by minimizing the sums of squares of deviations in the direction of any of the "independent" variables.

The theorem is relevant, not only to Engel curves, but to all cases in which regression equations are applied to two or more subdivisions of a given total, as in the recent attempt by Dr. T. Barna to relate the net output of an industry, and the main sub-divisions of net output, to the level of assets (Barna, 1957).

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* It is surprising that Professor Champernowne should have stopped short at this point, since he started on the scent of the general theorem and ended by claiming that his case was more general than Worswick's.

FURTHER NOTES ON THE PAPER BY J. E. G. UTTING AND DOROTHY COLE ON ESTIMATING EXPENDITURE, SAVING AND INCOME FROM HOUSEHOLD BUDGETS

Utting and Cole in their paper (this *Journal*, 1956) raised two main problems:

(*a*) How can the rate of return in budget enquiries be increased?
(*b*) Why is it that expenditure tends to exceed income?

The two problems are probably related, through the reluctance of the household approached to reveal how much money is being received. In some cases the people get over the difficulty by failing to make the return; hence the low rate. In other cases they handle the matter by suppressing some of their income without realizing that the fact will be noticeable when their income figures are compared with their expenditure figures.

The authors are reluctant to accept the conclusion that there is deliberate suppression or distortion in income returns. But this conclusion seems inescapable in the light of the history of budget enquiries and in the light of enquiries which are continually being made by research organizations into the problem of income.

Turning first to the history of income enquiries, one finds constant complaints that however detailed and searching the enquiries may be, expenditure exceeds the income revealed. This is the case even if one studies the records of social workers or charity organization enquirers who were handling only people in the lowest income strata. However poor these people might be, the phenomenon was found in a high proportion of cases. Expenditure might be fixed item by item so that there could be no doubt that the expenditure figures had been accurately given, and there was no confusion about the time period involved, yet the income they said they received was lower than the total amount they spent. There is no conclusion to be drawn other than that of deliberate suppression. Is there any reason why this conclusion should be repugnant to researchers? It would be astonishing if it were not true bearing in mind that when charity organizations are enquiring into the amount that is received, its level would determine the amount of aid to be given; with budget enquiries nowadays a high proportion of respondents have tax returns looming up behind them.

Asking all members of the household about their income is no guarantee against suppression or distortion. Research has established that one in every four husbands do not tell their wives how much they earn, and it is also a sad fact about family life that many young people living with their parents fail to tell them exactly how much they get each week. In these circumstances one member of a family cannot be used as a check on the information supplied by another.

Even in the expenditure field there is evidence of suppression, when, for instance, an enquiry is being made by an official body. We, the Gallup Poll, have on at least three occasions followed the Government Social Survey in making enquiries concerning the amounts spent in particular fields; holidays, gardening and house decorating. On each occasion we have found a very much higher total than was discovered by the Government Social Survey; in the case of gardening it was even 3 times as high, although the two enquiries were made within 12 months of each other. There is no reason to believe that our figures were exaggerated, because gardening, for instance, is a very seasonal occupation and the times of expenditure can be fixed with considerable exactitude. Similarly, with holidays and house decorating, they are all seasonal.

This brings me to the point made by the authors, that maybe the expenditure as reported is exaggerated. Undoubtedly this does take place for a variety of reasons. When the period of time is not exactly fixed it tends to be expanded and more expenditure crammed into it than is really the case. It is not impossible to arrange the enquiry so that the expenditure period can be defined with considerable accuracy, by referring, for instance, to high days and holidays, birthdays and so on. We are currently engaged in taking people back over the preceding 12 months concerning their expenditure in given fields, and there is every reason for believing, on checking against independent sources, that the details are we securing are correct.

We can sum up and say that depending on the circumstances surrounding the enquiry, and the skill with which it is conducted, expenditure may be suppressed or exaggerated, but it is more often exaggerated. There is no evidence of exaggeration of income; plenty of evidence that it is suppressed.

The implication is that it seems worthwhile in budget enquiries to try not asking about incomes. This will almost certainly have the result of sending up the rate of response, which is one of the problems raised. If the enquiry is cleverly worded, there will be information about savings, which from the point of view of most respondents is merely another form of expenditure. The total amount of money *going into* the household would be accounted for in terms of the total amount *going out*.

The possible objection to this course runs along these lines. "Expenditure seems to be exaggerated: therefore, it is desirable to have income figures as a check." This argument holds in theory, breaks down in practice, since discrepancies are consistently found. Everyone would admit that mainly, for most budget enquiries, accurate expenditure figures are the goal. Why not concentrate on it?

HENRY DURANT.

A REPLY TO DR. DURANT

Dr. Durant's note seems to be based largely on a misunderstanding of the views which we tried to express in our paper. We certainly did not wish to deny that distortion and suppression in income returns are sometimes deliberate (although we think that quite often they are not); but we did wish to emphasize that, in our study at least, there was a good deal of evidence to suggest that this was not the *only* reason for the discrepancy between income and expenditure-plus-saving. In our view, the known fact that income was understated has too often been assumed to account for the whole of the discrepancy, and may have led investigators to overlook the possibility of overstatement of expenditure. To ignore this possibility in our lowest income group would imply that income in this group is understated by more than one-third. This is not plausible in a group where incomes are low and consist mainly of State pensions of fixed amount. We conclude that there is overstatement of expenditure in this and other income groups, and that attention should be given to the elimination of reporting biases in expenditure as well as in income.

This is one reason why we are unwilling to accept Dr. Durant's suggestion that total expenditure is an adequate substitute for direct income information. Improvement of the expenditure information almost certainly lies in the use of well-defined time periods, which will be quite short for at least some of the items. Expenditure in short periods, even when combined with information about saving or dis-saving, is a very inadequate guide to an annual income total, such as is required for classifying a household to an income group. If, as in our own survey, different expenditure periods are used for different items—and this has many advantages for collecting reliable figures—it becomes almost impossible to use expenditure to determine income.

There is, in any case, very little evidence that asking income questions affects the response rate more than asking detailed expenditure and savings questions. In the Cambridgeshire survey, respondents were not told that they would be asked income or savings questions until the expenditure questions had been asked. There was a slight difference in response between the income half of the sample (74 per cent.) and the savings half (76 per cent.), but this can hardly have been due to the questions which followed. In both halves some 8 per cent. of those who had answered expenditure questions refused to give enough information to determine an income total or savings total respectively.

We entirely agree that individual members of a household may often hide their incomes from one another. This is probably true of many items of expenditure as well. It is for this reason that *household* totals can only be obtained by seeking information from all members of the household and by asking very detailed, searching questions. This is generally accepted in respect of expenditure studies and does not seem appreciably to have lowered response rates—or any reduction there may be is acceptable as the price of better information. We believe that the same will prove to be true of income figures; better designed and more detailed questionnaires will yield more reliable data with little reduction in response. The assumption that income is a difficult subject all too often leads to the use of weakly constructed questionnaires—in the hope that the questions will be "inoffensive"—and so obtains inadequate replies which appear to confirm the original hypothesis.

Finally, Dr. Durant states that for most budget enquiries accurate expenditure figures are the goal. This may be true, and we certainly do not wish to imply that, in such enquiries, expenditure is not the thing to concentrate on. Indeed our paper ends by suggesting a subject of research for the improvement of *expenditure* data. But for some purposes (studies of the sources of income, the make-up of household income, and so on) it is the detail of income that is important; in others the detail of savings or asset holdings may be the main question; and in others the inter-relationship of all these factors. We see no reason, at present, why the researcher in these fields should exclude any of these topics from his consideration on grounds either of importance or of feasibility.

DOROTHY COLE.
J. E. G. UTTING.

REVIEWS OF STATISTICAL AND ECONOMIC BOOKS

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1.—*Statistical Methods and Scientific Inference*. By Sir Ronald Fisher. Edinburgh and London: Oliver & Boyd, 1956. viii, 175 pp. 8¾″. 16*s*.

Teaching mathematical statistics to trained mathematicians is on the whole straightforward—one introduces some convenient axioms, based on intuitive ideas if necessary; then develops the properties of a variety of different distributions, in particular those of such quantities as t, z, F, r, χ^2 etc.; next, perhaps, something about the properties of different kinds of estimators for unknown parameters; then the theory of testing hypotheses—usually the Neyman-Pearson approach with some readily understood definitions and a good deal of ensuing mathematics; something about the abstract principles of experimental design; and a fleeting acquaintance with the elements of several other topics such as sequential analysis, stochastic processes, decision theory etc., according to choice. Providing the students learn the standard book-work properly and do not venture too far afield with essay questions, they are likely to pass their examinations adequately.

On the other hand, instructing intelligent research workers, particularly biologists, in statistical methods is a different story. One can no longer assume an innate propensity to manipulate mathematical symbols. After discussing the basic ideas of probability in an elementary fashion, with of course practical examples, a few simple distributions can be introduced in fairly general terms. However, something of obvious practical use must soon be forthcoming if interest is not to flag. What the scientist wants to know is mainly how to reduce his data to manageable proportions, how to decide whether apparent differences are due to chance, and how to make good estimates of basic parameters that interest him. The first big hurdle is the significance test, conveniently introduced perhaps through the χ^2 test for proportionality in 2 × 2 contingency tables or a t-test for mean values. It is often intuitively clear to the research worker that such tests are giving him what he wants—they seem, especially when used a few times on his own data, to be useful tools having an empirical justification. Estimation may be more difficult, but many biologists learn to use maximum-likelihood, particularly scoring methods, to extract useful information from apparently intractable data. The real trouble comes when the statistician is asked what precisely it all means—what exactly is probability, why is a significance test justified, how do you choose a significance level, and so on. Too often these questions are either evaded, stifled by appeals to mathematical authority, or aggravated by dark hints about errors of the first and second kinds. Needless to say none of this ever carries much conviction, and usually leaves both class and lecturer feeling slightly uneasy.

Another sign that something is wrong is the not uncommon situation in which a man, apparently well qualified theoretically in mathematical statistics, is confronted (perhaps for the first time?) with some actual data and is quite unable to draw any useful practical conclusions, although all that is required is an elementary application of t or χ^2.

Perhaps the chief reasons for the unsatisfactory state of affairs outlined above are an undue concentration in teaching on the purely mathematical aspects of statistical theory and insufficient attention to the problems of uncertain inference as they actually arise in practical work. Moreover, it is hardly to be wondered at when students, whose ideas on testing hypotheses have been largely based on the model afforded by industrial acceptance procedures, are baffled by the very different situations arising in much scientific research work.

For many years now there has been a good deal of strenuous controversy about the underlying basis of statistical inference, and the present book, *Statistical Methods and Scientific Inference*, by one of the protagonists in the drama, is therefore all the more welcome. It is not of course to be expected that all adherents of inverse probability and followers of the Neyman-Pearson line will immediately be converted to Sir Ronald Fisher's well-known views on the use of fiducial arguments, the frequent inadmissibility of errors of the second kind, and the unwise insistence on "repeated sampling from the same population". Nevertheless, it is extremely helpful to have these views clearly set out and closely argued within the compass of a single exposition, instead of having, as previously, only a number of papers scattered throughout the literature.

Sir Ronald emphasizes the fundamental distinction between situations, common in industry, involving acceptance procedures and the day-to-day problems confronting a scientific research worker who wishes to know whether various effects observed during the course of a research programme are likely to be due only to chance fluctuations. In the former case notions of errors of the first and second kinds, as defined in the Neyman-Pearson theory of tests, have an immediately physical interpretation. Only by a considerable stretch of imagination can this theory be applied to typical situations in the second case, and even then, as Sir Ronald points out, it may be positively misleading. In short, there are no cost functions in science. Naturally, the two broad areas do overlap to some extent, but the basic distinction remains.

Those who are intimately concerned with scientific research, as opposed to technological development, are more likely to find the approach of this book congenial. Certainly, in the experience of the reviewer, the task of providing the scientific research worker with the tools of elementary statistical method is more easily accomplished in the context of fiducial arguments than of Neyman-Pearson theory. The practical scientist is on the whole unimpressed by mathematical authority, but will adopt a new method if he can understand it well enough to use it and then finds it of positive help.

The first two chapters of *Statistical Methods and Scientific Inference* provide a general background and historical perspective for the main discussion. In chapter 3 the central topics of significance tests, Bayesian arguments, fiducial arguments, confidence limits and mathematical likelihood are introduced. Chapter 4 discusses "some misapprehensions about tests of significance", and deals in particular with Student's t-test, linear regression, 2 × 2 tables and Behren's test. This is then followed in chapter 5 by a more extensive account of the part played by probability and likelihood in the drawing of inferences, with special reference to such topics as Bayesian prediction, fiducial prediction, fiducial distribution of functions of parameters, etc. The sixth and final chapter gives a short exposition of the principles of estimation with special emphasis on the logical concepts introduced earlier.

Sir Ronald is to be congratulated on the publication of this book, a need for which has been felt for some time. It will do much to clear up existing misapprehensions as to the spheres of application and validity of the various available forms of uncertain inference. One hopes it will be widely read and discussed, making as it does a substantial contribution to a controversy of fundamental importance.

NORMAN T. J. BAILEY.

2.—*Language as Choice and Chance*. By G. Herdan. Groningen: Noordhoff, 1956. xiii, 356 pp. 9¾″. $8.

Language is essentially a conventional symbolic or approximate representational activity whose main function is to help thought and to influence the behaviour of others with the minimum use of effort or force. (A secondary use is to give pleasure or pain.) The best methods for most purposes are by writing, talking and verbal thinking: waving the arms, as in semaphore, is inefficient because the arms are heavy. A particular language is a nexus of intimately connected conventions of these types. Conventions are more or less frequently exercised, and part of the specification of a language should be a statement of the frequencies. A good dictionary should give some impression of the probabilities of words so that writers who wished to make themselves clear could avoid or define infrequent words. The probabilities are functions of time, place and context, and information about these aspects is already given in dictionaries as when a word is described as "archaic", "Lancashire" or "low", respectively. A language is a slowly-developing organism with tendencies to split and also to integrate.

F. de Saussure defined a *langue* (which I am here calling a language) as a set of linguistic units and also as a principle of classification. Herdan goes further and defines it as a set of linguistic units together with their probabilities of occurring, so that a language is a statistical population (pp. 3 and 79). "Without a sensibly stable series of relative frequencies of linguistic symbols or forms there can be no prediction, or rather no guessing [with] some expectation of being correct, and thus there can be no information in the sense of information theory. This is the connection between stability of relative frequencies of language data and information" (p. 80). The notion that linguistic forms have more or less stable relative frequencies is at the root of language teaching by the Orthological Institute, to which no reference is made. The aim of the book is "to conceive of literary statistics as an integral part of linguistics" (p. 2). Among the applications discussed are methods of defining numerical parameters of style, richness or efficiency of a language, and the merit of a translation, though these measures are liable to be rather superficial in the present state of the science of statistical linguistics.

The book has much in common with Yule's *Statistical Study of Literary Vocabulary* (1944), Zipf's *Selected Studies of the Principle of Relative Frequencies in Language* (1932), and Zipf's *Human Behaviour and the Principle of Least Effort* (1949). The book will probably stimulate further interest in statistical linguistics, partly because of the statistical data, partly because of the subjects discussed, and partly because the English style is good (though there are occasional lapses such as the use of ill-defined ideas like "duality").

The remainder of this review consists of examples to show that many of the arguments in the book are not entirely adequate.

In Chapter 3 the analysis of random samples of textual words (as distinct from random samples from a dictionary) is treated in a rather back-handed manner, and without mention of *Biometrika* **40** (1953), 237–264, where there is a much fuller treatment. (See also *Biometrika* **43** (1956), 45–63, by Good and Toulmin, which of course was not available when the book was written.) If, for convenience, we call each textual word an "animal" and each dictionary word a "species", then the natural meaning for the "sample size" is the number of animals in the sample. The author at first has no single symbol for this number, and refers to the number of distinct species in the sample as the sample size. This point of view leads him to say (p. 7) that "the mean and standard deviation as used in number statistics lose some of their useful and essential properties when applied to word frequency distributions, which compels us [Yule?] to develop another statistical quantity more suitable for describing such distributions, the so-called Characteristic." Both Yule and Herdan overlook the fact that Yule's characteristic is, after a trivial transformation, an unbiased estimate of what may be called the "repeat rate", i.e. the probability that two words chosen at random from the text will be the same dictionary word. (See the first *Biometrika* reference, p. 245, for further details.)

On p. 114, σ_G and σ_L are defined, for any language, as the standard deviations of the lengths of runs of grammatical words and "lexicon" words. It is found that among English, German and Russian, σ_L increases when σ_G decreases. Since, for a sample of only 3, the random probability of this result is as high as 1/6, the sample seems inadequate ground on which to base the generalization that σ_G and σ_L are anticorrelated. A similar criticism applies to p. 42.

It is stated twice (pp. 114 and 115) that var (G) var (L) is not less than cov (G, L). In this statement the covariance should be squared. The analogy with the uncertainty relation in physics (pp. 114 and 287) between conjugate variables seems far-fetched to the reviewer. Similarly, on p. 5, an incorrect description of Bohr's correspondence principle is dragged in, with a vague reference to Sir Edmund Whittaker's *From Euclid to Eddington* (in which the principle is not indexed).

On p. 162 the use of a binary code is described as "the theme of information theory" and is later used as a support for the idea of linguistic duality (p. 219). Examples of linguistic duality, given on page 218, are Good and Evil, God and the Devil, Content and Form, Body and Soul, Mind and Matter. What about Black, grey, white; Opaque, translucent, transparent; Hot, warm, tepid, cool, cold; God-like, saintly, good, nice, naughty, spiteful, bad, wicked, satanic? Even in information theory the binary code is not uniquely best for all purposes. If, in radix r, digits are represented by pulses along one of r wires, then the most economical radix is $r = 3$, with $r = 2$ and $2 = 4$ as runners-up.

In Chapter 10 there is a study of the numbers of syllables in English, German, and Russian words. For a given language let p_r be the probability that a word ("animal", see above) chosen at random will have r syllables. The expected amount of information per word, or entropy per word, if the only information considered is the number of syllables, is $H = -\Sigma p_r \log p_r$. In order to define redundancy we must determine what the maximum value of H could be for any conceivable language. But this maximum is infinite if the number of syllables is not bounded. Accordingly it seems natural to the reviewer to work with the entropy per syllable (or more generally with the entropy per unit of effort), i.e. with H/M, where $M = \Sigma\, rp_r$. It is then a simple exercise in the calculus to prove that the least redundancy per syllable would be obtained by taking $p_r = 2^{-r}$. (Or if the effort for expressing a word of r syllables is $f(r)$, then the optimal p_r is of the form $\exp(-cf(r))$.) It is interesting to observe that the distribution of the number of "strokes" in words expressed in Pitman's shorthand is quite close to the optimal distribution, $p_r = 2^{-r}$, and it speaks highly of Pitman's judgment. Through working with the entropy per word Herdan misses most of this. In order to obtain a finite value for the maximum entropy per word he assumes that the maximum number of syllables in words of a language is equal to the maximum number that happened to occur in his sample. In this way he obtains numerical values for the redundancies, and he finds that English, German and Russian are in decreasing order of redundancy. (Working with redundancy per syllable English would still be the most redundant of the three languages.) From this he deduces (pp. 188 and 190) that English, German, and Russian are in decreasing order of efficiency, whereas the reverse conclusion would be the natural one. It should be stressed here that we are only talking about the information available from the number of syllables, and also that some redundancy is required in a language in order that it should be intelligible (especially as most people are inattentive listeners).

On p. 266 it is mentioned that Willis's taxonomic law fits some word counts rather well. Here there should have been a reference to Zipf's work: in fact the law, in the abstract, could fairly be called the Pareto-Willis-Zipf law.

Language, like war, is a typically human activity, and it may be expected that its statistical study will be of increasingly practical value. Herdan's book should serve a useful function in stimulating interest in the subject.

I. J. GOOD.

3.—*Symposium on Monte Carlo Methods.* Ed. by Herbert A. Meyer. New York: Wiley, 1956. London: Chapman & Hall. xvi, 382 pp. 11″. 60*s*.

This is the first book entirely devoted to Monte Carlo methods. It contains the proceedings of a symposium held at the University of Florida, Gainesville, on March 16th and 17th, 1954, under the sponsorship of the Wright Air Development Center of the Air Research and Development Command. The first part of the book contains twenty papers by various authors on various Monte Carlo topics. The second part, occupying the last eighty-eight pages of the book, is a bibliography of relevant literature.

The collection opens with an admirable introductory note by A. W. Marshall. This not only introduces the remaining papers in the book, but also surveys the origins of Monte Carlo and the work done on it in the last decade. Any statistician who wants to become acquainted with Monte Carlo methods could not do better than read this article: indeed the book would be worth buying for this alone.

The remaining papers, as in any symposium of papers presented at a general meeting, are a mixed bag. Some are very good, while one or two are practically unintelligible even to experts in the field. Amongst the good papers one may cite a review on the generation and testing of pseudo-random numbers by Olga Taussky and John Todd, the original and stimulating work on conditional Monte Carlo by Trotter and Tukey, applications to gamma ray diffusion by Berger, a long survey of sampling techniques by Kahn, and the efficiency of Monte Carlo in solving simultaneous equations by Curtiss.

The useful bibliography is in four sections: (i) on Monte Carlo proper, (ii) on random digits, (iii) on sampling used as a method of solution or verification, and (iv) on stochastic processes. In (i) and (ii), and to some extent in (iii) and (iv), there is a short account attached to each reference, summarizing the contents of the paper in question.

J. M. HAMMERSLEY.

4.—*Numerical Analysis. Proceedings of Symposia in Applied Mathematics*, Vol. 6. New York and London: McGraw Hill, for the American Mathematical Society, 1956. vi, 303 pp. 10″. 73*s*.

This book, although reporting the events of a meeting held three years ago, is an indication of the increasing interest in the subject of numerical analysis to-day.

Ten years ago such a meeting would be regarded as a novelty. To-day nearly every year produces at least one such organized discussion. Likewise, ten years ago it could be predicted that the papers at such a meeting would be restricted to the classical problems of numerical analysis—the two problems of linear algebra (inversion of matrices and determination of eigenvalues), the determination of zeroes of functions (principally polynomials) and the numerical solution of differential equations (mainly ordinary differential equations, but with a hint of some of the difficulties of partial differential equations).

This should be contrasted with the topics in this book. Of the nineteen papers in the book only two are concerned with partial differential equations, none with ordinary differential equations explicitly and none with the traditional methods of zero-finding. There are, it is true, three papers on iterative processes for solving problems of linear algebra but these are all very general in nature and their emphasis is on the extension of the gradient methods to non-linear systems. A paper on the error bounds for eigenvalues of Symmetric Integral Equations completes those on classical numerical analysis.

What of the remaining thirteen papers? There are three on the approximation of one function by another. One of these is a brief account of the new celebrated empirical work of Hastings, another is on Tchebycheff polynomials, and the third is a very abstract account called "Function Spaces and Approximation". The remaining six are either on topics not yet formally invented ten years ago or else on branches of mathematics formerly regarded as the province of the purest pure mathematician.

In the former category there are papers on dynamic programming and the assignment problem; two cases of those awkward non-linear problems of the kind mostly associated with linear programming. Linear programming is not dealt with explicitly in this volume, due no doubt to the amount of work published in connection with the linear programming symposium which was held at about the time of this symposium.

In the latter category, there are papers on the application of automatic computers to number theory, algebraic number theory, combinational problems, permutation problems and finally conformal mapping.

The last paper, defying this classification, which must be mentioned is the only paper of direct interest to statisticians. This is an extension of the work of Teichroew on asymptotic transformations of distributions into the normal form. This paper by Wasow generalizes Teichroew's work and gives existence theorems for the asymptotic transformations for a wide class of distributions.

Undoubtedly, the change of emphasis displayed in this book is directly attributable to the development of high-speed computers which have brought into range problems previously shelved as intractable. This is a trend that must be welcomed but an inspection of papers on the "new" problems show that although we now recognize the possibility in principle of solving these problems numerically we are in many cases not much beyond that stage. A colleague of mine once had a text above his head "Remember that one fool can ask more questions than ten wise men can answer". I am not suggesting that the authors of these papers fall into the former category but they certainly are themselves under no illusions as to the difficulties of the problems they raise.

The book makes stimulating, but rather difficult, reading and is a veritable mine of references to other papers on all the topics of numerical analysis.

K. D. TOCHER.

5.—*A New Form of Table for Significance Tests in a* 2 × 2 *Contingency Table*. By P. Armsen. *Biometrika* (1955). Vol. 42, pp. 494–511. (New Statistical Tables, 21.) 10¾″. 2*s*.6*d*.

This is yet another table associated with this very old problem and the surprising thing is that the table is justified. Mr. Armsen points out that without a randomization procedure the discrete nature of the hypergeometric distribution not only prevents exact tests of significance at a pre-assigned level being carried out, but that it also introduces an ambiguity in the test region (based on that distribution) for two-tailed tests. The usual symmetric two-tailed test is formed by a region with probability well below the traditional limits of 5 per cent. and 1 per cent. and extra points in one or both of the tails can be added without raising the probability of the region above those limits. In many cases there will be a choice of possible additional points, and the paper suggests two alternative objective criteria for the inclusion of extra parts in the region which only differ from one another in a small minority of cases.

The tables are very compact, utilizing a rearrangement of the original table to give the largest marginal entry in a fixed position, and each entry gives the 5 per cent. and 1 per cent. critical parts for both one and two-tailed tests for all tables with a total sample size of up to 50.

They are based on the exact distribution and an appendix gives the mathematical properties of the distribution used in their construction.

The tables are photographically reproduced from a manuscript, which does not look particularly neat but has the merit of cheapness of production and the complete elimination of copying errors.

K. D. TOCHER.

6.—*CIBA Foundation Symposium on Extrasensory Perception.* Editors, G. E. W. Wolstenholme and Elaine C. P. Millar. London: Churchill, 1956. x, 240 pp. 8″. 27*s.* 6*d.*

Metaphysics was defined by F. H. Bradley as the finding of bad reasons for what we believe by instinct, and even Isaac Newton was said to have exclaimed, "Oh physics! Preserve me from metaphysics".

Extrasensory perception (ESP) is a subject which most scientists would be unhappy to have discussed in their drawing rooms. Those who do not prefer to avoid the subject are so divided on it that language unacceptable in most drawing rooms may be used. Yet there are aspects of ESP which statisticians cannot ignore. Results are claimed by experimenters in ESP to be based on statistical designs. Indeed most of the experiments of recent years bear a very close similarity to statisticians' experiments with random number generators. It is unfortunate that ESP verges on the metaphysical, but we can hardly claim that our own concept of randomness is completely satisfactory. This point is discussed in a paper included in the book (S. Brown, pp. 73–80). Other chapters will have varying degrees of attraction to statisticians.

The CIBA foundation has done a good service in calling an assembly to discuss ESP and in making their discussion available in this book. The method of presentation is well suited to the subject. Reading in print is a more suitable means of communication for this subject than discussion, for any discussion of ESP finds the audience divided into two parts, those who believe and those who just as passionately disbelieve. There may well have been some heat generated in the CIBA symposium, but little gets through the able editing. Some obvious misprints do appear, but the book was very quickly made available; the symposium took place in May 1955.

It is difficult to assess whether or not the book presents an unbiased account; rather, each contribution represents its own particular bias. In terms of the numbers present and taking part in the discussion the "believers" were in the majority. Whatever doubts may exist about the balance of the treatment of the subject, there is no doubt that this book presents one of the best general accounts of ESP in a form suited to those who are able to give the matter serious consideration.

LESLIE T. WILKINS.

7.—*Dynamic Factors in Industrial Productivity.* By Seymour Melman. Oxford: Blackwell, 1956. xiii, 238 pp. 8½″. 22*s.*6*d.*

This is an interesting but diverse book. The first part of sixty-six pages records a survey of conditions which have led to the increased use of mechanical handling equipment in factories making motor vehicles in the United Kingdom. This is supplemented by some discussion on the growth in the proportion of administrative workers to operatives. The remainder of the book makes use of information already published of national figures of output per man and electrical power used in manufacturing.

Money having lost much of its usefulness as a measure of value over time the unit used in this book is primarily a man-hour of labour. The cost of electricity and capital equipment are both referred to the cost of one hour's labour whilst the general aim is to determine the factors which increase output in terms of this unit. The approach is good; output per man-hour is not the sole criterion of productivity, the cost of any machine to replace the man has also to be considered. Thus, the first hypothesis considered is that the degree of mechanization in industry depends on the ratio of labour cost to the capital and running costs of the machine that can replace that labour. In this respect it is shown that in 1950 earnings per hour were 2½ times what they had been in 1938 whereas capital and running costs of electric trucks were only 1½ times as great so there was a greater urge to use such handling equipment after than before the war, other necessary conditions for mechanization of production being substantially unchanged from before the war. It seems therefore that the main reason for increased mechanization must be the change in alternative costs.

A difficulty arises if one attempts to generalize the proposition. A rise of only 50 per cent. in the price of capital equipment between 1938 and 1950 seems lower than the general rise. Elsewhere in the book estimates are given showing that between 1938 and 1947 capital goods (for export) rose in price nearly as much as earnings per hour and Redfern's work (this *Journal*, 1955) seems to show much the same thing for the period 1938 to 1950. One wonders therefore if experience of prices of trucks for mechanical handling is typical and whether, in general, comparative costs so favour the introduction of machinery. The book, working in units of a man-hour, does not discuss the advantages of immediate installation of durable equipment when money prices are rising.

The second hypothesis is that labour productivity per man-hour depends upon the degree of mechanization. Here the discussion is already on the national basis and introduces international comparisons. The argument is that installed horsepower of electric motors per production worker depends largely on the price of electricity in terms of a man-hour of labour and that output per person has increased at about the same rate as installed horsepower, both over time in the same country and between countries—though the scatter diagram showing this would look less impressive if it were not for the series of figures for the United States running from 1899 to 1947. The postulated relationship looks very precise when, for the United States and the United Kingdom, treated separately, it can be shown that:

$$\text{Log output per person} = a + b \text{ log number of Kwh. purchasable at the cost of a man-hour.}$$

The value of a however is much greater in the United States than in the United Kingdom; the differences in productivity cannot be measured only in terms of the price of electricity per man-hour of labour.

A third line of enquiry traces the growth of administrative staff compared with productive labour. In both the United States and the United Kingdom administrative employees increased in numbers from 10 per cent. to about 20 per cent. of productive workers in manufacturing industries over periods of 40 years. Rather curiously this is not investigated as a possible question of comparative costs, but rather as a feature of the need of managements to make decisions.

H. S. BOOKER.

8.—*National Income Visualised*. By Arthur O. Dahlberg. New York: Columbia University Press; London: Oxford University Press. 1956. x, 117 pp. 12¼″. 28*s*.

This book is a skilful description of the system of national accounting in a progressive series of fifty schematic pictures or flow-charts, accompanied by a concise text. The author-cum-artist begins with the basic two-way relationship between households and productive enterprises—the exchange of labour for incomes, and of incomes for goods. The complications are introduced one by one—the functions of government, taxes, savings and investment, foreign trade. But the basic relationships between household and productive enterprise are kept literally in the centre of the picture. This is helpful ; a failing of some graphical presentations of national accounts is that the student cannot distinguish the main line from the branch lines and sidings.

Dr. Dahlberg (who is Director of the Visual Economics Laboratory at Columbia University) has devised an ingenious pictorial system. He uses perspective drawings resembling flow charts but allowing for the occasional use of a third dimension by depicting two or more "storeys" in the drawing. Thus beneath the schematic factory (representing the enterprise, or the "business sector") we see a basement. Receipts from sales are shown as a current account flow ending in this basement which represents the liquid assets of the enterprise. Another flow of factor payments starts up in the liquid assets basement, surfaces, and terminates in the liquid assets basement of the schematic villa (the "personal sector"). This is preferable to the usual exposition (pictorial, verbal or statistical) which so often fails to emphasize the important relationships in economic life between stocks and flows—between income and expenditure on the one hand, and assets and liabilities on the other.

The author has two purposes in mind. One purpose is to describe the fundamental processes of economic activity. The other is to describe the system of national accounting. The use of national accounting categories and statistics is an excellent way of teaching elementary economics, as the success of Hicks' *The Social Framework* shows. But it has drawbacks. An accounting system cannot adequately display the dynamics of economic life—the operations of choice and anticipation—although the accounts reveal the end results. Neither *Bradshaw* (however-up-to-date) nor a railway track diagram can exhibit the principles of locomotion nor what happens when the lines are iced up. Again, although the national accounting categories cover the most significant elements of economic life, they are necessarily simplifications and hence to some extent arbitrary. Thus the student is obliged to make the extra effort of learning a set of accounting conventions, which are necessary for measuring and classifying economic activities, but not always for describing or understanding them. The accounting conventions and statistics sometimes seem to impose a veil between the observer and the things observed. Nevertheless, most students should find the statistical veil more transparent than the thick fog which purely verbal, algebraic, or geometrical descriptions often leave.

The book illustrates the national accounting system used by the United States Department of Commerce. The differences from the official United Kingdom system are not fundamental but there are some which the user of this book must bear in mind. Thus depreciation is treated in the United States system as a separate flow in the classification of the national income, added to net profits. Interest payments are also treated as a separate flow and not, as in the United Kingdom system, as a transfer out of profits. More important, there is almost no mention of foreign trade till the end of the book; the British user would need to rearrange the exposition in this respect.

The economist may raise his eyebrows at some features in the exposition. Thus Dr. Dahlberg seems to be led by the tidiness of the accounting system applied to equilibrium conditions into asserting the existence of a "market price"—the sum of factor costs and indirect taxes—from which the actual "sales price" may differ in certain circumstances. One imagined that hardheaded economists had abolished this distinction more than a generation ago. Also the treatment of indirect taxes is not altogether satisfactory. Dr. Dahlberg is, rightly, uneasy about the distinction made in national accounts between "factor cost" and "market prices". (The factor cost is intended to measure how much factor income is directly generated by a given expenditure on goods and services. The market price is what is actually paid by the buyer and includes any indirect taxes as well as payments to factors of production). Dr. Dahlberg appears to feel that indirect taxes, and, for that matter, direct taxes, would better be treated as factor payments to the Government for the services which it performs (protection of society, provision of public services etc.). This raises a vexed question in national accounting. But the solution suggested seems to create unnecessary problems by implying that the tax content of an article measures the price of "government services" embodied in it—so that the purchase of tobacco involves the purchase of a large quantity of government services, while the purchase of subsidized milk involves the free receipt of government services as well as the milk. The conventional solution adopted by national income statisticians, who treat government as a final purchaser of goods and services comparable with consumers, cuts through these improbable implications and seems to be a justified use of Occam's razor.

The main feature of the book, however, is the use of graphical exposition as a pedagogic method. There can be no doubt that, in many students, interest is more easily aroused, comprehension facilitated and memory aided, by visual methods. The teacher who is willing to recognize the needs of such students, and who has besides a ready hand with chalk and blackboard, will find this book helpful and suggestive.

Two blocks of letterpress have been displaced on pages 27 and 83. C. T. SAUNDERS.

9.—*Introduction to Demography*. By Mortimer Spiegelman. Chicago: Society of Actuaries, 1955. xxi, 309 pp. 9″. $6.

The author's preface begins "The primary purpose of this volume is to introduce students who are preparing for the examinations of the Society of Actuaries to the methods of demography. Accordingly, the emphasis is largely on the practices that have developed in the United States and Canada".

Ideally, a book of this nature should be written in such a way that it assists in the student's gradual acquisition of knowledge of a new subject. It must therefore develop systematically, express itself with complete clarity and try to convey its meaning equally to the student who likes to learn from a mathematical formula and to one who prefers verbal explanations. It must also be indexed and cross-referenced in such a manner that the student can locate quickly any section on which he wishes to refresh his memory. By these criteria Mr. Spiegelman's text-book must be judged a good one.

The book opens with a short introductory paragraph followed by a very important one dealing with the collection of census statistics and vital statistics. It shows reproductions of the census schedule used in the United States in 1950 and of the forms of birth and death certificate currently in use. The explanation of the method adopted at the 1950 census is interesting in that it shows the differences from the method used for the 1951 census in the United Kingdom. It is described as the *de jure* method, and involves the collection from a household of particulars of its usual members, not those who happen to be under the same roof on the census date. The information is collected by trained paid enumerators over a period of about four weeks; the form is not filled in by the householder. This is a very interesting chapter, comparing as it does the advantages and disadvantages of different methods of census taking, and illustrating some of the uses made of sampling for obtaining supplementary information at a census, for checking on the quality of the enumeration, or even as a substitute for a complete enumeration. The remainder of the chapter deals with the collection and publication of vital statistics, and to complete the picture, the chapter following deals with errors in the statistics collected and how they are adjusted for.

There follow three chapters dealing with mortality. The first discusses the conventional measures of mortality, examines mortality differences according to age, sex, race, marital status, geographical location and social-economic status, shows the part played by various causes of death, and explains methods of standardizing the mortality experiences of communities to enable comparisons to be made. The second deals with the construction of life tables, a subject with which the actuarial student in this country will be familiar from his detailed study of it elsewhere. Its inclusion here, however, is quite logical and it will be of value to the non-actuarial student whose familiarity with these techniques cannot be taken for granted. The third chapter in this group deals with mortality projections and discusses both generation trend and time trend methods of approach.

The chapter on morbidity statistics deals adequately with a very difficult subject; the definition and measurement of sickness and disablement present problems not found in the realm of vital statistics. Morbidity is often said to be out of place under the heading of demography, but this seems altogether too narrow a viewpoint. Quite apart from the direct association between illness and death, the well-being of the members of a population is very important and a study of the quality of life is a natural corollary to its quantitative measurement in years.

Family formation, composition and dissolution and fertility and reproduction are discussed very thoroughly as befits this important aspect of demography. Of marginal interest, perhaps, is the finding that "When age of mother and order of birth are both taken into account, the ratio of males to females at birth did not seem to rise in the United States during World War II, contrary to general impression that this ratio rises in wartime".

Population distribution and migration receive rather fuller treatment than in this country, which may be a consequence of the federal system in the United States and the need for separate figures for each State.

In the chapter on the working population, the treatment is not restricted to the requirements of differential mortality and fertility, but goes much further in its analysis and includes such matter as unemployment and labour mobility and turnover. It is admitted by the author that although the emphasis is on the demographic characteristics of the working population, the economic features are dealt with also. The result is a very interesting and comprehensive chapter, and it would be less than just to cavil at its extending beyond the bounds of pure demography.

The last chapter deals at some length with population estimates and projections. It spends too much time, perhaps, on mathematical methods which are somewhat crude in their application and does not sufficiently pursue the component method. It is necessary for some purposes to prepare population projections not only by age and sex but also by marital status, and the problems involved in obtaining the last-named subdivision are considerable. It is mentioned that "labour force participation by women is influenced in considerable degree by their marital status", but no explanation is given of how to obtain a projection by marital status. Whatever marriage rates may be assumed for the future for men and women in the light of current experience or otherwise, it will be a matter of some difficulty to obtain from them numbers of married men and women which correspond in future years. It would have been a valuable addition to the subject of population projections had this difficulty been discussed, and possibly a practical method of approach demonstrated for the guidance of actuarial students. C. M. STEWART.

10.—*Operations Research for Management*, Vol. II. Edited by Joseph F. McCloskey and John M. Coppinger. Baltimore: Johns Hopkins Press, 1956. (London: Oxford University Press.) xxxvi, 563 pp. 8¾″. 63*s.*

A couple of years ago Johns Hopkins University Press issued a text book on Operations Research. This book consisted of a series of papers on the various applications and techniques of the subject written by specialists in the different fields. Success deservedly followed this venture and the publishers have now issued a second volume.

Like its predecessor, this volume consists of papers given at the Operations Research Seminar at Johns Hopkins University. The layout is now slightly different in that chapters are grouped in three sections—Case Histories, Methods and Information Handling. This has the effect of improving the integration of the book although the linkage between successive chapters within a section is not always well established.

The book has as introduction a paper titled "From Plato to the Linear Program" by the Earl of Halsbury, Managing Director of the National Research Development Corporation. This is a most stimulating survey of the interaction between science and the community, and pleads for a bridge between operational research and social science. The book is almost worth purchasing for this article alone.

The section on Case Histories opens with a number of chapters on traffic problems. Three of these may be termed "classic" studies. Firstly there is the work of Edie on Traffic Delays at Toll Booths which won the Lanchester Prize for 1954 as the best published operational research work in that year. The second of these papers is an investigation in the New York subway fare structure, attempting to optimize fare structure on the grounds of efficiency, collection cost, revenue and income distribution. This paper is most interesting for the light it sheds on the interaction between a transport system and the life of a community. It is unfortunate that, as the author says, "It must be admitted that the influence of the report has so far been limited to a *succès d'estime*: economists have spoken flatteringly of it; in political and transit circles it appears to have become more of a political football and an object of derision". Such reactions will not be strange to any industrial research worker! The third traffic subject in this section is the work of Eddison and the British Iron and Steel Research Association on iron ore handling in which the optimum layout and equipment for unloading ore at ports are derived.

The section on methodology has chapters on measurement, the design of experiments, organization, queueing, linear programming, game theory and Monte Carlo methods. The final chapter in this section is a philosophical analysis of the relationship between operational research, planning and value by West Churchman, one of the United States's leading operational research workers.

For some years past the Massachusetts Institute of Technology has carried out research on Information Handling. A number of papers on this topic are grouped in the final section. The problem here is a study of the management of communications. What channels of communication within a "body" are best for the task the "body" has to accomplish? Early work on this was devoted to experiments on teams set various tasks in which their communication patterns were laid down *a priori*. From this has stemmed a whole field of enquiry—mostly on the laboratory or controlled experimental approach from which a number of fairly firm conclusions emerge. This work is important for all who have the direction of teams but it will need linking up with reality—the theory now needs testing in the hurly burly of industry.

As can be seen the book covers a wide field of industries, of types of problem and of methods of solution. Like its predecessor, it is concerned with mapping out the ground of operational research and as such it will be of interest both to the operational research specialist, who wants to know the work done in particular fields, and to the scientist in other related fields who wants to know something of what the operational research people are up to. However, if operational research is a science of its own, then it is capable of a logical connected exposition of its theory with practical examples and case studies thrown in as illustrations. Whether or not this stage has been reached is open for debate but there is still a case for someone somewhere to write the first integrated account of this young and exciting science.

B. H. P. RIVETT.

11.—*Distribution's Place in the American Economy since* 1869. By Harold Barger. National Bureau of Economic Research. Princeton University Press, 1955. (London: Oxford University Press.) xviii, 222 pp. 9″. $4.50.

This volume is a valuable addition to the series of National Bureau publications dealing with production, employment and productivity in American industry. Mr. Barger has played an important part in the writing of the earlier volumes in the series, having been author and co-author respectively of the volumes on *The Transportation Industries*, 1889–1946 (1951) and on *American Agriculture*, 1899–1939 (1942). Much of the present volume is firmly based on the results of work done, mostly at the Bureau, by the author himself and by Carson, Kuznets, Shaw, Stigler and others.

The scheme of the book is straightforward. The first part, which deals with productivity in distribution, contains three chapters on the growth of labour input, distributive output and labour productivity. The second part presents the results of the author's comprehensive researches into the history of distribution costs in the United States since 1869. It consists of four chapters dealing respectively with measures of the cost of distribution, channels of distribution, trends in margins and measures of total distributive spread by kind of retail outlet.

The text takes up no more than 100 pages. Another hundred are given over to four highly valuable appendices. Two of these appendices contain detailed descriptions of sources and methods used in the text tables concerned with employment, earnings, labour income and value added in distribution. Another, of 45 pages, lists periodicals and other sources from which retail and wholesale margin data were drawn, chronologically and by kind of retail and wholesale business. The final appendix provides a bibliography of pre-1919 periodicals in the distribution field.

The claim made in the preface, that "the earlier figures (i.e. those in respect of the period before World War I) represent the clearest picture of that period that can now be, or perhaps ever will be, assembled for the particular field to which they relate", is not likely to be questioned. The interest and value of the new material provided on trends in margins and in channels of trade over a long period are very great, and will be further enhanced if it proves practicable to match them with data for other countries, in due course.

Mr. Barger lays particular stress on three of his findings. The first of these is that the proportion of the labour force engaged in distribution has increased continuously, from 6 per cent. in 1870 to 16 per cent. in 1950. In explaining this, Mr. Barger invokes four factors: (i) the scope for a specialized industry concerned with distribution has increased through time with the growth of specialization in production, (ii) output per man has increased less rapidly in distribution than in production, owing to a slower change in its technology and a relatively slower growth in capital per worker, (iii) hours of work declined more in distribution than in commodity production, and (iv) the quantity of distributive services provided per unit of goods distributed has increased. The fourth of these explanations is put forward more tentatively than the others, and is the only one in the list which is likely to be questioned. On Mr. Barger's own showing, service at the point of sale has diminished with self-service, packaging and minor processing within distributive establishments has declined, credit furnished has become less and there has been no trend in the extent of delivery services. While there has been an improvement in store facilities (e.g. air conditioning, concealed lighting, escalators in larger establishments) it is not clear that this is a factor which increases labour requirements within the distributive trades themselves This leaves only the increases in free home trials of merchandise, in return privileges and in other "adjustments" (i.e., free service and repair and replacement of parts whether or not defective) to set against the declines in services already listed. It is not easy to see why Mr. Barger should be led, by this evidence, to conclude that a net increase had occurred in the quantity of services provided, per unit of goods, by distributors themselves. Moreover, Mr. Barger fails to consider the important possibility that, at a given standard of service, labour requirements in distributing a given quantity of goods may well have fallen owing to increases in the average size of transactions, as real income and wealth increased. The scattered data available on numbers of retail transactions suggest that this has probably been the case. It is worth stressing this, because it may well account for much of what Mr. Barger finds surprising about his second main finding, namely, that, in the face of the paucity of technological innovations in the industry, output per manhour in distribution rose steadily by as much as 1 per cent. per annum in distribution over the period he studied. (Mr. Barger measures output in distribution by the volume of goods passing through retail outlets, valued at manufacturers' prices and weighted by distributive margins.) The same point also has relevance to the third of Mr. Barger's main findings, namely, that the share of total distribution costs and profits in relation to retail sales has remained practically unchanged at around 37 per cent. since 1920, having previously shown a rather slow increase. This result is certainly in contrast with what had usually been assumed. One wishes that Mr. Barger had commented on it more consecutively than he has. If efficiency in distribution has lagged behind its increase in commodity production, the price, per unit, of distributive output must be expected to have risen. But if work required to be done per unit of retail sales has in fact fallen, it is possible to account for the recent constancy of distribution costs in the United States which Mr. Barger finds. In general, it is on the very intractable problems of output and efficiency measurement in distribution that the book is least enlightening. But this must not be allowed to obscure its solid merits. Mr. Barger has placed all students of the distributive trades very greatly in his debt.

JOHN KNAPP.

12.—*Method of Construction and Calculation of the Index of Retail Prices*. Ministry of Labour and National Service. London: H.M.S.O., 1956. 40 pp. 8¼". 1*s*. 9*d*.

This booklet covers the same topics as its predecessors which described the Interim Index of Retail Prices. It gives the background leading up to the introduction of the index, describes its scope and structure in simple terms and gives details of the collection of prices and the way in which they are combined to form the monthly index. The presentation, however, is much improved by combining in one section all the information on the methods used for particular series and in one appendix the items priced and the weights attributed to them.

The most important difference between the new index and the Interim Index of Retail Prices is its coverage, which now amounts to nearly nine-tenths of all households, or about one-tenth more than previously. Instead of covering only "working class" and small salary earners medium salary earners are also included. The only households excluded are those in which the head of the household earned more than £20 a week in 1953 and those most dependent upon National Insurance and National Assistance payments. With the increasing scope of the official index, it is to be hoped that the budget enquiry results may be analysed to see whether there is a case for publication of more restricted indices, e.g. for rural areas; for particular occupations; or to which wages might be more appropriately linked. Since even the preliminary results showed a different expenditure pattern by those excluded from the index there is already an excellent case for publishing a separate index covering those households who receive most of their income in pensions and supplements to them. Publication of more than one index might also help to dispel the idea that the Index of Retail Prices measures the (one and only) "cost of living".

In other respects the new index simply improves upon the old. The change to the more up-to-date 1953 distribution of purchases and the wider coverage have led to considerable alterations in the weighting of the various groups. It is disappointing, however, to see that adjustments have had to be made to the results of the expenditure enquiry in order to obtain the weights not only for alcoholic drink and tobacco but also for meals consumed outside the home, sweets and chocolates, ice cream and soft drinks. The reason is presumably forgetfulness in recording rather than the desire to minimize "vicious" expenditure which applies to the former commodities. No information is given on the size of the adjustments, but this does not look well for the general accuracy of the survey or for the value of detailed breakdowns of its results. The index is now much more detailed: the number of separate commodities and services priced has increased by about one-third to 350, the number of separately weighted sections is now ninety-one instead of fifty, ten group indices and thirty-five series within the groups are published instead of nine and twenty-five respectively. The further consideration promised in the 1952 Report of the Cost of Living Advisory Committee (Cmd. 8481) has led to a much extended and improved index for housing. No change in the method of dealing with seasonal commodities has been found to be worth while. The problem of forced substitutions disappeared with the end of rationing. (Although the argument that most people are restricted in their choice of housing to what others have decided to build seems to the reviewer a stronger argument in favour of taking as an indicator *average* current rent and rates than the one quoted, i.e. that the improved quality of new housing roughly only offsets the depreciation of the old.)

As before, the index relates to the Tuesday nearest to the 15th of each month and publication is in the third week of the following month.

RITA J. MAURICE.

13.—*The Social Accounts of the Welsh Economy*, 1948 *to* 1952. Edited by Edward Nevin. Cardiff: University of Wales Press [1956]. xiv, 17 pp. 11″. 3*s*. 6*d*.

Much labour and ingenuity have gone into this attempt to answer some of the unsolved problems of a regional economy. Scarcely a single Welsh statistic exists which has not been utilized. A lucid note on The Interpretation of Social Accounts is followed by six Summary Tables and ten Detail Tables. The Statistical Appendix indicates the derivation of about 150 different items, and the degree of reliability which the editor attaches to each estimate. On the whole these estimates are reasonable, and there is little to which grave exception can be taken. The statement that the "possible error" in the estimates of fixed capital formation is "probably 5–6 per cent." seems to be rather optimistic; and the frequency with which the Welsh labour force (or some other indicator) has been expressed as a percentage of the United Kingdom labour force, and this ratio applied to the relevant Blue Book figure for the United Kingdom, leaves one with the feeling that the implicit assumption of some kind of equivalence in the Welsh and United Kingdom economies is being made rather often in a study whose *raison d'être* is to reveal differences. But when one considers the nature of the available data one sees that sometimes the only alternative was to make no calculation at all.

It is when Dr. Nevin gathers in the threads, and constructs his summary tables, that he goes astray. In his Introduction he points out the dangers of residuals, and shows that if a residual item of £5m. has been derived by subtracting £100m. from £105m., then a 5 per cent. error in one of these component figures will lead to an error of 100 per cent. in the residual. "Accordingly", he says, "we have taken the precaution of having items derived as residuals printed in bold-faced type". Bold-faced type is, in fact, used on only six of the twenty occasions on which it should be used.

This alone is bad enough, but even if the proper type had been used there would have been some strange results. In Table II, for example, Personal Saving is estimated at £15m. for 1951. But if a five per cent. possible error is attached to the component figures, then this residual item becomes anything between *minus* £37m. and *plus* £71m. To print this as £15m. in bold type and still call it "Saving" is just not good enough. A barrel of mixed fruit is not to be called "Pears" simply because, if any pears exist, then officially they are in that barrel. When one considers that these tables are (as the notes make clear) intended largely for the use of amateur statisticians, one wonders all the more why this item has not been called "Residual error, including saving".

Table V sets out to show "Transactions with The Rest of the World". Because of the misleading way in which the figures are printed, Welsh "net imports" seem to jump from £168m. in 1949 to £232m. in 1950. In fact, these figures mean nothing at all, as a careful study of the table will reveal. Quite apart from this, almost every figure in the table is, in fact, a residual; and if we permit a possible error of as much as 20 per cent. (which is probably charitable) we find that the "net investment abroad", which is a residual of residuals, could be anything in 1952 between *minus* £127m. and *plus* £113m. It is printed as *minus* £7·5m.

Table I calculates the G.N.P. from the income side and separately from the expenditure side. Two sets of figures (in bold-faced type) are residuals from Table V. All other items are derived from the detail tables or by some independent method. But the amazing thing is that the two sides balance! Two estimates of the G.N.P. are unlikely to balance exactly even once; but for Dr. Nevin they balance five times out of five! All I can suggest is that possibly he has derived the differences between what he calls "imports" and "exports and net income received from abroad" as a pure residual between the income and expenditure sides of the table; and then estimated one or the other of these items by the incredible jugglery of Table V. But there is no clue at all in the notes; and Table I itself attributes these figures to Table V, not Table V to these figures. All this is, indeed, most puzzling.

To say that because Tables V and VI (Combined Capital Account) balance "then all is well" is just not true. It only shows that the faults are not attributable to the adding machine. It is a pity that the pains that have gone into the detail tables have been accompanied by Summary Tables and Introductory Notes which pay lip-service to caution and then create an air of spurious accuracy. Economic statistics are often sired out of ingenuity by ambition; but on this occasion, ambition has made demands so far exceeding the legitimate potentialities of the data that even the more respectable estimates must be treated with reserve. J. PARRY LEWIS.

14.—*Childbirth, Complications and Mortality of Obstetrical Patients in British Columbia* (Health Statistics Research Survey). Prepared by Zygmunt Lehoczky. Victoria, B.C., 1955. x, (118), 1 pp. 14½″.

In 1953 the public health authorities of Canada and British Columbia initiated a Health Statistics Research Project, the first stage to be concerned with maternity and its complications and the second with infant morbidity. The report on the first stage, now completed, contains over 700 pages of tables, and in this volume Dr. Lehoczky presents a summary of the main findings.

The Province of British Columbia has a population of such a size that the maternity records for a single year provide a numerically adequate basis for fundamental research on the normal processes and hazards of birth. There are several other features which are helpful to research of this kind, such as the presence of an Asiatic and an Indian group among the main population of mixed European stock, and the fact that no less than 97 per cent. of all confinements took place in hospital in the year reviewed. Most of the data for this study were drawn from standard hospital admission-discharge records. Unfortunately these records were deficient in a number of important respects. For example, they contained no information on the mother's parity, and they could not be relied upon to distinguish multiple from single births, or even live- from stillbirths. Information on the last two items was, however, obtained from the civil registration records of the Province. A more serious problem was the lack of any means of collating the separate records for women who were admitted to hospital more than once during pregnancy, childbirth and the puerperium. (Dr. Lehoczky makes some suggestions for a reform of the World Health Organization list of diseases by which these three phases of the same process would be treated as a unit.) To add to these difficulties, the records were only available for some 44 weeks of 1953, so that derivation of the essential figures for the full registration year necessarily involved some guesswork.

Even in this summary version of the report the quantity of information presented is somewhat indigestible, and one wishes that the author had clothed his somewhat stark presentation of the facts with more discussion of his material and more comparisons with the experience of other countries. Can it really be the case that the stillbirth rate in British Columbia is 40 per cent. below the renowned Danish standard? Does the very low reported incidence of forceps delivery (about one-seventh of that in a contemporary English survey) give a correct account of the situation, or is it due to incomplete recording?

It was hardly to be expected that a study based solely on existing official records would add anything fresh to our knowledge of the causes of stillbirth, congenital malformation or infant death. Nevertheless, by building up the total population of pregnancies at risk Dr. Lehoczky puts the loss of young life in a truer perspective than is obtained when, as is usual, pregnancies terminating before the twenty-ninth week are ignored. Even when no allowance was made for unrecognized losses in early pregnancy the number of abortions was found to exceed the combined total of stillbirths and first-week deaths by more than four to one. The author has attempted to get some clues to the causes of foetal death in this somewhat neglected period by means of a detailed analysis of the other diagnoses associated with abortion. But this does not lead very far since in the great majority of cases of abortion there was no secondary diagnosis at all.

No such frustrations are encountered in the analysis of the service requirements of different types of maternity case and the consequent estimation of the number of beds needed for a community of specified sex and age composition. The experience of British Columbia should be of assistance to planners in areas where the maternity service is less well-equipped.

D. HEWITT.

STATISTICAL NOTES

(1) British Official Statistics

The index of retail prices compiled by the Ministry of Labour and National Service which was 102 (January 17th, 1956 = 100) in August 1956 remained at that figure in September and rose to 103 in October and November. The rise in October was due mainly to higher prices for bread with some increases in rents and in the price of beer. These were partly offset by lower prices for eggs. The detailed figures of the weights used in calculating the index and the indices for the different commodity groups were as follows:

	Food	*Alcoholic Drinks*	*Tobacco*	*Housing*	*Fuel and Light*	*Durable Household Goods*	*Clothing and Footwear*	*Transport and Vehicles*	*Miscellaneous*	*Services*	*All Items*
Weights:	350	71	80	87	55	66	106	68	59	58	1,000
Aug. 14th, 1956	101·7	101·9	105·3	103·2	101·1	101·3	100·6	102·4	103·2	105·2	102·3
Sept. 18th, 1956	100·8	101·9	105·3	103·7	101·9	101·3	100·9	102·6	103·2	105·4	102·1
Oct. 16th, 1956	101·8	102·6	105·3	104·5	102·4	101·3	101·0	103·1	103·5	105·6	102·7
Nov. 13th, 1956	102·5	102·6	105·3	104·7	104·5	101·4	101·1	102·9	103·4	105·9	103·1

A supplementary calculation shows that, on the basis of June, 1947 = 100, the index at November 13th was 158, compared with 157 at October 16th, September 18th and August 14th. This supplementary calculation will not be made after the end of 1956.

The Ministry of Labour index of weekly wage rates which was 165 in August (June 30th, 1947 = 100) remained at that figure in September, October and November. In manufacturing industries the figure rose from 163 in August to 164 in the remaining months. At the end of November the indices for all industries and services were 164 for men, 169 for women and 181 for juveniles. For manufacturing industries alone the figures were 161, 173 and 182.

The total working population and the numbers in civil employment in the four months ended October, 1956, were as follows:

(*Thousands*)

	Total Working Population			*Numbers in Civil Employment*		
Date	*Males*	*Females*	*Total*	*Males*	*Females*	*Total*
July, 1956	16,143	7,869	24,012	15,266	7,789	23,055
Aug., 1956	16,194	7,907	24,101	15,282	7,817	23,099
Sept., 1956	16,190	7,910	24,100	15,272	7,812	23,084
Oct., 1956	16,173	7,910	24,083	15,247	7‘804	23,051

The number of persons on the unemployment registers of the Employment Exchanges fell by 18,900 in September and rose by 6,700 in October and by 10,500 in November. The total for November represented 1·2 per cent. of the estimated total of employees in Great Britain. The percentages in the different Regions ranged from 0·6 in the North Midlands to 2·2 in Wales and 2·3 in Scotland.

The following is a sex analysis of the figures:

Number of Unemployed Persons on the Registers of Employment Exchanges

Date	*Men and Boys*	*Women and Girls*	*Total*
Aug. 13th, 1956	181,703	84,838	266,541
Sept. 17th, 1956	158,826	88,814	247,640
Oct. 15th, 1956	160,081	93,992	254,073
Nov. 12th, 1956	167,517	97,061	264,578

Of the total of 264,578 in November, 73,693 had been unemployed for not more than two weeks, 79,759 for two to eight weeks and 99,458 for over eight weeks, while 11,668 were temporarily stopped. In the eight weeks ended November 14th 313,000 vacancies were filled by the Employment Exchanges. The number of vacancies unfilled at this date was 300,000, 18,000 less than in the previous month.

The numbers of insured persons absent from work owing to sickness, including self-employed as well as employed, were 789,200 in August, 835,900 in September, 873,800 in October and 910,400 in November. The numbers of employed persons absent owing to industrial injuries were 52,300, 57,000, 59,500 and 60,900.

An analysis is published in the *Ministry of Labour Gazette* for December of the size of manufacturing establishments. The figures are based on returns from 56,313 establishments with more than 10 employees, covering a total of 7,888,000 workers. They show that nearly one half of this total are employed by 13,844 firms with between 100 and 1,000 workers each. Of the remainder about 20 per cent. are with 41,339 firms having less than 100 workers and about 30 per cent. in 1,130 establishments of over 1,000 workers each. An analysis is given by Regions and by industry groups.

(2) OTHER STATISTICS

The International Statistical Institute has resumed its studies on international statistics of large towns which it undertook before the war. In collaboration with the International Union of Local Authorities a new series of publications has been planned of which the first two volumes have recently been published. Their scope is limited to towns of over 100,000 inhabitants in Europe and Egypt. The first volume, *Population and Vital Statistics 1946–1951* (126 pp., 15 florins) gives, in spite of the title, births for 1937, 1938, 1946–51 and deaths and marriages for 1937, 1938, 1948–51, as well as population estimates for 1850, 1875, 1900, 1925, and 1950, and distribution by sex, age and marital status for the latest census. The second volume *Housing and Building Statistics 1946–1953* (68 pp., 15 florins) gives statistics of dwellings by ownership, date of construction, number of inhabitants, number of rooms and facilities as well as data on newly constructed dwellings in 1934 to 1938 and 1946 to 1953 (230 towns are covered).

As most international statistical publications deal almost exclusively with countries as a whole, there is a need felt for a collection of municipal statistics. This need has been admirably met by these two volumes which are to be followed by others dealing with economic characteristics, public utilities and transport, tourism, and education.

A revised version of the report submitted to the eighth International Conference of Labour Statisticians on *International Comparisons of Real Wages* has now been published by the International Labour Office (89 pp., 6*s*.). It describes the methods by which consumer prices, consumption patterns, and the various types of money wages can be combined to yield measures of the relative purchasing power of wages. The methods proposed are essentially the same as those used by the International Labour Office before the war and described in this *Journal* (1930, pp. 398–441), and in fact the "theoretically desirable formulae" discussed in Chapter III of this report are identical with those given (as formulae A and B) on p. 437 of this reference. The only important difference is in the concept of money wages. It is now recommended that for wages, "earnings adjusted to include social benefits" should be used, as such benefits "particularly since the second World War have expanded enormously", and some data are given showing that "total benefits as percentage of average earnings" may reach in some countries 50 per cent.

No international real wage comparisons are made at present by the International Labour Office, but the report describes and gives the results of such enquiries which have been recently made, particularly by the four Northern Countries, the European Coal and Steel Community, etc.

The latest issue of the *Economic Bulletin for Europe* (Vol. 8, No. 2, August 1956) is chiefly devoted to an article on "Recent developments in trade between Eastern and Western European countries". Tables are given showing the value (in dollars) of total imports and exports between each Western and each Eastern country for 1954, 1955 and 1956 (1st quarter), as well as tables giving the data by commodity groups. The text discusses the pattern of east-west European trade and the changes in the composition of eastern European exports in the last few years. Imports into, and exports from Western Europe are estimated to have increased from 1954 to 1955 by about 30 per cent. and 12 per cent. respectively in value (25 per cent. and 6 per cent. in volume). The volume of trade between the two regions is estimated to be only about 45 per cent. (imports) and 66 per cent. (exports from Western Europe) of the volume of 1938. Trade in foodstuffs has shown little change in 1955, the increases being confined chiefly to fuels and manufactures from Eastern Europe and to raw and semi-manufactured goods from Western Europe.

The changes made in the International Statistical Classification of Diseases, Injuries and Causes of Death in 1948 have made it possible to compile statistics on the various aspects of mortality from accidents in different countries. The World Health Organization has therefore published a study on *Mortality from Accidents* (*Other than Transport*)* which gives the statistics for the years 1950 to 1953 for about 20 countries based on the Intermediate List of 8 principal categories, the Detailed List of 74 titles yielding figures which are often too small, especially when given by sex and age, to have statistical significance.

After summary tables comparing deaths from all accidents and deaths from transport accidents with those from causes other than transport, detailed tables and mortality rates are given by sex and age-group. Appendix tables give data for a number of countries analysing deaths by fall and by accidental poisoning, and of infants under 1 year, as well as data then available for 1954 for 3 countries.

* *Epidemiological and Vital Statistics Report*, Vol. 9, No. 1, 1956 (131 pp. 13*s*. 6*d*. Geneva, 1956).

STATISTICAL AND ECONOMIC ARTICLES IN RECENT PERIODICALS

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Annals of Human Genetics—

November 1956—Heredity and rheumatic fever. Some later information about data collected in 1950–51: *A. C. Stevenson* and *E. A. Cheeseman.*

Biometrika—

December 1956—Studies in the history of probability and statistics. III. A note on the history of the graphical presentation data: *E. Royston*; IV. A note on an early statistical study of literary style: *C. B. Williams.* A goodness of fit test for spectral distribution functions of stationary time series with normal residuals: *A. M. Walker.* Sufficiency conditions in regular Markov chains and certain random walks: *J. Gani.* Some asymptotic distribution theory for Markov chains with a denumerable number of states: *C. Derman.* On the accuracy of weighted means and ratios: *G. S. James.* On estimating the latent and infectious periods of measles: *N. T. J. Bailey.* Significance tests for a variable chance of infection in chain-binomial theory: *N. T. J. Bailey.* On the variation of yield variance with plot size: *P. Whittle.* On the construction of significance tests on the circle and the sphere: *G. S. Watson* and *E. J. Williams.* Notes on bias in estimation: *M. H. Quenouille.* An introduction to some non-parametric generalizations of analysis of variance and multivariate analysis: *S. N. Roy* and *S. K. Mitra.* A two-sample distribution-free test: *A. R. Kamat.* Sequential analysis applied to certain experimental designs in the analysis of variance: *W. D. Ray.* Lognormal approximation to products and quotients: *S. R. Broadbent.* A rejection criterion based upon the range: *C. I. Bliss, W. G. Cochran* and *J. W. Tukey.* Confidence intervals for a proportion: *E. L. Crow.* Serial correlation in regression analysis: *G. S. Watson* and *E. J. Hannan.* Revised upper percentage points of the extreme studentized deviate from the sample mean: *H. A. David.* Exact linear sequential tests for the mean of a normal distribution: *J. Taylor.* On the sum of squares of normal scores: *H. Ruben.* On the moments of the range and product moments of extreme order statistics in normal samples: *H. Ruben.* On estimating binomial response relations: *F. J. Anscombe.* Existence and uniqueness of a uniformly most powerful randomized unbiased test for the binomial: *A. A. Blank.* A note on the circular multivariate distribution: *G. S. Watson.* The fitting of regression curves with autocorrelated data: *N. A. Huttly.* Bounds for the variance of Kendall's rank correlation statistic: *A. Stuart.* A note on the theory of quick tests: *D. R. Cox.* A note on the signs of gross correlation coefficients: *O. Reiersøl.* The estimation of the mean of a censored normal distribution by ordered variables: *P. G. Moore.* Likelihood function for capture-recapture samples: *N. E. G. Gilbert.* Tables of Poisson power moments: *J. B. Douglas.*

British Journal of Sociology—

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Faculty of Actuaries, Transactions—

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December 1956—Statistical works in early American statistics courses: *P. J. Fitzpatrick.*

American Statistical Association, Journal—

September 1956—The optimum strategy in blackjack: *R. R. Baldwin, W. E. Cantey, H. Maisel* and *J. P. McDermott.* Two sequential tests against trend: *G. E. Noether.* Equivalence of two estimates of product variance: *D. W. Gaylor.* The economics of the President's economic reports: *A. C. Harberger.* Analytical graduation of fertility rates: *I. Lah.* Asymptotic efficiencies of a nonparametric life test for smaller percentiles of a gamma distribution: *J. E. Walsh.* Estimates of bounded relative error for the ratio of variances of normal distributions: *S. Reiter.* Machine computation of higher moments: *N. Jaspen.*

Annals of Mathematical Statistics—

September 1956—Some continuous Monte Carlo methods for the Dirichlet problem: *M. E. Muller.* Contributions to the theory of rank order statistics: two-sample case I: *R. Savage.* The admissibility of Hotelling's T^2-Test: *C. Stein.* A method of constructing partially balanced incomplete block designs: *J. W. Archbold* and *N. L. Johnson.* A note on combined interblock and intrablock estimation in incomplete block designs: *D. A. Sprott.* Asymptotic minimax character of the sample distribution function and of the classical multinomial estimator: *J. Dvoretzky, J. Kiefer* and *J. Wolfowitz.* A comparison of tests on the mean of a logarithmico-normal distribution with known variance: *N. V. Severo* and *E. G. Olds.* Two-sample procedures in simultaneous estimation: *W. C. Healy, Jr.* Distribution of the sum in random samples from a discrete population: *C. K. Tsao.* On the distribution of the number of successes in independent trials: *W. Hoeffding.* Variances of variance components: *J. W. Tukey.* Matrix methods in components of variance and covariance analysis: *S. R. Searle.* On the hypothesis of no "interaction" in a multi-way contingency table: *S. N. Roy* and *M. A. Kastenbaum.* The bias in certain estimates of the parameters of the extreme-value distribution: *B. F. Kimball.* A queueing system with χ^2 service-time distribution: *D. M. G. Wishart.* Errors in normal approximations to the *t*, *r*, and similar types of distribution: *J. T. Chu.* On the stochastic independence of two-second-degree polynomial statistics in normally distributed variates: *R. G. Laha.* The WAGR sequential *t*-test reaches a decision with probability one: *H. T. David* and *W. H. Kruskal.* The use of generalized probability paper for continuous distributions: *H. Chernoff* and *G. J. Lieberman.* The modified means square successive difference and related statistics: *S. Geisser.* On the Tukey test for the equality of means and the Hartley test for the equality of variance: *K. V. Ramchandran.* Remarks on some nonparametric estimates of a density function: *M. Rosenblatt.* Sufficient statistics with nuisance parameters: *D. A. S. Fraser.* A note on balanced incomplete block designs: *W. A. Thompson, Jr.* The efficiency factor of an incomplete block design: *O. Kempthorne.* The null distribution of the difference between the two largest sample values: *J. St.-Pierre* and *A. Zinger.* A certain class of solutions to a moment problem: *L. Weiss.*

Biometrics—

September 1956—Extensions to missing plot techniques: *H. R. Thompson.* The analysis of a 3 × 6 experiment arranged in a quasi-Latin square: *G. E. Hodnett.* A note on the

4*n* series of factorial experiments: *P. J. Claringbold.* Some small sample tests of significance for a Poisson distribution: *C. R. Rao* and *I. M. Chakravarti.* Matched pairs in sequential trials for significance of a difference between proportions: *W. Z. Billewicz.* A note on the rank analysis of incomplete block designs—applications beyond the scope of existing tables: *O. Dykstra.* Extension of multiple range tests to group means with unequal numbers of replications: *C. Y. Kramer.* Simplified LD_{50} (or ED_{50})) calculations: *H. J. Horn.* A simple method for fitting an asymptotic regression curve: *H. D. Patterson.* Some problems of experimental design and technique with perennial crops: *S. C. Pearce.*

Econometrica—

October 1956—Resource allocation for economic development: *H. B. Chenery* and *K. S. Kretschmer.* A fundamental theorem for the aggregation problem of input-output analysis: *J. C. H. Fei.* Application of linear programming to competitive bond bidding: *J. K. Percus* and *L. Quinto.* Complementarity and long-range projections: *P. J. Verdoorn.* An eclectic approach to the pure theory of consumer behavior: *H. Wagner.* The theory of capital and its time-measures: *C. A. Blyth.*

Industrial Quality Control—

August 1956—Establishing control of tire cord testing laboratories: [*F. Akutowicz* and *H. M. Truax.* Quality control of tubular steel products: *W. T. Rogers.* Statistical quality control in a press shop: *L. S. Eichelberger.* Fundamentals of analysis of variance, Part I—the analysis of variance (ANOVA) model: *C. R. Hicks.*

September 1956—Fundamentals of analysis of variance, Part II—the components of variance model and the mixed model: *C. R. Hicks.* Statistical quality control will work on short-run jobs: *H. R. Harrison.*

October 1956—The nature of the standard control chart‘, and some of it's competitors: *E. G. Olds.* Quality control as applied to continuous processes: *J. F. Occasione.* Fundamentals of analysis of variance, Part III—nested designs in analysis of variance: *C. R. Hicks.* A teaching aid for regression, correlation, analysis of variance, and other statistical techniques: *J. J. Moder, Jr.*

November 1956—Quality control of complex assemblies: *P. A. Robert* and *M. C. Bugonian.* Five bases of a standard: *W. A. Shewhart.* Have you tried BIPP?: *C. R. Hicks.* Comparisons of two fractions defective: *F. E. Satterthwaite.*

Journal of Political Economy—

October 1956—Iron and steel in the balance of world power: *J. B. Parrish.* Income, expenditure, and the terms of trade for New Zealand, 1929–38: *J. H. Auten.* Some foundations of economic development in the Upper East Tennessee Valley, 1850–1900. II: *W. H. Nicholls.*

Management Science—

July 1956—Operations engineering: *E. R. Bowerman* and *S. B. Littauer.* Some models of inventory and an application: *E. Naddor.* A fruitful application of static marginal analysis: *H. W. Karr* and *M. A. Geisler.* A mathematical model for integrated business systems: *I. J. Lieberman.* An optimum geographical distribution of publicity expenditure in a private organization: *A. P. Zentler* and *D. Hyde.* Geographical distribution of production in multiple plant operations: *J. D. Dillon.*

October 1956—Management planning and control—what next?: *P. Kircher.* Formalism, realism and management science: *J. S. Minas.* On the caterer problem: *W. Prager.* Solving the transportation problem: *L. R. Ford Jr.* and *D. R. Fulkerson.* Optimal design and utilization of communication networks: *R. E. Kalaba* and *M. L. Juncosa.* The allocation of aircraft to routes—an example of linear programming under uncertain demand; *A. R. Ferguson* and *G. B. Dantzig.* A model for the optimal programming of railway freight train movements: *A. Charnes* and *M. H. Miller.* Variety in retailing: *W. J. Baumol* and *E. A. Ide.* Capital equipment analysis: the required rate of profit: *M. J. Gordon* and *E. Shapiro.* Dynamic programming and the smoothing problem: *R. Bellman.*

Milbank Memorial Fund Quarterly—

July 1956—The reliability of longitudinal surveys: *J. W. B. Douglas* and *J. M. Blomfield.* Social and psychological factors affecting fertility. XXXII. The number of children desired at the time of marriage: *H. V. Muhsam* and *C. V. Kiser.*

Operations Research—

October 1956—The theory of search: Part II, target detection: *B. O. Koopman.* Estimation of reliability growth in a complex system with a Poisson-type failure: *H. K. Weiss.* A method of computing survival probabilities of several targets versus several weapons: *J. I. Robertson.* Weapon-system cost analysis: *G. H. Fisher.* Inventory management of slow-moving parts: *A. C. Heyvaert* and *A. Hurt.*

Quarterly Journal of Economics—

November 1956—Accounting for depreciation in the Soviet economy: *R. W. Campbell.* A probabilistic theory of consumer behavior: *R. E. Quandt.* A complete capital model involving heterogeneous capital goods: *P. A. Samuelson* and *R. M. Solow.* The shifting of the corporation income tax in a growing economy: *D. Bodenhorn.* Shift working: an application of the theory of the firm: *K. J. W. Alexander* and *J. Spraos.*

BELGIUM—

Bulletin de l'Institut de Recherches Économiques et Sociales—

November 1956—Les religieuses du diocèse de Tournai. Étude sociologique de leur provenance: *M-T. Mattez.* L'intérêt pour l'art musical. Essai d'analyse sociologique: *R. Hoebaer.*

CZECHOSLOVAKIA—

Aplikace Matematiky—

Vol. 1, Part 1—On an estimate of the parameter δ from normal population. (Czech text, Russian and English summaries): *V. Dupač* and *M. Josífko.*

Vol. 1, Part 2—Stratified sampling (Czech text, Russian and English summaries): *J. Hájek.*

Vol. 1, Part 3—Median estimates (Czech text, Russian and English summaries): *F. Zítek.*

Czechoslovak Mathematical Journal—

Vol. 1, Part 6—Linear estimation of the mean value of a stationary random process with convex correlation function (Russian text, English summary): *J Hájek* A decision function: *V. Fabian* (English text). Note on K. Menger's probabilistic geometry: *A. Spacek* (English text). Linear estimation of the mean value of a stationary random process with convex correlation function: *J. Hájek* (Russian text, English summary).

FRANCE—

Journal de la Société Statistique de Paris—

July–September 1956—La statistique religieuse: *M. le Bras.* Statistique et causalité (à propos des statistiques de causes de mort): *M. Aubenque.* Publication des premiers résultats détaillés du recensement de la population de mai 1954: *J. Antoine.*

Population—

July–September 1956—Niveau de vie des familles suivant le nombre d'enfants: *Y. Martin.* La population algérienne—croissance, niveau de vie, investissements: *L. Tabah.* La population de l'Union Soviétique—situation, croissance et problèmes actuels: *A. Sauvy.* Méthode mécanographique pour établir la généalogie d'une population—application à l'étude des esquimaux polaires: *J. Sutter* and *L. Tabah.*

Revue de Statistique Appliquée—

Vol. IV, No. 3—La recherche opérationnelle et ses applications: *G. Th. Guilbaud.* Les lois de probabilité de Halphen: *G. Morlat.* Le problème des évacuateurs de crues de Serre-Poncon: *M. Labaye.* Quelques examples concrets d'application des méthodes statistiques au contrôle des fabrications et à la recherche aux États-Unis: *F. Bastenaire.* La statistique comme instrument de connaissance: *Y. Mainguy.*

ITALY—

Annali dell'Istituto di Statistica—

Vol. XXVII—Sull'interpolazione analitica e sulla distribuzione dei redditi: *P. Mazzoni.* Sulla rappresentazione analitica delle serie logistiche per mezzo della curva normaleintegrale: *G. Chiassino.* Sull'interpolazione della curva logistica: *G. Chiassino.*

Bollettino del Centro per la Ricerca Operativa—

No. 1 (1956)—Considerazioni intorno alla ricerca operativa: *F. Brambilla.* La programmzaione dell'ottima sequenza di produzione: *B. Giardina.* Distribuzione dei "vuoti" in processi stocastici di tipo Poissoniano generalizzato: *L. Castoldi.*

No. 2—La programmzaione lineare applicata a problemi di distribuzione: *A. Restelli.* Uno schema teorico per la descrizione e classificazione dei processi mentali di apprendimento: *L. Castoldi.* Proof of the transitivity property in Giardina's rule for solving a scheduling problem with two stages and two objects: *S. Danö.*

No. 3—Piano di produzione e piani di investimento: il problema del "Budget" ottimo: *B. Giardina.* Apparenze conduttive e propagative in processi Markoviani speciali (Processi conservativi e processi livellativi): *L. Castoldi.*

Statistica—

July–September 1956—Analisi statistica della frequenza del cosiddetto ritmo theta nell'elettroencefalogramma umano normale: *G. Barbensi* and *M. Nistri.* L'analfabetismo in Italia: *L. Bergonzini.* Riflessi stagionali della recessione nel mercato cinematografico: *L. Bergonzini.*

October–December—La misura della componente casuale delle vincite al Totocalcio: *A. Naddeo.*

JAPAN—

Bulletin of Mathematical Statistics—

March 1956—On the confidence interval of the extreme value of a second sample from a normal universe: *A. Kudo.* On the invariant multiple decision procedures: *A. Kudo.* Unbiased estimation based on sufficient statistics: *Y. Washio*, *H. Morimoto* and *N. Ikeda.* The operational calculus and the estimations of functions of parameter admitting sufficient statistics: *T. Kitagawa.* Some aspects of stochastically approximative analysis: *T. Kitagawa.*

Institute of Mathematical Statistics, Annals—

Vol. VIII, No. 1—On the distribution of the Hotelling's T^2-statistics: *M. Siotani.* Simple estimators of the parameters of exponential distributions when samples are censored: *B. Epstein.* On the evaluation of the sampling error of a certain determinant: *H. Aoyama.* Note on preference and axioms of choice: *H. Uzawa.* On confidence limits for the ratio of regression coefficients: *B. M. Bennett.* On the distribution of the product Γ-distributed variables: *H. Akaaike.* Note on the sums of the independent variates of K. Pearson's type V: *I. Higuti.* Note on optimal machine setting: *Y. Suzuki.*

NETHERLANDS—

Statistica—

Vol. 10, Nos. 3–4—Verdelingsvrije toetsen voor twee steekproeven en de metode der 2 × 2-tabel: *C. van Eeden.* The frequency of industrial accidents: *F. X. Thomissen.* Enige rangcorrelatieschema's: *Ph. van Elteren.*

SPAIN—

Trabajos de Estadistica—

Vol. VII, Part II—Regresión en mediana y la programación lineal: *J. Bejar.* Perks' distributions and their role in the theory of Wiener's stochastic variables: *J. Talacko.* Distribucion de la suma algebraica de variables de Poisson: *J. Romani.* Note on the Poisson index of dispersion: *B. M. Bennett.* Métodos y problemas de la investigación operativa: *S. Rios.* El método del "simplex" de la programmación lineal: *R. San Juan Llossa.* Inspección por muestreo: *G. Arnaiz.*

SWITZERLAND—

Mitteilungen der Vereinigung schweizerischer Versicherungsmathematiker—

November 1956—Elektronische Rechenmaschinen in Versicherungsbetrieben: *O. W. Spring* and *P. Leepin.* Zur Anwendung der kollektiven Risikotheorie in der schweizerischen obligatorischen Unfallversicherung: *R. Latscha.* Änderungen in der Grundgesamtheit der Betriebsunfallkosten: *H. Bühlmann* and *W. Hartmann.* Sterbegesetze, für welche der Barwert einer Verbindungsrente gewisse Mittelwerteigenschaften erfüllt: *E. Rufener.* Beiträge zur Theorie der charakteristischen Funktionen stochastischer Verteilungen: *H. Loeffel.*

LIST OF ADDITIONS TO THE LIBRARY

Since the issue of Part IV, 1956, the Society has received the publications enumerated below.

I.—OFFICIAL PUBLICATIONS

United Kingdom. *Central Office of Information.* Social services in Britain. 2nd ed. London, H.M.S.O., 1956. iv, 75p. 9¼″. 3*s*. 6*d*. (C.O.I. Ref. Pamphl. 3)

General Register Office. Census 1951. England and Wales. General tables, comprising population, ages and marital condition, non-private households, birthplace and nationality, education. London, H.M.S.O., 1956. xiii, 224p. fold.table. 13″. 52*s*. 6*d*.

Census 1951. England and Wales. Housing report. London, H.M.S.O., 1956. cxxix, 150p. fold.table. 13¼″. 60*s*.

Census 1951. England and Wales. Occupation tables. London, H.M.S.O., 1956. xv, 672p. fold.table. 13¼″. 147*s*.

Code of surgical operations, with classifications of radiotherapy and anaesthetic procedures. London, H.M.S.O., 1956. v, 276p. 9¼″. 15*s*.

Medical Research Council. The hazards to men in ships lost at sea, 1940–44; R. C. McCance, C. C. Ungley, J. W. L. Crosfill and E. M. Widdowson. London, H.M.S.O., 1956. v, 44p. 9¾″. 5*s*. (M.R.C. Spec. Rep. Ser. 291)

A study of attitudes to factory work; S. Wyatt and R. Marriott, assisted by R. A. Denerley, H. Campbell, J. Walker, Yvonne Kapp and F. G. L. Stock. London, H.M.S.O., 1956. vii, 115p. 9¾″. 10*s*. (M.R.C. Spec. Rep. Ser. 292)

Medical Research Council. Whooping-Cough Immunization Committee. Vaccination against whooping-cough; relation between protection in children and results of laboratory tests. . . *Brit. Med. J.* (1956), **2**, p.454. 9p. 11″.

Ministry of Education. Report of the Burnham Committee . . . on scales of salaries for teachers in primary and secondary schools maintained by local education authorities, England and Wales, 1956. London, H.M.S.O., 1956. viii, 62p. 9¾″. 3*s*.6*d*.

Ministry of Housing and Local Government. Rent control: statistical information. London, H.M.S.O., 1956. 4p. 9¾″. 4*d*. (Cmnd. 17)

Ministry of Labour and National Service. Method of construction and calculation of the index of retail prices. London, H.M.S.O., 1956. 40p. 8½″. 1*s*. 9*d*.

Ministry of Transport and Civil Aviation. The British Transport Commission: proposals for the railways. London, H.M.S.O., 1956. 43p. 9½″. 1*s*.9*d*. (Cmd. 9880)

Tonnage measurement of ships: instructions for the guidance of surveyors. London, H.M.S.O., 1956. 83p. 8½″. 4*s*.

Monopolies and Restrictive Practices Commission. Report on the supply of linoleum. London, H.M.S.O., 1956. vi, 114p. 9¾″. 5*s*. (H.C. 366)

Northern Ireland. General Register Office. Census of population of Northern Ireland, 1951. County of Antrim . . . County of Armagh . . . Belfast County Borough . . . County of Down . . . County of Fermanagh . . . County and County Borough of Londonderry . . . County of Tyrone. Belfast, H.M.S.O., 1953–54. 7 pts. 13½″. 7*s*.6*d*.ea.

Census of population of Northern Ireland, 1951. General report. Belfast, H.M.S.O., 1955. lii, 95p. fold.tables. 13″. 15*s*.

Royal Commission on Common Land. Minutes of evidence 5–12 . . . London, H.M.S.O., 1956. 8 pts. 9¾″. 17*s*.9*d*.

Scotland. Department of Agriculture. Scottish agricultural economics: some studies of current economic conditions in Scottish farming. Vol. VII. Edinburgh, H.M.S.O., 1956. 50p. 9¾″. 3*s*.

Social Survey. Consumers' expenditure surveys in 1955. London, Central Office of Information, 1956. 46p. 13″. 17*s*.6*d*.

University Grants Committee. Methods used by universities of contracting and of recording and controlling expenditure. London, H.M.S.O., 1956. 45p. 9¾″. 2*s*. (Cmnd. 9)

Argentina. *Direccion Nacional de Estadistica y Censos.* Informe demografico de la Republica Argentina, 1944–1954. Buenos Aires, 1956. 290p. 10¾″.

Brazil. *Conselho Nacional de Estatistica.* Brésil d'aujourd'hui. Rio de Janeiro, 1956. xiii, 162p. fold.map. photos. 7″.

Burma. *Directorate of Labour.* Sample survey of labour force in Rangoon: a study in methods; by John Clement Koop. Rangoon, Superintendent, Union Government Printing and Stationery, 1955. [iii], 101p. 2 fold.tables. 11″.

Canada. *Dominion Bureau of Statistics.* Ninth census of Canada, 1951. Vol. X. General review and summary tables. Ottawa, 1956. Various paging. 11″. $3.

Cuba. *Banco Nacional.* Opinion autorizada sobre la economia nacional juicio sobre el 1er semestre de 1956. *Latin-Amer. Busin. Highlights* (1956), Sept. [11]p. 11″.

Consejo Nacional de Economia. La estimulacion industrial en Cuba. La Habana, 1956. [iv], 142p. 8¼″. (Publ. del Consejo Nacional de Economia 14)

France. *Institut National de la Statistique et des Etudes Economiques.* Catalogue des périodiques français et étrangers conservés à la bibliothèque. Paris, Person, 1956. 175p. 9½″.

Institut National de la Statistique et des Etudes Economiques & Ministère de la France d'Outre-Mer. Service des Statistiques. Le recensement de la population non originaire des territoires d'outre-mer en 1951. *Bull. Statist. Outre-Mer* (1956), Supp. Sér. Etudes 33. ii, 118p. 10¾″.

Frankfurt am Main. *Statistisches Amt.* Die soziale Lage der Studierenden an der Johann Wolfgang Goethe-Universität, Frankfurt am Main, im Sommersemester, 1955; von Dr. Paul W. Reuff. Frankfurt, 1956. 39p. 11¾″. (Sonderheft 6, *Statist. Monatsber.*, 1956, **18**)

Germany (Eastern). *Staatliche Zentralverwaltung für Statistik.* Statistisches Jahrbuch der Deutschen Demokratischen Republik, 1955. 1er Jahrgang. Berlin, VEB Deutscher Zentralverlag, 1956. xii, 292p. maps. 9″.

India. *Office of the Registrar General.* Census of India. Papers No. 3 and 4, 1955. Economic classification by age groups—1951 census: Uttar Pradesh. [v], vii, 350p. 4*s.* Mysore. [v], 200p. 3*s.* New Delhi, 1956. 10¾″.

Census of India. Papers No. 2–5, 7, 9, 1956. Means of livelihood and industries tables. 1941 census, on *Y*-sample. Bihar. [iii], 91p. 1*s.*9*d.* Orissa. [iii], 65p. 1*s.*9*d.* West Bengal. [iii], 65p. 1*s.*6*d.* Assam. [iii], 69p. 1*s.*6*d.* Bombay. [iii], 111p. 1*s.*9*d.* Punjab. [iii], 75p. 1*s.*6*d.* New Delhi, 1956. 10½″.

International Labour Office. Co-operation: a workers' education manual. Geneva, 1956. viii, 157p. 9½″. 9*s.*

International comparisons of real wages: a study of methods. Geneva, 1956. vi, 89p. 9½″. 6*s.* (Studies and Reports N.S. 45)

Japan. *Bureau of Statistics.* Family income and expenditure in post-war Japan, 1946–1955. [Tokyo, 1956.] [x], 193p. 10¼″.

New South Wales. *Bureau of Statistics and Economics.* Population review No. 1. (Based on censuses of 1954 and previous years). Growth and distribution of population. Sydney, 1956. 25p. 12½″.

New Zealand. *Department of Statistics.* Census of 17 April, 1956. Interim returns of population and dwellings. Wellington, 1956. 27p. 12¼″.

Organisation for European Economic Co-operation. The chemical industry in Europe. Paris, 1956. London, H.M.S.O. 211p. 9½″. 14*s.* (Trends in Economic Sectors, 3rd year)

Oil: the outlook for Europe. Paris, 1956. 115p. 9½″. 8*s.* (Trends in Economic Sectors, 1955)

Pulp and paper statistics, 1955. Paris, 1956. 73p. fold.diagr. 10¾″. 10*s.*

Recent developments in the cement industry in O.E.E.C. countries. Paris, 1956. 33p. 10¾″. 4*s.* (Trends in Economic Sectors, 1955)

Statistical quality control. Paris, 1956. 89p. 9½″. 5*s.* (Project 148)

Statistics of the hides and skins sector. Paris, 1956. 31p. 10¾″. 4*s.* (Trends in Economic Sectors, 3rd year)

European Productivity Agency. Market research methods in Europe. Paris, 1956. 139p. 9½″. 9*s.* (Project 261)

Spain. *Instituto Nacional de Estadistica.* Indice cronologico de legislacion estadistica, años 1813–1956. Madrid, 1956. 236p. 8½″. (Publ. del 1er centenario de la estadistica española).

Numeros indices mensuales del movimento anual: nuevo metodo para calcular numeros indices mensuales de la produccion industrial y, en general, de cualquier actividad economica; por Angel Alcaide Inchausti. Madrid, 1956. 98p. 10½″.

Publicaciones estadisticas de España. Madrid, 1956. 202p. 8½″. (Publ. del 1er centenario de la estadistica española)

Resumen historico de la estadistica en España; por Anselmo Sanz Serrano ... Centenario de la estadistica oficial española, 1856–1956. Madrid, 1956. xv, 324p. 9½″.

Sudan. *Population Census Office.* The 1953 pilot population census for the first population census in Sudan. Khartoum, 1955. 183p. photos. fold.map. 4 fold.tables. 11″. 20*s.*6*d.*

First population census, 1955/56. First interim report. 52p. fold.map. 4*s.* Supplement to interim reports. 37p. 10*s.*3*d.* Khartoum, 1956.

Switzerland. *Eidgenössisches Statistisches Amt.* Recensement fédéral des logements, 1er décembre, 1950. Berne, 1955. 79*, 248p. 11¾″. (Statistiques de la Suisse 277)

Union of South Africa. *Bureau of Census and Statistics.* Population census, 8th May, 1951. Vol. I. Geographical distribution of the population of the Union of South Africa. vi, 121p. maps (2 fold.) 24*s*.6*d*. Vol. III. Religions of the white population of the Union of South Africa, together with 1946 census figures for all races of the population. vi, 77p. 15*s*.6*d*. Pretoria, [1956]. 12¼″.

Union of Soviet Socialist Republics. *Tsentral'nyĭ Statisticheskyĭ Upravlenie.* Vestnik statistiki: organ Tsentral'nogo Statisticheskogo Upravleniya pri Sovete Ministrov S.S.S.R. No. 1. Yanvar'-Fevral', 1956. Moskva, 1956. 96p. 10¼″. *r*.5.

United Nations. *Department of Economic and Social Affairs. Population Branch.* Manuals on methods of estimating population. Manual III. Methods for population projections by sex and age. New York, 1956. London, H.M.S.O. vii, 81p. 11″. 5*s*.6*d*. (Popul. Stud. 25)

Economic Commission for Latin America. Economic survey of Latin America, 1955: including an essay on government income and expenditure, 1947–1954. New York, 1956. xiii, 176p. 14*s*.

Economic Commission for Latin America & others. Pulp and paper prospects in Latin America . . . Report of the Latin American meeting of experts . . . ; sponsored by the secretariats of the Economic Commission for Latin America, the Food and Agriculture Organization . . . and the Technical Assistance Administration . . . New York [etc.], 1955. v, 465p. 11″. 32*s*.

Educational, Scientific and Cultural Organization. Basic facts and figures: international statistics relating to education, culture and mass communication, 1956. Paris, 1956. 96p. 9½″. 5*s*.

Economics in the United States of America: a review and interpretation of research; by Rutledge Vining. Paris, 1956. 62p. 9½″. 5*s*. (Documentation in the Social Sciences: Economics)

Food and Agriculture Organization. Selected problems of production and trade in the Near East. Rome, 1956. vi, 167p. 9″. 8*s*.9*d*.

International Monetary Fund. The first ten years of the International Monetary Fund. Washington, [1956]. [v], 49p. 11″.

United States. *Bureau of Labor Statistics.* Economic forces in the U.S.A. in facts and figures: the United States, its people, its labor force, and its economy; prepared by the U.S. Department of Labor, Bureau of Labor Statistics, in cooperation with International Cooperation Administration. 4th ed. Washington, 1955. iii, 104p. 10¼″. 55*c*.

Department of Agriculture. Analytical tools for measuring demand; by Richard J. Foote and Karl A. Fox. Washington, Superintendent of Documents, 1954. ii, 86p. 10¼″. 50*c*. (Agric. Handbook 64)

National Science Foundation. Federal funds for science. V. The federal research and development budget, fiscal years 1955, 1956, and 1957. Washington, Superintendent of Documents, 1956. [ii], 47p. 10¼″. 35*c*. (NSF 56–19)

Research and development by nonprofit research institutes and commercial laboratories, 1953; prepared for the National Science Foundation by the Maxwell Research Center, Syracuse University. Washington, Superintendent of Documents, 1956. ix, 81p. 10¼″. 50*c*. (NSF 56–15)

Science and engineering in American industry: final report on a 1953–1954 survey . . . ; prepared for the National Science Foundation by the U.S. Department of Labor, Bureau of Labor Statistics. Washington, Superintendent of Documents, 1956. vii, 119p. 10¼″. 70*c*. (NSF 56–16)

II.—AUTHORS AND MISCELLANEOUS

Abruzzi, Adam. Work, workers and work measurement. New York, Columbia University Press, 1956. London, Oxford University Press. xvi, 318p. 9″. 60*s*.

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AN INQUIRY CONCERNING INTERVIEWER VARIABILITY

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INTRODUCTION

1. Previous field inquiries by the Research Techniques Unit at the London School of Economics have been chiefly, though not solely, concerned with sources of error arising from the sampling process itself. The stage has now been reached at which we feel that sampling error is fairly well understood and can be brought under control in normal operating conditions without undue cost; or at least, that the problems which it still presents are much less important by comparison with other types of error which affect a social survey. We are therefore planning inquiries into non-sampling errors such as arise, for example, from the fallibility of interviewers, imperfections in questionnaire design, or interactions between interviewer and respondent.

2. This paper describes the first of these inquiries and concerns the variability of interviewers. One of the major problems in designing the inquiry was to define clearly what it was about. A questionnaire which an interviewer completes after making contact with the right person may be defective for a number of reasons. Some of these, such as faulty questions, inadequate or misleading instructions, accidental or deliberate inaccuracy in the respondent's answers, are not his fault. When such factors are abstracted from discussion there may remain some variation due to the interviewer himself. We may distinguish three different contributory elements:

(a) *Accuracy.*—This is the extent to which the interviewer obeys his briefing instructions (so far as they concern the interview) and correctly records the information given by the respondent. The complementary quality is usually called interviewer *bias*, but interviewer *inaccuracy* would be better. An interviewer may be inaccurate without being systematically so, although in practice bias is a frequent source of inaccuracy.

(b) *Reliability.*—This notion, familiar to the psychologist, may be regarded as denoting the extent to which an interviewer will produce similar results under similar conditions. An interviewer, without being inaccurate, may vary his technique (consciously or unconsciously) and might produce somewhat different answers from the same respondent on two different occasions; provided that it were possible to put the same questions to the same respondent on two different occasions. The complementary quality *unreliability* may be regarded as a variance component of error specific to the interviewer.

(c) *Variability.*—Two interviewers, without being inaccurate or unreliable, may differ between themselves. The eliciting of responses in an interview situation is not a purely mechanical process of observation and there are perfectly legitimate variations in the responses which different interviewers will produce.

In terms more familiar to the statistician, the three types might be described as variability within an interviewer on the same occasion, within an interviewer on different occasions, and between interviewers on the same occasion.

3. Ideally, perhaps, an exploratory experiment should try to distinguish these sources of difference between interviewers. In practice, however, such distinctions are apt to be blurred and in any case we cannot measure reliability directly. It appeared to us that reliability and variability must sometimes be run together as a single "error" term. We would like to have measured accuracy separately, but this would have involved validation techniques which, after prolonged discussion, were felt to be impossible or undesirable on this occasion.* This inquiry is not, therefore, concerned with accuracy.

4. Interviewer variation has been studied by a number of workers in the United Kingdom, the United States and India. For a discussion and summary of the results reference may be made to Hyman (1954), especially chapter 6. It is not easy to summarize succinctly the findings of so many different workers in different fields, but very broadly speaking the situation seems to be this:

Putting aside errors due to cheating or idiosyncrasies of particular respondents (which may be important but are not under discussion here) the individual interviewer has a component of variation in the sense that, confronted with the same set of circumstances, he will not always reproduce the same *set of* results. Very different degrees of "reliability" in this sense have been reported.

Two interviewers working in similar circumstances may or may not reproduce similar results. The evidence here is conflicting, or appears so at first sight. Durbin and Stuart (1954) came to the conclusion that, within homogeneous grades of interviewer and questions of a factual nature, there was no appreciable evidence of differences among interviewers. Some of Mahalanobis's studies in interpenetrating sampling point in the same direction. A good deal depends here on what we regard as a homogeneous grade of interviewer.

Differences between experienced and inexperienced interviewers exist (Durbin and Stuart (1951), Booker and David (1952)) but are not always marked. The indications are that they concern response and failure rates rather than answers to relatively simple questions; the situation in regard to complicated questions and probing is unknown.

In this study we began with the assumption that some "unreliability" was present. The extent to which we found it will be briefly indicated below, but we hope to present a further study on another occasion. What we are primarily concerned with in this paper is the extent to which interviewers differ among themselves in similar circumstances—namely, with the variability of their variability. As we have pointed out above, it is not always easy to separate the component of error due to unreliability, which is idiosyncratic, and variability-between-interviewers, which is a group property. But most of the analysis and comment in the paper are directed towards the latter.

THE PLANNING OF THE INQUIRY

5. Two important factors are believed to influence the degree of variability among interviewers: first, the nature of the briefing; second, the types of question which have to be asked, coupled with the amount of probing permitted. It was therefore decided to have two types of briefing, B_1 and B_2. Interviewers in both groups received the same set of written instructions, which were fairly full and of the type normally used in practical surveys.† Interviewers of type B_1 received the instructions by post and were given no further briefing; those of type B_2 attended a briefing meeting, went out to test the questionnaires and came back to a further meeting to discuss their difficulties. (Many of the items in the questionnaires had been previously piloted at the drafting stage.)

6. Two different questionnaires, Q_1 and Q_2, were prepared on the same subject, which was chosen to be television because of its current general interest.‡ Q_2, which is given in Appendix

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* For example, there are legal and ethical questions involved in the recording of interviews without the knowledge of respondent or of interviewer or of both.

† Space forbids us to set out these instructions in full, but copies can be supplied on request.

‡ It must be made perfectly plain that the primary purpose of the inquiry was not to discover facts about television which would be representative of people as a whole or of the country as a whole. No attempt will be made to interpret the results as an inquiry into T.V.

B, made use of "open" questions and probing; Q_1 (given in Appendix A) was confined almost entirely to "closed" questions, i.e. questions for which the types of answers are pre-arranged in categories on the questionnaire.

7. Six organizations shared the field work: Attwood Statistics, the British Broadcasting Corporation Audience Research Department, the British Market Research Bureau, the Government Social Survey, Hedley's and Market Information Services. Each organization provided eight interviewers and each interviewer was given an assignment of 40 questionnaires. Each individual interviewer worked on the same questionnaire under the same briefing for his whole assignment. There should thus have been 1,920 interviews. To avoid missing plots so far as possible, and to simplify later analysis, substitutes were permitted. In the event, 47 interviewers completed 40 questionnaires each. The remaining interviewer defaulted and by the time all the inquiries after her had been made it was too late to replace her. No interviews from this person have been included in the analysis and there is thus one set of 40 missing from the 1,920 originally intended.

8. Up to this point, the scope of the inquiry almost determined itself. On practical grounds more than two questionnaires or two briefings would have made the inquiry unmanageable. The number of organizations was determinate. To make an adequate measure of variability possible, the individual assignments could not be much fewer than 40. The only remaining point for determination was the area of the inquiry and, for practical reasons again, it was decided to confine the field work to London. To allow for possible variations in social pattern four boroughs were chosen, Ealing, Wandsworth, Stepney and Ilford.

9. There were 24 pairs of interviewers; at least, there should have been had not one interviewer failed to complete her assignment. We had six organizations (O), two questionnaires (Q), two briefings (B), and four areas (A). A complete factorial would have involved 6×2×2×4=96 pairs of interviewers. Having only 24, we had to do some partial replication and confounding. It was ultimately decided to lay out the design in four blocks, corresponding to the four areas, as follows:

	A_1			A_2	
O_1	Q_1	B_1	O_1	Q_2	B_2
O_2	Q_1	B_1	O_2	Q_2	B_2
O_3	Q_1	B_2	O_3	Q_2	B_1
O_4	Q_2	B_1	O_4	Q_1	B_2
O_5	Q_2	B_2	O_5	Q_1	B_1
O_6	Q_2	B_2	O_6	Q_1	B_1

	A_3			A_4	
O_1	Q_1	B_2	O_1	Q_2	B_1
O_2	Q_1	B_2	O_2	Q_2	B_1
O_3	Q_1	B_1	O_3	Q_2	B_2
O_4	Q_2	B_2	O_4	Q_1	B_1
O_5	Q_2	B_1	O_5	Q_1	B_2
O_6	Q_2	B_1	O_6	Q_1	B_2

10. The sample was chosen by the Government Social Survey at random from the 1955 electoral lists. To concentrate travel and to save expense two or three adjacent wards were chosen in each borough and a set of 720 names taken from them. (Only 480 completed assignments were required; the additional names were for wastages and substitutions.) Actually, the average rate of substitution was 21·65 per cent. An examination of the records was subsequently made to see if any interviewers were using unduly high numbers of substitutes or had unusual failure rates. The following are the results:

Numbers of Interviewees (Out of 40) Substituted for Originally Chosen Names

	Number of Interviewees														
	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	*Total*
Q_1 . .	—	2	1	1	4	5	4	1	1	2	2	—	1	—	—	24
Q_2 . .	1	—	2	2	5	5	1	1	—	4	—	1	—	—	1	23
Total .	1	2	3	3	9	10	5	2	1	6	2	1	1	—	1	47

The mean proportion for the total 47 is ·2165. The variance of a binomial with this value of p and an exponent of 40 is 6·785. The actual variance in the bottom line is 8·905. On the whole, it seems that, with one or two possible exceptions, the interviewers were homogeneous in substitution rate.*

11. The verbal briefing was carried out on May 25th, 1955, and the postal briefing at various times during the succeeding month. The field work was done mainly in June, some early in July and one set in August. Especial care was taken with the coding, which was done under the direction of Miss Harris of the Government Social Survey.

METHODS OF ANALYSIS

12. Some measure of variability between interviewers had to be devised. The data, it will be remembered, were in the form of polytomized qualities, the classes ranging from 2 to 12 in number; even in the few cases where measurable variables were concerned (as in income and size of viewing screen) the data were available only in coarse groups. Moreover, the classes in some of the multiple classifications were not orderable. It seemed inevitable, therefore, that we should use some form of a chi-squared statistic.

The interviewers were comparable in pairs. Suppose the answers to any one question are coded in k categories (not necessarily the same for all questions). If the numbers of respondents falling into these categories are $x_1, x_2, \ldots x_k$ for interviewer X and $y_1, y_2, \ldots y_k$ for interviewer Y we may array the data in a 2 × k table

x_1	x_2	. . .	x_k	n_1
y_1	y_2	. . .	y_k	n_2

This table has $k - 1$ degrees of freedom and we took as the measure of variability the statistic

$$V = \chi^2/(k-1)$$

13. If the interviewers are in complete agreement in the sample $V = 0$. If they differ in the sample V is greater than zero, but a non-zero value may arise as a chance effect in sampling from the same population of respondents. In such a case the expected value of V is unity. As the two rows become more divergent V increases without limit. If we know that the sampling is from the same population we may therefore regard observed values of V in excess of unity with suspicion which increases as the excess becomes larger; "suspicion", that is, in the sense of suspecting genuine differences between the interviewers.

If $n_1 = n_2 = 40$ the calculation of V is relatively simple. In the case where $n_1 \neq n_2$ it is more troublesome and as several hundred values had to be computed we used an approximation given by Haldane (1955).

* The instructions on substitution were as follows:

Substitutes

When it has proved impossible to interview a named informant you *must* take a substitute. When you are given your Interview Record Sheets you will also be given Substitute Record Sheets. When you require a substitute it should be taken from these sheets, beginning with the first name on the list. *On no account must you use a neighbour or any person other than the one specified as a substitute.* Should you not be able to interview the named substitute after two recalls (three calls in all), or more, *then take the next name off your list of substitutes and so on.*

It is very important in this survey that the number of substitutes should be kept to an absolute minimum. *You should try to get every possible one of your original informants.*

In all these calculations we did not group frequencies, however small, although if there was a pair of empty cells, the number of degrees of freedom was reduced. It might have saved a little time to do some grouping and would have been more in accordance with the practice recommended in most text-books. But we are not sure that grouping is necessary, even in individual cases, and for the comparison of large numbers of values of chi-squared it did not seem to us worth while.

14. The placing of interviewers in pairs in districts implies that, to a large extent, inter-block effects will be absent from V. A comparison of the answers to various questions for the four districts was made and, broadly speaking, indicated that variability among areas was not greater than could be expected on a chance basis. For example, the total values of χ^2 on some questions were as follows:

Question	A_1	A_2	A_3	A_4
1 $(Q_1 + Q_2)$	10·83	6·19	17·00	23·37
2(*a*) (Q_2)	47·19	28·58	17·97	27·87
7 $(Q_1 + Q_2)$	48·26	29·33	50·00	71·46
14(*a*) (Q_1)	13·27	3·38	4·97	21·32
14(*d*) (Q_1)	18·27	6·64	5·85	26·68
14 (Q_2)	9·62	36·04	26·79	12·97

The values for A_4 (Wandsworth) for the early questions seemed to be higher than for the other three districts but the effect was not maintained and on the whole we felt it safe to ignore inter-area differential effects on the comparison of pairs of interviewers. We may then treat the 23 values of V for any question as a 2 × 2 × 6 factorial with one missing plot and analyse as:

	d.f.
Between questionnaires (Q)	1
Between briefings (B)	1
Between organizations (O)	5
Interactions:	
QB	1
QO	5
OB	5
Residual	4
	22

15. This analysis, however, is not entirely straightforward. There is first of all the effect of the missing plot. Secondly, the values of V (when systematic effects have been abstracted) need not have the same residual variances in all cases; it would be rather surprising if they did. Briefly, our procedure was as follows:

(*a*) After preliminary scrutiny, the values of V were tested in the first instance by an ordinary variance analysis with an estimate for the missing plot obtained in the usual way. (See, for example, Kendall, 1955, p. 229.)

(*b*) Where, in this crude analysis, significance appeared we went back to the V values and repeated the analysis by an exact non-orthogonal method. The actual technique used was a covariance adjustment described by Quenouille (1948).* It seemed to us very unlikely that a more exact analysis, on the "non-significant" approximate analyses, would product significance.

(*c*) If the non-orthogonal analysis gave no significances the analysis was allowed to stand for certain questions. But if significances appeared and in any case for questions 7 to 16, the values of V were transformed by taking logarithms and a variance analysis carried out on the transforms.

RESULTS

16. It may, in the first instance, be useful to look at the total values of χ^2 for each question. There is one value for each of the 23 cells. If the over-all picture were one of complete homogeneity (all interviewers alike, sampling from a single population) the expected value in each cell would be equal to the numbers of degrees of freedom. They have been summed over the 23 cells, together with the corresponding degrees of freedom, and are presented in Table 1.

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* For a discussion of the method in relation to missing plots generally see Kendall (1956).

TABLE 1

Values of χ^2, for each Question, Summed Over 23 Cells

Question	χ^2	*d.f.*	*Question*	χ^2	*d.f.*	*Question*	χ^2	*d.f.*
1	56	23	8(*c*)	123	72	16	217	99
2(*a*)	220	269	9(*a*)	63	42	17 Sex	33	23
2(*b*)	357	269	9(*b*)	67	50	17 Age	293	276
3	303	99	9(*c*)	67	30	17 M.S.	48	46
4(*a*)	111	95	10	104	101	18(*a*)	170	144
4(*b*)	242	164	11(*a*)	14	14	19(*a*)	189	170
5(*a*)	48	23	11(*b*)	84	55	20	210	112
5(*b*)	110	75	12(*a*)	67	45	21(*a*)	216	213
5(*c*)	228	166	12(*b*)	38	42	21(*b*)	168	124
6(*a*)	190	180	12(*c*)	122	100	21(*c*)	52	36
6(*b*)	67	60	13(*a*)	178	77	21(*d*)	73	29
7	199	23	13(*b*)	196	88	Number of calls	105	92
8(*a*)	128	80	14	n.a.				
8(*b*)	120	79	15	374	183			

In most cases χ^2 exceeds its expected value and in a number of cases significantly so. On the classification data (questions 17–21) the agreement is reasonably good except on question 20 and question 21(*d*) on which we comment below. For the rest, there seems on the whole something to be explained, certainly for some of the questions, and we feel justified in going on with the analysis.

17. Again, shortage of space prevents us from giving the data in such detail as we should like, but in Table 2 we give a typical specimen (for question 13(*a*)) of the set of 23 $2 \times k$ tables which arise from each question.

TABLE 2

Results for Question 13(a). *The Replies are Classified into Four Categories* ($k = 4$) *for Q_1 and Q_2. Reading from Left to Right the Categories are "Much","Little", "None", "Don't know", and, for Q_2 "Miscellaneous"*

Interviewers	Q_2						Q_1				
O_1	12221	34	3	0	2	1	11211	19	6	1	14
	12222	23	2	1	8	6	11212	12	7	0	21
	13121	17	1	0	18	4	14111	14	9	1	16
	13122	12	5	0	20	3	14112	20	11	0	9
O_2	22221	32	2	1	4	1	21211	25	8	0	7
	22222	28	0	0	7	5	21212	20	11	1	8
	23122	15	5	2	16	2	24111	26	7	1	6
	—						24112	16	11	5	8
O_3	32121	26	5	1	8	0	31111	16	7	4	13
	32122	19	11	2	8	0	31112	15	11	1	13
	33221	15	2	0	14	9	34211	1	6	1	32
	33222	13	7	1	15	4	34212	17	15	1	7
O_4	41221	12	7	2	11	8	42211	16	4	0	20
	41222	16	4	0	17	3	42212	16	9	0	15
	44121	14	4	1	18	3	43111	10	13	4	13
	44122	16	1	0	18	5	43112	18	5	1	16
O_5	51121	13	5	0	15	7	52111	22	14	1	3
	51122	24	1	0	9	6	52112	21	12	0	7
	54221	9	8	1	19	33	53211	25	9	0	6
	54222	22	2	1	9	6	53212	20	4	2	14
O_6	61121	14	0	2	15	9	62111	5	16	3	16
	61122	22	6	3	4	5	62112	15	7	1	17
	64221	15	0	4	14	7	63211	22	12	3	3
	64222	20	6	1	5	8	63212	22	12	0	6

TABLE 3

Values of V

Question	Q_2B_1						Q_2B_2						Q_1B_1						Q_1B_2					
	O_1	O_2	O_3	O_4	O_5	O_6	O_1	O_2	O_3	O_4	O_5	O_6	O_1	O_2	O_3	O_4	O_5	O_6	O_1	O_2	O_3	O_4	O_5	O_6
1	12·468	—	·157	1·455	3·66	·457	·457	·075	3·13	1·053	·313	2·051	16·807	2·222	4·012	·392	5·216	·157	·075	1·569	·524	·125	0	1·013
2(*a*)	1·059	—	1·333	·946	1·078	1·960	·675	·590	·486	1·222	1·218	·334	·93	·62	·30	·60	4·00	·27	1·54	·73	·70	·89	·89	·45
2(*b*)	1·025	—	1·688	·873	1·355	1·764	1·004	1·976	1·27	1·23	2·704	·656	2·06	1·42	·74	1·67	1·14	·70	1·37	·89	1·18	·53	1·44	2·14
Quotes 2(*a*)	1·689	—	3·136	·946	2·345	2·369	2·662	1·642	1·184	1·271	1·407	·78	1·12	2·03	1·31	·69	·56	1·14	1·88	·60	1·32	1·00	2·62	2·59
Quotes 2(*b*)	4·694	—	·648	1·979	·559	1·066	1·31	5·271	1·847	1·570	·444	·389	3·06	1·46	3·02	1·37	·84	1·40	6·84	1·00	2·23	2·46	1·06	4·73
3	8·271	—	2·177	2·017	1·347	2·331	1·887	3·160	26·501	1·756	1·966	2·353	2·82	1·41	2·87	2·67	1·32	4·77	1·85	2·44	1·13	4·34	1·41	3·54
4(*a*)	1·325	—	1·526	1·16	1·395	·980	1·013	1·820	1·076	1·096	·356	1·209	·60	1·65	1·02	1·65	2·43	2·30	·83	·73	1·00	·57	·42	·67
4(*b*)	1·599	—	2·699	2·062	1·096	2·612	1·633	1·138	·343	1·083	1·548	1·738	1·86	1·16	1·39	2·25	·58	1·50	·69	1·30	1·32	·83	1·81	1·40
5(*a*)	·201	—	5·115	6·764	·453	10·026	·208	1·818	·800	·457	·05	·201	6·65	2·09	·29	4·77	1·93	1·06	·01	·03	1·40	·22	·06	3·21
5(*b*)	4·544	—	1·409	4·362	2·749	2·557	1·409	3·362	·76	5·984	5·186	8·472	1·68	1·65	·66	·87	2·00	·94	1·85	·16	4·19	·81	2·27	·71
5(*c*)	1·80	—	2·01	1·16	·80	2·77	1·59	2·25	·68	2·04	1·36	·75	2·21	1·73	·92	1·08	1·05	·37	1·83	1·17	·71	·44	·58	1·91
6(*a*)	·78	—	1·26	1·13	·54	1·38	2·58	·34	·83	·75	1·07	·85	1·58	1·48	1·32	1·04	·62	·70	1·16	·45	·75	1·05	·85	1·19
6(*b*)	1·25	—	1·10	·45	·93	1·80	·97	0	2·89	1·46	·41	1·05	·90	·69	1·07	1·23	1·28	1·01	1·0	1·23	0	·98	·97	·07
7	9·825	—	·228	3·274	7·04	·833	5·333	13·333	0	4·266	41·813	·457	4·501	19·012	4·073	8·455	·503	9·825	15·238	16·807	2·4	·105	31·648	·065
8(*a*)	1·12	—	·25	·32	·54	1·97	1·28	2·61	1·87	2·33	2·02	·80	·40	2·08	3·18	·36	4·62	2·29	2·03	1·26	1·54	·92	4·62	1·02
8(*b*)	1·71	—	1·37	·71	·70	1·75	1·83	2·33	·55	1·24	1·47	3·14	·40	1·11	2·91	1·18	1·72	1·79	·51	·91	2·24	·98	1·10	·74
8(*c*)	1·02	—	·29	1·40	2·21	2·84	1·41	3·70	·24	1·46	1·90	2·48	1·22	4·18	1·34	1·00	1·08	2·30	·61	·69	·84	1·85	2·09	·91
9(*a*)	0	—	·13	·01	1·85	1·81	3·52	6·68	1·14	5·16	·82	·82	1·22	·71	·40	·77	1·57	·36	1·05	5·31	·90	1·23	·52	1·48
9(*b*)	·05	—	1·24	2·63	·57	1·68	1·91	2·09	·87	1·24	·53	·43	·24	·68	·01	·06	·44	5·44	1·89	1·95	·59	·30	4·45	1·01
9(*c*)	2·82	—	·05	·10	6·95	·83	8·09	·18	4·48	1·26	1·91	·27	1·61	1·97	2·17	5·32	·49	2·13	1·36	·60	·20	1·08	·48	·40
10	1·61	—	·76	·28	1·14	2·81	·97	1·25	1·07	·08	2·82	2·16	1·16	1·71	2·09	1·34	·42	1·53	2·32	1·17	1·77	1·21	1·01	1·54
11(*a*)	1·04	—	·08	0	·05	0	1·00	1·8	0	·89	0	0	0	·88	1·04	1·88	0	1·03	0	0	·88	0	0	0
11(*b*)	1·8	—	3·91	·72	·85	1·29	2·75	·10	·30	2·53	·14	1·18	·65	·78	·69	1·39	1·02	2·46	1·52	1·90	4·77	3·69	·62	·59
12(*a*)	1·96	—	1·10	1·40	2·38	3·15	1·50	·34	·65	1·53	·90	1·04	·02	2·74	1·85	1·2	1·53	3·39	1·07	·61	1·84	·34	2·55	1·80
12(*b*)	·04	—	·13	1·00	·54	2·82	1·53	·52	·52	1·97	·78	·01	·07	1·96	·60	0	·38	1·79	·67	·18	·41	·7	1·48	·06
12(*c*)	·30	—	·64	1·05	2·04	2·10	1·22	1·23	·60	1·05	1·24	1·07	1·59	2·02	·81	1·0	2·18	1·90	·59	·10	·95	1·00	1·87	1·14
13(*a*)	1·259	—	1·224	·858	2·505	3·872	2·623	1·688	1·469	1·737	3·406	3·211	1·406	2·074	·907	2·65	·926	3·184	1·353	·699	11·368	1·318	2·559	1·333
13(*b*)	3·23	—	·99	2·26	·63	8·10	1·05	1·71	2·96	1·49	2·69	·45	2·53	·70	1·26	·93	2·31	5·04	3·03	·91	6·55	·30	·49	2·62
15	2·58	—	1·17	·50	2·71	1·04	2·65	7·77	3·89	1·40	2·97	·96	·73	5·16	2·03	·76	·66	1·68	2·15	1·40	2·81	·25	·95	2·56
16	·45	—	1·79	·80	1·57	1·61	·65	1·67	·71	1·50	1·35	1·39	3·68	4·40	1·77	3·89	2·56	3·13	1·96	6·81	3·16	·86	2·98	5·23
17 Sex	·80	—	·20	·46	5·33	·05	4·53	2·49	6·76	·88	2·26	·83	·06	·22	·49	·05	·83	·20	·55	·21	2·58	·80	·82	1·82
17 Age	·86	—	·80	·62	1·94	·68	·67	·61	1·08	1·98	1·28	1·18	1·57	·68	1·18	·61	1·48	1·42	·86	·90	1·67	·79	·83	·58
M.S.	·79	—	0	·80	·95	·81	1·05	·14	2·08	·59	·78	2·92	·28	·13	3·48	·29	1·51	·79	·30	·31	1·30	1·16	2·06	2·41
18(*a*)	·68	—	1·70	1·51	2·79	1·02	2·49	·63	1·66	·96	1·61	·52	·69	·47	·90	·91	·52	1·01	1·25	·27	·99	·66	·72	2·54
19(*a*)	·52	—	·97	1·68	1·82	1·62	1·52	·70	1·76	·51	1·53	1·12	·51	·32	·79	1·44	·68	1·05	·78	·88	1·11	1·39	1·00	1·92
20	2·67	—	·66	·68	·86	1·67	4·47	2·72	2·16	·85	6·62	1·81	1·98	·68	1·82	·91	1·09	1·63	·18	·60	·73	·93	1·24	1·68
21(*a*)	2·19	—	1·02	·32	·98	·89	·63	1·06	1·10	·88	1·05	·87	1·03	·56	·75	·91	·64	1·01	1·90	1·35	·86	·82	1·76	·80
21(*b*)	1·22	—	1·38	·97	·35	1·85	·58	2·99	·87	·77	1·17	·88	1·75	1·84	2·41	3·29	·75	1·23	·62	1·42	1·83	·43	1·76	1·45
21(*c*)	1·25	—	1·87	·41	·85	·93	2·68	·17	1·23	·05	1·68	1·93	·57	5·39	1·54	4·61	·86	1·61	1·03	·67	·82	·02	1·42	3·95
21(*d*)	2·35	—	·10	0	·01	·89	·94	4·58	·84	·87	0	·26	1·36	·42	18·84	1·19	7·57	1·36	7·05	2·05	·20	·01	2·90	1·32
No calls	1·01	—	·89	·87	2·30	1·81	·53	1·92	2·29	·36	3·32	·70	2·50	1·06	1·91	·13	1·06	·51	·58	·08	·46	·72	1·17	·12

18. The 23 values of V obtained for each of the questions are given in Table 3.

19. As a preliminary to the variance analysis it may be useful to look at those questions where little difference between class-effects was to be expected. Table 4 shows the values of V for an even better case, the number of calls made by the interviewer. On each completed questionnaire a note had been made of the number of calls made on the informant before the interview had been effected. χ^2 was calculated on the distribution of this number of calls.

TABLE 4

Values of V on Number of Calls per Interviewer

	Organization					
	1	2	3	4	5	6
Q_1B_1	1·01	—	0·89	0·87	2·30	1·81
Q_1B_2	0·53	1·92	2·29	0·36	3·32	0·70
Q_2B_1	2·50	1·06	1·91	0·13	1·06	0·51
Q_2B_2	0·58	0·08	0·46	0·72	1·17	0·12

The mean of these 23 values is 1·13 (with $k = 5$) and they seem to present a fair picture of homogeneity. The analysis of variance is given in Table 5.

TABLE 5

Analysis of Variance of Table 2

	d.f.	*s.s.*	*Mean Square*
Q	1	2·6202	2·62
B	1	0·6767	0·68
O	5	5·0699	1·01
QB	1	0·6835	0·68
QO	5	4·1945	0·84
OB	5	2·0632	0·41
QBO . . .	4	2·6169	0·65
Totals . .	22	17·9249	0·81

The only mean-square suggestive of real variability is that between questionnaires, and this is not significant.

20. We look next at the classificatory data, questions 17 to 20, and the information about possession of a television set, question 21. The results of the variance analysis are summarized in Table 6.

TABLE 6

Analysis of Variance of Values of V Derived from Questions 17–21 *Inclusive*
(*Mean Squares*)

		Question									
	d.f.	17 (*age*)	17 (*sex*)	17 (*marital status*)	18*a*	19*a*	20	21*a*	21*b*	21*c*	21*d*
Q	1	0·03	13·14	0·40	1·49	0·24	6·09	0·01	0·31	1·21	46·57
B	1	0·01	8·21	1·16	0·02	0·22	3·34	0·16	1·17	2·86	7·12
O	5	0·31	2·62	1·69	0·29	0·38	1·72	0·23	0·93	1·24	10·42
QB . . .	1	0·43	0·73	0·41	0·40	0·22	8·70	0·43	0·20	1·06	18·99
QO . . .	5	0·24	1·83	0·55	0·70	0·18	1·53	0·03	0·78	1·46	14·19
OB . . .	5	0·31	3·84	0·50	0·45	0·21	1·34	0·13	0·65	5·28	18·56
QBO . . .	4	0·12	2·39	1·15	0·40	0·22	1·39	0·38	0·51	1·07	24·06
Value of k–1 .		12	1	2	6	8	9	10	5	2	2

Among these answers (which are not, of course, independent) very few effects stand out as possibly significant. The high values in column 21*d* are due to a single value of chi-squared of

18 obtained in one cell. This resulted from a large divergence in the numbers of T.V. sets *observed* to be in working order, by one pair of interviewers, and was perhaps due to the different times of day at which these interviewers were working. Among the rest, we seem to have homogeneity by age, marital status, etc., and the only matters calling for comment are the sex-differences by Q and B (Question 17 (sex)) and, possibly, the education-age differences on Q, B and QB (Question 20). We can see no reason why there should be any real sex-differences and education-differences and can only suppose that these "significances" are sampling effects such as one must occasionally expect.* The value of OB in column 21*c* may be a reflection of the fact that the verbal briefing would put interviewers more *au fait* with the nature of a station selector. On the whole, however, the data seem to agree fairly well with the hypothesis of homogeneity.

21. The remaining questions wherein comparison between questionnaires was possible may be considered in two groups: factual (as to T.V. viewing, interests, viewing practice); and knowledge of and opinions about commercial T.V. programmes (name of authority, whether advertisement should be permitted, etc.) and T.V. intentions (to convert the set, etc.).

In the first group (questions 1–6), although an analysis on V suggested certain significances, these disappeared in an analysis of log V; and the data appear to be homogeneous.

22. For the members of the second group (questions 7 to 16) we transformed the data by taking log 100 V as the variable. The following summarizes the analysis for questions 7 to 16 inclusive, except for question 14, where no comparison was possible.

TABLE 7

Summary of Analysis of Variance on Log 100 V *for Questions* 7 *to* 13, 15 *and* 16

Question	*Cases Exceeding Residual by a Factor* f			
	$1 \leqslant f < 2$	$2 \leqslant f < 3$	$3 \leqslant f < 4$	$f \geqslant 4$
7† . . .	O, QO, OB			
8(*a*) . . .	B, QO			
8(*b*) . . .	O, QB		QO	
8(*c*) . . .		Q, OB		O*, B, QO
9(*a*)† . . .	O	QB		B
9(*b*)† . . .	Q, O, QB, OB, OB	B		
9(*c*)† . . .		QB		
10† . . .	OB	Q		O*, QO*
11(*a*)† . . .	B, QO			
11(*b*)† . . .	O, OB	Q, QB		
12(*a*)† . . .	QB, QO, OB			
12(*b*)† . . .	O, QO			OB
12(*c*) . . .	OB	O		B
13(*a*)† . . .				
13(*b*)† . . .	OB			
15 . . .				
16 . . .	QO	O	Q	

The asterisk (*) means that the value is significant at the 5 per cent. level on an ordinary F test. A dagger (†) means that the form of question was identical in the two questionnaires.

23. This table has to be interpreted in the light of the questions themselves. No B difference emerges into significance. In a few cases it is suggestive, notably on 9(*a*) and 12(*c*). But 9(*a*) was identical on both questionnaires and there is no reason to suspect any difference; and 12(*c*) is an open-ended question on one questionnaire and closed or semi-closed on the other. It would

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* We have compared the substitution-rate for pairs of interviewers with the values of V for sex of informant. There is little relationship and the effect cannot, therefore, be attributed to the fact that some interviewers reject one sex and proceed until the other is substituted.

not have surprised us if the B-effects had been more marked, seeing that the discussion at the briefing meetings was fairly lengthy. It appears, however, that if the verbal briefing exerted any effect on the variability between interviewers, it was too subtle to be detected by the methods we have employed.

24. Somewhat similar considerations apply to the Q effects. None is significant and only one (16) very suggestive. Such indicators as there are relate to questions 8(*c*) and 16, where the two questionnaires differed, and 10 and 11(*b*), where they were the same. Again it would seem that differences between questionnaires were not of very great magnitude, at least as far as our methods show them up.

25. This does not mean, of course, that the amount of information (or misinformation) produced by an open-ended question is the same as that for a closed question. We have made a few comparisons on this point. The distributions of the number of kinds of programmes mentioned in answer to questions 2(*a*) and 2(*b*) show that the closed form of the question in each case elicited the larger number of responses.

TABLE 8

Percentage Distribution of Number of Mentions

		0	1	2	3	4	5	6	7	8	9	10+
2(*a*)	Closed	1·7	3·8	4·5	9·7	12·6	15·3	13·4	12·3	10·8	7	8·9
	Open	7·6	9·3	28·9	29·3	15·2	7·0	2·3	·3	·1		
2(*b*)	Closed	8·7	38·6	26	15	5·9	2·7	·9	·7	·6	·9	
	Open	23	57·5	16	3	·4	·1					

Distributions of this type were not obtained for any other questions but a comparison of total numbers of replies is possible. For instance, from 292 persons answering the open form of 12(*c*) only 294 reasons were obtained, whereas for the closed form, from 266 persons, 292 (*sic*) reasons were obtained. Similarly, to question 15 in the open form 286 mentions of specific goods were obtained from 920 persons, whereas in answer to the closed form of the question 1,589 mentions of specific goods were obtained from 960 persons.

Thus, wherever comparison of the number of replies received to open- and closed-ended questions can be made, the closed form with the printed list of possible answers on a card produced the larger number of replies. We hope to examine this subject further on another occasion.

26. There remain for consideration the differences between organizations. Here, two questions, 8(*c*) and 10, give significant differences; and a number of others, together with interactions, are suggestive, 8(*b*), 12(*b*), 12(*c*) and 16. Notwithstanding imperfections in the model, it seems to us that these effects are most likely due to inter-organizational differences. A statement like this, of course, requires careful interpretation. It must be remembered that the interviewers were not under the control of their own organizations but were directed by us. The effects *could* be due to the fact that some organisations sent us interviewers who are not typical of the general run of interviewer in those organizations. (They were asked not to, and the attempts made to avoid purposive selection were, to the best of our belief, successful.) It *could* also be due to an unlucky accident in the allocation of interviewees to the organizations; but this is most improbable. It appears to us that, in so far as the interviewers engaged in this experiment were representative of their organizations, the inter-organization differences are real.

27. It would not be proper for us to say which organizations showed the greater variability. The differences are fairly substantial on individual questions, but they tend to be blurred on summarization. Table 9, which shows mean value of V by organization, over three groups of questions, does not suggest any very significant difference between organizations.

TABLE 9

Mean Values of V

	Organizations					
Questions	1	2	3	4	5	6
1–6	2·32	1·42	1·93	1·55	1·42	1·79
7–16	1·93	2·79	1·57	1·42	2·69	1·91
17–21	1·38	1·39	1·90	·88	1·65	1·43
1–21	1·90	1·97	1·76	1·31	2·00	1·74

28. *Acknowledgments*

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SUMMARY

29. The findings of the inquiry may be briefly summarized as follows:

(*a*) We do not confirm the findings of some previous investigations that there is no variability between two interviewers working under similar conditions; some variability was found but it is not very marked.

(*b*) The type of briefing may contribute to this variability, especially on questions which are not absolutely clear; and type of question may also contribute, especially when asked in an open-ended form. But no decisive indication of either effect was found.

(*c*) Signs, but not conclusive signs, were found of variation in variability itself from one organization to another.

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Appendix A and B follow (see p. 132).

Q_1

APPENDIX A

Questionnaire Q_1

SURVEY RESEARCH UNIT TELEVISION SURVEY

Interviewer's No....../....../....../....../

Serial No. of Informant ....

1. I am working for the Survey Research Unit asking people what they think about television. Do you yourself ever watch T.V.; it doesn't matter where?

Yes 1
Never 2

IF "YES" ASK 2(a). IF "NEVER" GO STRAIGHT TO Q.5(b) CROSSING THROUGH Q.2(a) to Q.5(a) INCLUSIVE.

2(a). Which of these kinds of T.V. programmes are you interested in?
(Show card and read)

2(b). Are there any you are particularly interested in?
(Show card and read again, record in Col. 2(b))

	2(*a*)	2(*b*)
Panel Games	Y	Y
Full Length Plays	X	X
Sporting Events	0	0
Ceremonial Events	1	1
Newsreels	2	2
Serial Plays	3	3
Opera or Ballet	4	4
Women's Programmes	5	5
Children's Programmes	6	6
Discussions	7	7
Variety	8	8
Documentaries	9	9
None	10	10
Don't know	11	11

(Record which card used:

....)

3. If you were particularly interested in a programme, at which of these times, Mondays to Fridays, would you most like it to be televised?

(Ask first four only)
Afternoon 1
6-8 p.m. 2
8-10 p.m. 3
After 10 p.m. 4
No preference 5

(Record any comments here: *For office use:*
.... 6 7 8 9
....) Y X 0

4(a). On how many days in the past seven days have you watched T.V. in the afternoon, that is to say any time up to 6 p.m.?
(Ring number stated)
0 1 2 3 4 5 6 7 times
Don't know 8

4(b). And how many times in the evenings, again in the past seven days?
0 1 2 3 4 5 6 7 times
Don't know 8

5. Before I ask this question, I must explain exactly what I mean by "household." Your household is the people living in this house with whom you share your meals.

5(a). Was any of this on your set or haven't you got one in your household?
Own set 1
Haven't got one 2

ASK Q.5(b) OF ALL INFORMANTS.

5(b). Now we want to know whether anyone in your household intends to get a (another) T.V. set soon. Which of these four applies to you?

(Ask first four only)
You definitely will 1
You probably will 2
You probably won't 3
You definitely won't 4

Don't know 5

(Record any comments here: *For office use*
.... 7 8 9
....) Y X

IF "PROBABLY WON'T" OR "DEFINITELY WON'T" (CODES 3 OR 4 TO Q.5(b)) ASK Q.5(c)

5(c). Is that for any of these reasons?
(Show card and read)
Present set will do 1
It costs too much 2
You would rather spend the money on other things 3
You have not enough spare time 4
You don't think the programmes are good enough 5
You just aren't interested in T.V. 6

Not applicable 0

Or is there some other reason?
(Record here)
....
.... *For office use:*
.... 7 8 9
.... Y X
....

ASK Q.6(a) OF ALL WITH T.V., i.e. ANSWERING "OWN SET" TO Q.5(a). FOR OTHERS GO STRAIGHT TO Q.7 CROSSING THROUGH Q.6(a) AND 6(b).

6(a). Can you remember which year it was when you first had T.V. in your household?
(Give year)
....
Don't know X

6(b). If you have had a T.V. set before this one, when did you get your present set?
(Give year) ....

No previous set ....Y
Don't know when X
Don't know whether had a previous set ......A

ASK Q.7 OF ALL INFORMANTS

7. Have you heard of any plans for commercial T.V.?
Yes 1
No 2

APPENDIX A—*contd.*

IF "YES" TO Q.7 ASK Q.8 IF "NO" GO STRAIGHT TO Q.10 CROSSING THROUGH Q.8 to Q.9(c) INCLUSIVE.

8. On the whole, how do you think the quality of the new programmes is likely to compare with the BBC?

8(a). First of all Panel Games.
Do you think they will be:
(*Read*)

better than the BBC	Y
or worse	X
or about the same	0
or don't you know?	1

8(b). And how about Plays?
Do you think they will be
(*Read*)

better than the BBC	2
or worse	3
or about the same	4
or don't you know?	5

8(c). And lastly, Variety?
Do you think it will be:
(*Read*)

better than the BBC	6
or worse	7
or about the same	8
or don't you know?	9

9(a). Do you happen to know the name of the organisation that will provide these programmes?
(*Record as stated*)

Office use

.... ☐

.... ☐

9(b). Do you know when the London station is due to open?
(*Ask first three only*)

By the end of September	1
By the end of December	2
Next year	3
Don't know	4

9(c). Do you think it will cover this borough at first, or not?

Yes	6
No	7
Don't know	8

ASK Q.10 OF ALL WITH T.V., i.e. ANSWERING "OWN SET" TO Q.5. OTHERS GO STRAIGHT TO Q.13(a) CROSSING THROUGH Q.10 TO Q.12(c) INCLUSIVE.

10. Do you think money will have to be spent on your set or aerial in order to get the new commercial programmes?

Yes, on both	1
Yes, on set	2
Yes, on aerial	3
On neither	4
Intends to get a new set	5
Don't know	6

IF "YES, SET" OR "YES, BOTH" ASK Q.11(a) ONWARDS. OTHERWISE GO STRAIGHT TO Q.13(a) CROSSING THROUGH Q.11 TO Q.12(c) INCLUSIVE.

11(a). Will you, or somebody in your household have to spend the money?

Yes	1
No	2
Don't know	3

IF "YES" OR "DON'T KNOW" TO Q.11(a) ASK Q.11(b). IF "NO" GO STRAIGHT TO Q.13(a) CROSSING THROUGH Q.11(b) TO Q.12(c) INCLUSIVE.

11(b). Roughly how much is it going to cost altogether?
(*Ask first three only*)

Under £5	1
£5-£10	2
Over £10	3
Don't know	4

12(a). Are you going to take some steps so that you can receive these new programmes?

Yes	5
No	6
Undecided	7

IF "YES" TO Q.12(a) ASK Q.12(b). IF "NO" OR "UNDECIDED" GO TO Q.12(c).

12(b). Are you going to have these alterations made or are you going to get a new set?

Not applicable	0
New set	1
Adapt old set	2
Undecided	3

IF Q.12(b) HAS BEEN ASKED GO STRAIGHT TO Q.13(a).

12(c). Is that for any of these reasons?
(*Show card and read*)

It costs too much	1
You want to know what the programmes will be like	2
You don't agree with competitive T.V.	3
You think it is too soon to decide	4

Or are there some other reasons?
(*Record as stated*)

For Office use

.... 5 6 7 8

.... 9 X Y

....

Not Applicable	0

ASK Q.13(a) ONWARDS OF ALL INFORMANTS.

13(a). In a T.V. service paid for by advertisers, how much influence do you expect the advertisers to have on the programmes themselves?

Much	1
Little	2
None	3
Don't know	4

13(b). Where do you expect the advertisements to come in the programmes?
(*Read*)

At the beginning	5
At the end	6
Or will they come during the programmes	7
Or don't you know?	8

14. Here are some things which various people have said about advertising on T.V. Will you tell me whether you agree or disagree with them?
(*Show card and read*)

14(a). Good advertisements can be just as entertaining as the programmes themselves.

Agree	1
Disagree	2
Don't know	3

14(b). All T.V should be paid for by advertising, including BBC programmes.

Agree	4
Disagree	5
Don't know	6

14(c). Advertising is all right as long as it doesn't interfere with the programmes.

Agree	7
Disagree	8
Don't know	9

14(d). The only way to get good programmes cheaply is to have advertising on T.V.

Agree	Y
Disagree	X
Don't know	0

APPENDIX A—*contd.*

14(e). Advertising will have a bad effect on the standard of programmes.

Agree 1
Disagree 2
Don't know 3

14(f). Advertising shouldn't be forced on you in the home.

Agree 4
Disagree 5
Don't know 6

14(g). Advertisements are generally bad and misleading and should never be permitted on T.V.

Agree 7
Disagree 8
Don't know 9

15. Do you think any of these products should not be advertised on T.V.?

(Show card and read)

Beer and Spirits 1
Cigarettes 2
Football Pools 3
Patent Medicines 4
Films 5
Magazines 6
Foreign goods 7
Clothing 8

None 9
Don't know 0

(Record which card used)

....

16. Do you think that on the whole T.V. has a good or bad effect on children, or don't you think it has any effect?

Good 1
Bad 2
No effect 3
Don't know 4

17. Now I would like to ask you a few details about yourself and your family. Would you mind telling me who are the people living in your household?

Relationship to Informant	Sex	Exact age if under 21	Ring if 21 or over
1 .... Informant	M F		21
2	M F		21
3	M F		21
4	M F		21
5	M F		21
6	M F		21
7	M F		21
8	M F		21
9	M F		21
10	M F		21
11	M F		21
12	M F		21

No. in Household, including informant, aged:

0 - 4 ....
5 - 15 ....
16 - 20 ....
21 & over ....
TOTAL

Exact age of informant: ....

(Record serial No. in table above of Head of Household: ....)

Office use ☐ ☐ ☐ ☐ ☐

18(a). Are you working?

Yes ....A
No, retired 8
No, temp. stopped 9
No, housewife X

IF "HOUSEWIFE" GO STRAIGHT TO Q.19 CROSSING THROUGH Q.18(b) & Q.18(c).

IF "YES"

What is your job?

IF "RETIRED" OR "TEMP. STOPPED"

What was your last job? *(Obtain fullest possible descriptions of occupation and industry)*

18(b). Occupation *(Give type of work done)*

.... ☐

....

18(c). **Industry** *(Give nature of employer's business)*

.... ☐

....

19(a). Has the head of your household got a job at the present time?

Yes ....A
Self X
No, temp. stopped 9

IF "YES"
What is his job?

IF "TEMP. STOPPED"
What was his last job?

19(b). Occupation *(Give type of work done)*

.... ☐

....

19(c). **Industry** *(Give nature of employer's business)*

.... ☐

....

20. At what age did you finally stop receiving full-time education?

14 or under 1
15 2
16 3
17 4
18 5
19 6
20 or over 7
Don't know 8
Not yet finished 9

FOR THOSE WITH T.V. (ANSWERING "OWN SET" TO Q.5(a)). ASK Q.21 FOR THOSE WITHOUT T.V. CROSS THROUGH Q.21(a) to (d).

21. Would you mind if I had a look at your T.V. set?

Agreed 1
Refused 2

21(a). *(Record make of set here)*

.... ☐

21(b). *(Record size of viewing screen here)*

Height:.............. **Width:**.............. ☐

APPENDIX A—*contd.*

21(c). *(Record whether set has a station selector)*

Yes 1
No 2
Don't know 3

21(d). *(Record whether set is in working order)*

Yes (observed) 1
Yes (asked) 2
No 3
Don't know 4

NAME OF INFORMANT *(Block Capitals)*

....

....

ADDRESS OF INFORMANT *(Block Capitals)*

....

....

Date of Interview.......................

Q_2

APPENDIX B

Questionnaire Q_2

SURVEY RESEARCH UNIT TELEVISION SURVEY

Interviewer's No....../....../....../....../

Serial No. of Informant ....

---

1. I am working for the Survey Research Unit asking people what they think about television. Do you yourself ever watch T.V.; it doesn't matter where?

Yes 1

Never 2

IF "YES" ASK 2(a). IF "NEVER" GO STRAIGHT TO Q.5(b) CROSSING THROUGH Q.2(a) TO Q.5(a) INCLUSIVE.

2(a). What kinds of T.V. programmes are you interested in? *(Record as stated)*

	Office use		
....	01	02	03
	04	05	06
....	07	08	09
	10	11	12
....	Y	X	O

Probe: And are there any others you are interested in?

	Office use		
....	01	02	03
	04	05	06
....	07	08	09
	10	11	12
....	Y	X	O

2(b). Are there any you are particularly interested in? *(Record as stated)*

	Office use
....	☐
....	☐
....	☐

Office use: ☐ ☐ ☐ ☐ ☐

2(c). Are there any ways in which you think the BBC T.V. programmes could be improved? *(Record as stated)*

	Office use		
....	Y	X	O
....	1	2	3
....	4	5	6
....	7	8	9

Probe: And in any other ways?

....	Y	X	O
....	1	2	3
....	4	5	6
....	7	8	9

Office use: ☐ ☐ ☐

3. If you were particularly interested in a programme, at which time, Mondays to Fridays, would you most like it to be televised? *(Record as stated)*

	Office use
....	☐
....	☐

4(a). On how many days in the past seven days have you watched T.V. in the afternoon, that is to say, any time up to 6 p.m.? *(Record as stated)*

....

Don't know 8

4(b). And how many times in the evenings, again in the past seven days? *(Record as stated)*

....

Don't know 8

5. Before I ask this question, I must explain exactly what I mean by "household." Your household is the people living in this house with whom you share you meals.

5(a). Was any of this on your set, or haven't you got one in your household?

Own set 1

Haven't got one 3

ASK Q.5(b). OF ALL INFORMANTS

5(b). Now we want to know whether anyone in your household intends to get a (another) T.V. set soon? *(Record as stated)*

....

.... *Office use*

.... ☐

IF DOES NOT INTEND TO GET T.V. SOON ASK Q.5(c).

5(c). Why not? *(Record as stated)* **Not applicable**...........0

	Office use		
....	Y	X	O
....	1	2	3
....	4	5	6
....	7	8	9

ASK Q.6(a) OF ALL WITH T.V., i.e. ANSWERING "OWN SET TO" Q.5(a). FOR OTHERS GO STRAIGHT ON TO Q.7 CROSSING THROUGH Q.6(a) AND Q.6(b).

6(a). Can you remember which year it was you first had T.V. in your household? *(Give year)*

....

Don't know X

6(b). If you have had a T.V. set before this one, when did you get your present T.V. set? *(Give year)*

....

No previous set ....Y

Don't know when X

Don't know if had a previous set ....A

APPENDIX B—*contd.*

ASK Q.7 OF ALL INFORMANTS

7. Have you heard of any plans for commercial T.V.?

Yes 1
No 2

IF "YES" TO Q.7 ASK Q.8. IF "NO" GO STRAIGHT TO Q.10 CROSSING THROUGH Q.8 TO Q.9 INCLUSIVE

8. On the whole, how do you think the quality of the new programmes is likely to compare with the BBC?

8(a). First of all how do you think the quality of Panel Games will compare with the BBC?

Office use
....
.... ☐

8(b). And how about the quality of Plays compared to the BBC?

Office use
....
.... ☐

8(c). And, lastly the quality of Variety compared to the BBC?

.... *Office use*
.... ☐

9(a). Do you happen to know the name of the organisation that will provide these programmes?
(*Record as stated*) *Office use*

.... ☐ ☐
....

9(b). Do you know when the London station is due to open?
(*Record as stated*)

Office use
....
.... ☐ ☐

9(c). Do you think it will cover this borough at first, or not?

Yes 6
No 7
Don't know 8

ASK Q.10 OF ALL WITH T.V., i.e. ANSWERING "OWN SET" TO Q.5. OTHERS GO STRAIGHT TO Q.13(a) CROSSING THROUGH Q.10 to Q.12(c) INCLUSIVE.

10. Do you think money will have to be spent on your set or aerial in order to get the new programmes?

Yes, on both 1
Yes, on set 2
Yes, on aerial 3
On neither 4
Intends to get new set 5
Don't know 6

IF "YES" SET OR "YES BOTH" ASK Q.11(a) ONWARDS. OTHERWISE GO STRAIGHT TO Q.13(a), CROSSING THROUGH Q.11 to Q.12(c) INCLUSIVE.

11(a). Will you, or somebody in your household, have to spend the money?

Yes 1
No 2
Don't know 3

IF "YES" OR "DON'T KNOW" TO Q.11(a) ASK Q.11(b). IF "NO" GO STRAIGHT TO Q.13(a) CROSSING THROUGH Q.11(b) TO Q.12(c) INCLUSIVE.

11(b). Roughly, how much is it going to cost altogether?
(*Record as stated*)

Office use
....
.... ☐

12(a). Are you going to take some steps so that you can receive these new programmes?

Yes 5
No 6
Undecided 7

IF "YES" TO Q.12(a) ASK Q.12(b). IF "NO" OR "UNDECIDED" GO TO Q.12(c).

12(b). Are you going to have these alterations made or are you going to get a new set?

Not applicable 0
New set 1
Adapt old set 2
Undecided 3

IF Q.12(b) HAS BEEN ASKED GO STRAIGHT TO Q.13(a).

12(c). Why not?
(*Record as stated*) **Not applicable** 0

....
.... *Office use*
.... ☐
....

ASK Q.13(a) ONWARDS OF ALL INFORMANTS.

13(a). In a T.V. service paid for by advertisers, how much influence do you expect the advertisers to have on the programmes themselves?
(*Record as stated*)

.... *Office use*
.... ☐
.... ☐

13(b). Where do you expect the advertisements to come in the programmes?
(*Record as stated*)

.... *Office use*
.... ☐

14. Now will you tell me how you feel about advertising on T.V. generally?
(*Probe freely*) *Office use*

.... ☐
.... ☐
.... ☐
.... ☐

15. What products, if any, do you think should not be advertised on T.V.?
(*Record as stated*) *Office use*

.... 1 2 3
.... 4 5 6
.... 7 8 9
.... O Y X

(*Probe: Any others?*)

.... ☐
.... ☐

APPENDIX B—*contd.*

16. On the whole what sort of effect do you think T.V. has upon children?

.... *Office use*

.... ☐

.... ☐

17. Now I would like to ask you a few details about yourself and your family. Would you mind telling me who are the people living in your household?

Relationship to Informant	Sex	Exact age if under 21	Ring if 21 or over
1 .... Informant	M F		21
2	M F		21
3	M F		21
4	M F		21
5	M F		21
6	M F		21
7	M F		21
8	M F		21
9	M F		21
10	M F		21
11	M F		21
12	M F		21

No. in Household, including informant, aged:

0 - 4 ....
5 - 15 ....
16 - 20 ....
21 & over ....

TOTAL

Exact age of informant: ....

(*Record serial No. in table above of Head of Household:*

....)

Office use: ☐ ☐ ☐ ☐ ☐

18(a). Are you working?
Yes ....A
No, retired 8
No, temp. stopped 9
No, housewife X

IF "HOUSEWIFE" GO STRAIGHT TO Q.19 CROSSING THROUGH Q.18(b) & Q.18(c).

IF "YES"
What is your job?

IF "RETIRED" OR "TEMP. STOPPED"
What was your last job? (*Obtain fullest possible descriptions of occupation and industry*)

18(b). Occupation (*Give type of work done*)

.... ☐

....

18(c). **Industry** (*Give nature of employer's business*)

.... ☐

....

19(a). Has the head of your household got a job at the present time?
Yes ....A
Self X
No, temp. stopped 9

IF "YES"
What is his job?

IF "TEMP. STOPPED"
What was his last job?

19(b). Occupation (*Give type of work done*)

.... ☐

....

19(c). **Industry** (*Give nature of employer's business*)

.... ☐

....

20. At what age did you finally stop receiving full-time education?
14 or under 1
15 2
16 3
17 4
18 5
19 6
20 or over 7
Don't know 8
Not yet finished 9

FOR THOSE WITH T.V. (ANSWERING "OWN SET" TO Q.5(a)). ASK Q.21 FOR THOSE WITHOUT T.V. CROSS THROUGH Q.21(a) to (d).

21(a). What make is your set?

.... ☐

....

21(b). What is the size of the screen?

.... ☐

....

21(c). Has the set got a station selector?
Yes 1
No 2
Don't know 3

21(d). Is the set in working order?
Yes 2
No 3
Don't know 4

NAME OF INFORMANT (*Block Capitals*)

....

....

ADDRESS OF INFORMANT (*Block Capitals*)

....

....

Date of Interview........................

DISCUSSION ON PAPER BY MISS GALES AND PROFESSOR KENDALL

MR. H. F. LYDALL: Professor Kendall and his Research Techniques Unit have produced a series of impressive experiments in the field of social surveys. The present paper, which is a worthy continuation of the series, enters a new and very interesting part of the field.

One of the principal aims of the organizers of social surveys is to eliminate the "human element" from their work; to reduce the process so far as possible to a standardized pattern, conforming to rules laid down in advance. This "human element", however, keeps creeping back at all points, whether at first contact between interviewer and respondent, when the crucial decision is taken to co-operate or not to co-operate in the survey, or in the interview itself when the questions are put and the answers given, or in the recording of the results and the coding and tabulating of the data. With so many links in the chain, it is particularly desirable that someone should go along from time to time testing each link and pointing out possible sources of weakness. And if this can be done by an impartial, public and academic body, such as the Research Techniques Unit, so much the better.

But the testing of research techniques in the field of social surveys is not an easy matter. I am not thinking so much of the well-known difficulties of carrying out any kind of social experiments with human beings, but rather of the difficulty of appreciating fully the purposes in the minds of the initiators of social surveys themselves. In order to perform useful tests of social survey techniques it is particularly important to choose for study those problems which are most acutely felt by social survey workers, and to see the problems in the context of the survey worker's interests.

The social survey research worker is faced with the following practical problems: (1) How great is the tendency for interviewers to vary in performance? (2) Can this variability be reduced by such measures as—(*a*) more appropriate methods of selection and training of field staff, (*b*) specific briefing and test interviews in each survey, (*c*) simplifying the questionnaire by precoding, clearer layout, etc., (*d*) greater efforts to check up on interviewers by repeat interviews or other methods? Now in this experiment only methods (*b*) and (*c*) can in fact be tested, since any variation in (*a*) is a function of the survey organizations, whose differences in recruiting and training methods cannot be discussed without revealing their identities; while the possible influence of (*d*) on interviewer variability was not considered.

In effect, therefore, this experiment resolves itself into a test of whether interviewer variability can be reduced by (1) better briefing or (2) precoding the questionnaire. Of these two issues I should certainly regard the first as of greater practical importance.

The cost of bringing interviewers together for briefing meetings can be an expensive item. If anyone can prove that briefing meetings have no effect in reducing interviewer variability some organizations would be able to make substantial savings in their fieldwork costs.

The findings of this paper on the influence of briefing are negative. The authors remark on this point that "it would not have surprised us if the *B*-effects had been more marked". I agree with them, as a general proposition; but I am not sure that I agree with them in this particular case.

This brings me to my major point of criticism of the paper. I think that the test was conducted at much too elementary a level. The questionnaire is, for the most part, a very simple one which, it seems to me, only touches the surface of the problem with which it deals. Indeed, I find it somewhat difficult to imagine what went on at the briefing meetings. Before we can make a judgment about the value of briefing, the whole issue needs to be studied very much more thoroughly. Tests should be made of questionnaires of greater operational importance, requiring a higher standard of interviewing than, I believe, was needed in this example. I have in mind surveys designed to collect basic facts about consumer habits, on expenditure or on attitudes towards institutions, policies or products. Such surveys put the interviewers on their mettle. If they can achieve consistent results in these surveys without verbal briefing then we shall really be able to congratulate ourselves on the high standard of our interviewers.

Considering the results of this test in detail, I was struck by the wide variation in the values of V for some of the questions, as they can be derived from Table 1. If we consider the questions which produced a value of more than 2 we find that they fall into three groups. First, there are questions 1, 5(*a*) and 21(*d*) which are all concerned with whether the respondent is a member of the T.V.-class or not. Question 1 asks: "Do you yourself ever watch T.V.?" Question 5(*a*) asks whether the respondent actually owns a set. Question 21(*d*)—the last question of all—asks "Is the set in working order?" I would suggest that all these questions are what might be called "status" questions.

The next group of questions yielding a high value of V consists of three which, it seems to me, have significant political or moral overtones. These are questions 13(*a*), 15 and 16. The first asks about the influence of advertisers on the programmes, the second about products that should not be advertised and the third about the influence of T.V. on children.

The third group, which consists of questions 3, 7, 9(*c*) and 13(*b*), has nothing very obviously in common in subject matter; but I think I discern another characteristic which is shared by at least two of them, namely a certain ambiguity. Question 7 asks: "Have you heard of any plans for commercial T.V.?" Frankly, I am not clear what this means. It could mean either: "Do you know *whether there is going to be* commercial T.V.?" or: "Do you know *what plans have been made* for commercial T.V.?" Perhaps it was deliberately left vague since it is merely a "lead-in" question. Anyway it seems highly probable that it was this element of ambiguity in the question which produced an over-all value of V of 8·65.

I am also puzzled by question 13(*b*) which asks: "Where do you expect the advertisements to come in the programme?" The prompts in the precoded questionnaire read: "At the beginning, at the end, or will they come during the programme, or don't you know?" These answers are not, to my mind, mutually exclusive or exhaustive. Supposing the advertisements are expected to come at the beginning, at the end *and* during the programmes, what then? And how does one distinguish between the beginning of one programme and the end of the preceding one?

I suspect that there are elements of ambiguity about questions 3 and 9(*c*) also, but I would not press the point.

All told, it seems that these 10 high V-value questions include at least 8 which are peculiar in that either they are likely to arouse some emotonal response from the respondent or they are ambiguous. It would not surprise me to learn that it was precisely on the answers to such questions that the influence of the individual interviewer is most apparent.

I hope the authors will forgive me for offering certain criticisms of their methods. I can assure them that this is really a tribute to the stimulating and suggestive character of their paper. I have much pleasure in proposing the vote of thanks.

Mr. Gray (seconding the vote of thanks): I should like to congratulate the Division of Research Techniques on leaving the relatively well-charted waters of sampling for the deeper waters beyond.

I will comment first on non-response. The figures are given on p. 124. I do not think that the possible bad effects of non-response have been properly dealt with. We should have been told why the originally chosen people were not interviewed. There is a variation of between 17 and 3 in the number of substitutes used. I should like to see a chi-square for the pairs of interviewers, for the number of people not interviewed because they were out—that is, not at home when the interviewer called. Being out is quite clearly related to not having a television set.

Television set ownership shows a significant interviewer variability. This is very suspicious, because it is a factual item for which we in the Social Survey investigated the interviewer variability and did not find any.* The explanation may lie in the failure to contact the originally chosen people or there may be another reason to which I will refer later. Nevertheless it would have been a good idea to have observed for the originally chosen people whether a television aerial could be seen at the address.

I shall follow the last speaker now in seeking to suggest how interviewer variability arises; for there is a lot of it. In Table 1, the authors give an over-all measure of interviewer variability for each of 39 items. These questions are not independent, but there seems to be highly significant interviewer variability on 20 out of 39, which is over half. The analysis of variance that follows is not very fruitful.

It seeks to find out whether the amount of variability is affected, firstly, by precoding the answers; secondly, by giving verbal briefing on top of written instructions; and, thirdly, by the different type of interviewer provided by the different organizations. Only two significant differences emerge. If there had been more I should have wanted to know three things: first, whether the same classification was used for coding open questions as was used for precodes; secondly, exactly what additional decisions and definitions were made at the verbal briefing—what was added to the written instructions; and, thirdly, the background of training and experience of the interviewers in the different organizations. Perhaps this is not so necessary with only two significant differences. These are between organizations, and I will return to them in a moment.

We are left with a large amount of unexplained interviewer variability for 20 out of 39 items. Turning to the precoded questionnaire on p. 136: Question 1, "Do you ever watch?" showed a significant amount of variability. The question "Do you ever?" implies some degree of regularity but does not define the degree. It is not "Have you ever?" but "Do you ever?" and different interpretations will be put upon "ever" by different interviewers and informants. Verbal briefing will not have any effect unless one defines "ever".

---

* Gray, P. G. (1956), "Examples of Interviewer Variability taken from Two Sample Surveys". *Applied Statistics*, **5**, 73–85.

Question 2(*a*) is not significant but 2(*b*) is, and the trouble lies in "particularly interested". Does this mean "sufficiently interested to stay at home and not go out because of the programme"? It could mean almost anything and it would depend very much on how it was spoken. You may be sure the interviewer will sometimes be asked for a definition.

On question 3, as on a number of questions, I should like to know whether the difference lies between "no preference" and the rest. Do some interviewers persuade some people to choose something rather than nothing? This leads to a general criticism, that no attempt has been made to telescope classes together to see how this affects the interviewer variability.

Question 4(*a*) is interesting because it is quite factual and definable. It is not significant, and I do not think it should be, but 4(*b*) is. The information sought is perfectly factual but I think the form of the question is very bad. "Evening" is defined by implication from 4(*a*). But between 4(*a*) and 4(*b*) the informant may well have digressed and said something about an interesting afternoon programme, and may even have poured out a cup of tea. Each question should be self-contained as far as possible. Even more important, 4(*b*) could be misunderstood for "How many times in an evening is a programme viewed?" People do not listen very carefully to questions and they cannot see the questionnaire and what follows what. Therefore, the full question should be given with careful attention to defining what is meant by the "last seven days".

5(*a*) is significant. This is the interesting question about television ownership. There are three possible answers to the question as printed: first, "Yes, on the household set", secondly, "On someone else's set", and, thirdly, "Have not viewed in the last seven days". Perhaps the written instructions offered some guidance, but during an interview the question will be put often as it stands regardless of supplementary instructions. It seems to me that the question is a roundabout approach to asking, "Has your household got a television set?" If that is what you want to know, why not ask it? The set is probably visible or is in the next room, so you are likely to be told the truth. When we have asked this direct question, we have had no interviewer variability whatever, and I do not expect it on such a factual item.

Question 5(*b*) is significant. The four classes defy definition. The very tone of voice will affect the answer and when the question is left open the answers given are often qualified—"Yes, if . . ." "No, unless . . ." The informants do not oblige by answering the question in the way you want it answered.

I agree with the preceding speaker about the ambiguity of some of the questions. Almost all the opinion questions show variability and the factual definable items in the main do not. An important exception is question 20. "At what age did you finally stop receiving full-time education?" It should be possible to get the information without interviewer variability. I am a little puzzled at what has gone wrong here and should like the authors to supply the detailed figures for this question. I wonder whether the difference lies between codes 1 and 2. Were there such answers as, "I went to an elementary school" which got translated into "15" by some interviewers not familiar with pre-1944 conditions?

In summing up the results, I feel one should pretend to be conducting the inquiry for some business firm interested in the future for I.T.V. It is important always to have some reason for doing a survey. Which of these results would then worry me? First and most important, question 5(*a*). I think we ought to be able to get that right; also, 4(*b*), and possibly 20.

As for many of the other items showing variability, I do not think I should ever have regarded them as anything but very imprecise measurements of the public's attitude.

There is little reason, having regard to the probable source of interviewer variability, to expect that precoding the answers, or giving an additional verbal briefing, would reduce the variability in this survey. Many surveys, certainly of the sort with which I am concerned, are of a much more technical nature and the briefing has to instruct the interviewers in the subject of the inquiry. In this case little or no such instruction was necessary for it deals with matters of common knowledge.

Returning to the two significant differences between organizations, the most interesting is that for question 10, for which the chi-square is only 104 and the d.f. are 101 (see Table 1). So we have the rather surprising result that, although the over-all interviewer variability is not significant, there is a significant difference between organizations in the amount of variability. The only clues to this puzzle are provided by the detailed V values in Table 3. Organization 4 has done almost too well. The average V for the four pairs of interviewers, on questions 17 to 21, is 0·88. That is against an expected value of 1·0, allowing for just sampling variability and no interviewer variability whatever. The last pair of interviewers of organization 4 has done exceedingly well. The average for the whole 41 items is only 0·93. Perhaps this is just luck.

One might expect some difference between organizations. For example, the interviewers will have had different amounts of experience with random sampling. Different emphasis is laid on selection and training by the different organizations. Different types of questionnaire will have been used. For example, the interviewers from one organization were not, I think, accustomed to interviewing of this type. They were used to asking questions without filling in a questionnaire at the time of the interview. The questionnaire would be completed afterwards. And there are other differences.

A very rewarding method of studying interviewer variability is to play over to a group of interviewers a recorded interview and get them to take down the answers. You will then discover what the interviewers do with the answers they are given. Another method is to sit in on, or record, an appreciable number of interviews during the pilot stage of one's inquiry. In this way one can hope to reduce to insignificance variability on questions of a factual nature. On opinion questions there will always be a variability.

In conclusion, I should like to thank the authors for presenting such a valuable set of data. It is all too difficult for organizations which are doing surveys to incorporate experimental designs into their work. Often they have not the time or the money to do so.

The vote of thanks was carried unanimously.

Mr. M. J. R. HEALY: I find the distinction between accuracy and variability a little hard to follow. In the present work, it seems to me that all three sources of differences between interviewers are contributing.

The authors have expressed some disappointment at the lack of clear-cut results from their investigation. I think that one reason for this is that the experiment is technically a very small one, containing only 24 observations and giving rise to only 5 d.f. for error, one of which was lost owing to the defaulting interviewer. From this point of view, the design of the experiment may seem rather too ambitious for the resources available.

I would like to support the proposal that specific details of the basic tables should receive further analysis. Inspection of Table 2 shows, for example, that considerable differences occurred in the proportions of "miscellaneous" or "don't know" answers. However, I would expect such condensation of the tables to emphasize rather than to reduce the variability.

Mr. MASON: The first thing a business man asks on seeing results is, "How can I apply this material?" Although it is encouraging, in a sense, that no positive evidence has materialized from this research, it is hard to see how it can help him to improve his interviewing.

Finance dictates 40 as the "unit" in the analysis. I think one of the difficulties in sorting out this material lies in that sample size; parallel samples of 40 can never be identical in practice, and thus sampling variance adds to the interviewer-respondent variance, connected with latent flexibility in the questioning itself.

On question 10, organization 4 seems to have the minimum variance. It also shows up "best" on questions 17 to 21 in the inter-organizational variance table. There seems a direct connection here. To some extent, with these small samples, the variability, for want of a representative sampling basis for the work, makes interpretation difficult (e.g. the variance on the sex factor cannot be due to the interviewer but to the luck of the draw of the samples itself. These should surely have been cut out before "doing chi-squares"). It is still not shown that the variability is not in the main the same as what one would normally expect the sampling to produce with perfect interviewers.

It would be most valuable to have the actual results, question by question, or, at least, the variance, shown in the results between the organizations. Research men want to see whether the variability or departure from true measures is of such a size that it is worth developing more practical controls than are at present imposed.

Unfortunately, the interviewers, because of lack of finance, are not given jobs in more than one district. It may well be that the man (or woman) who can cover Stepney adequately is not the man (or woman) for Wimbledon. Research must get the people with the personality able to get into a docker's house and talk to his children and also to attend on a class-A business executive in the same survey. By widening the sample for each interviewer and spreading over various districts and, if necessary, even reducing the number of interviewers, it might have got nearer to the practical business problem of interviewer selection on which any progress in this field is more likely to be made.

It is assumed, rather superficially, that the smaller the variability the better the result, but I question that. I feel a paradox exists in that, within certain limits, the greater the variability the better the broad survey results which may be achieved. That is why I would ask for a rank correlation with the actual survey results.

On the time factor, differences of quality are bound to occur according to the period over which the survey is extended; between June and August last year a great deal happened in the general public's awareness of commercial television. Would it be possible to confirm that all stages of the experiment were conducted in a balanced way throughout the period? The Q_1, Q_2 comparison may not be of much significance unless the possibility of a growing awareness, or a change in population being irregularly measured can be eliminated. I am not interested in "four decimal places", as I feel only the threshold of the solution has been reached. Research organizations must now consider very carefully what finance is needed, and where and how to use what is procurable to get something worth while for practical work, but I am sure all the organizations will continue to co-operate as best they can.

Dr. Nuttall: We owe such a debt of gratitude to the Division of Research Techniques that it is a little sad that everyone should appear to be frustrated by the results of this particular survey, which marks a turning point in the Division's work. It is not easy to see how this sense of frustration has arisen. The work has been so well carried out, despite all the questions that have been raised. It is not new to come across such difficulties, which I would suggest may be due to too much reliance being placed on the techniques of design of experiments, statistical analysis and questionnaire design alone.

The analogy of taste testing is very apt. Ten years ago it was common to assume that small differences in taste could be identified by untrained respondents and that products incorporating these small differences could be ranked in order of preference. There were many non-significant results in these experiments which were usually attributed to the simple design then in use. But even elaborate designs carefully carried out proved equally negative.

However, no one would now accept that taste differences, if detectable, can necessarily be communicated in meaningful terms to the interviewer. Even if they could be, no one really believes they can be generalized from the individual to the class. This change has resulted from adding to statistical method and questionnaire design techniques contributed by the food chemist, the physiologist, and the psychologist; with this team work we have achieved a position of some strength. Much the same kind of approach could be adopted for tackling the problem now discussed.

There will be many suggestions for the further analysis of the results. I will suggest just one. Variability among interviewers probably develops as the series of interviews progresses, and I wonder whether it would be possible to analyse the results sequentially. The random selection of interviewers must have produced some new and some very experienced ones. The new ones may through over-anxiety select answers because they are flustered; the very experienced ones probably have an occupational disease of selection anyway, which may come out if the results are analysed in the way I suggest.

Mr. N. J. Squirrell: There have been one or two references to the limited value of this inquiry. As a representative of one of the participating organizations, I should like to make clear that we feel we have obtained some value from it.

Of the two kinds of interview covered, we normally undertake only the type which makes use of open-ended questions and we have useful information from this study on the variability of our interviewers in undertaking this type of work.

The variability between organizations and between interviews, as measured here, is not too relevant to normal practice since some organizations, like our own, were, for the purposes of the inquiry, carrying out work of a type they do not normally handle.

There is a tendency for these results to be generalized out of their context. A paper read before the Society in 1952 by Mr. David and Mr. Booker dealt with differences in results obtained by experienced and inexperienced interviewers. The conclusion broadly was that the differences were small, and some people might have been excused for getting the impression that experience had not much to offer. The inquiry was of such a nature, however, as to offer little chance for different results between the two classes of interviewers. In the same way, in the present study any conclusions about the small contribution made by briefing as opposed to no verbal briefing cannot be generalized beyond the actual context.

Mr. J. L. Nicholson: As this is one of a series of experiments in social survey methods, and as we all hope that further experiments will be undertaken by the very able team of statisticians working at the Research Techniques Unit, it seems worth glancing at the achievements to date of the series as a whole.

In an experiment on experienced versus inexperienced interviewers, the former achieved the higher response rates, but the two classes of interviewer obtained broadly similar answers to rather simple questionnaires. An interesting experiment on call-backs and clustering showed that it is well worth making at least three calls before abandoning the chase—which is the general practice in this country. The results on clustering may fairly be described as negative. A detailed study of quota sampling showed that skilful application may mitigate, without overcoming, some of its theoretical disadvantages. Another inquiry demonstrated the importance of care and accuracy in coding. The results of the present enquiry are inconclusive and cannot be regarded as superseding the previous evidence.

For the most part, these experiments have merely confirmed, or added precision to, the conclusions which were already, I believe, widely accepted by those with practical experience of social surveys. Those who have to design surveys of which the results are required for operational purposes are aware of many other problems which they would like to see investigated; research into some of these problems might have been more fruitful.

There is apparently no intention of tabulating the answers to the two questionnaires used in the present experiment and this may partly account for some of the weaknesses remarked on by other speakers. To prevent the possibilities of a divorce between theory and practice, I would suggest that, apart from the factors chosen for special investigation, any further experiments should be run on the same lines as, or even in conjunction with, an actual survey; and that the choice of subjects for investigation should be governed by the needs which working practitioners feel to be most important.

Mr. W. Smith: There is an acknowledgment to Miss Harris of the Social Survey for her work. I am wondering what influence the coding had on the answers which have been analysed on interviewer variability. Has any work been done on that? Did Miss Harris herself do the whole of the coding, or was she responsible for a team of coders and, if so, what controls were there on the coders?

Miss Harris: We are aware that coding can make an enormous difference to the results of surveys. In this case, knowing that interviewer difference was of such importance we took a great deal of trouble to ensure that we did not add to it.

We know from experience the ways in which coding can introduce errors. For example, a coder given the coding frame and the same material can code in one way to get one result in January, and, given precisely the same coding frame and precisely the same instructions and the same answers, can six months later get a quite different result. We know that we can add in error if there is some ambiguity in the actual coding frames or in the instructions. In this case we tested quite a number of questions which we would not normally have done. We tested them wherever there might possibly be some ambiguity in the coding frame.

In testing coding frames we take the coders who are going to work on the job and give them a coding frame and the same number of answers, with the same instructions, and they each separately code the same answers. We then compare the codes they have given to the answers, with the actual schedule involved, and eliminate from the test any of the answers on which all the coders are in complete agreement. We then have some answers of which some are coded in one way and some in another. We can go through these and discuss why one particular coder thought they should go in one category and another thought they should go in another. We can then amend the coding instructions and say, "If so-and-so is said it must go into this particular category,' and be reasonably sure of comparability between coders.

The purpose of this experiment is to see whether there is any difference between *interviewers*. So when the work was actually being done only experienced coders were used and we ensured that a particular coder coded one question right through the entire survey. The interviewers were numbered and we insisted that a pair of interviewers be coded at the same time without any break.

What we could have done, but did not, was take one from one pile and one from another, but we ensured that there was not a break of anything longer than ten minutes between coding the end of one of a pair of interviewers and beginning on the next.

One speaker mentioned the question of coding qualified answers. In fact, when we did the open questionnaire, although we tried as far as possible to follow the closed questionnaire, we did find we needed these qualified categories, otherwise coders might perhaps have had to decide subjectively in which category something would go. If the coding has had any effect on this survey it is slightly to iron out any differences by ensuring that our categories were precise.

Mr. Wilkins: The whole of the emphasis has been on the question of minimizing the variance between interviewers, but I wonder whether, if we get too great an agreement, we might not suspect that agreement to have been arranged. What checks were made and what precautions were taken to ensure against this? Might it not be desirable to look at the other side of the chi-squared test? Was the agreement sometimes too good to be likely to have arisen in random sampling?

The following written contribution was received after the meeting:

Dr. H. W. Durant: The Gallup Poll is a member of the Committee that was concerned with the organization of this project. However, the Gallup Poll did not participate in the experiment for reasons that were set out in writing to the authors of this paper at the time that we withdrew. The main grounds were as follows:

(*a*) We had a large number of criticisms to make on the questionnaire, even as finally agreed by the Committee. In spite of written recommendations from us, many of which were accepted and resulted in radical changes, there remained between 20 and 30 points where we felt that the wording was not sufficiently precise, or that the framing of the questions, or the design of the questioning, could have been much improved. It seemed to us axiomatic that only with a completely satisfactory questionnaire would one be entitled to say that the interviewers themselves introduced variations. Mr. Gray has picked up this point already and has observed that it is precisely on those questions where the wording is most unsatisfactory that the variation is greatest. This is what we had anticipated at the beginning, and since we could not secure the amendments to the questionnaire which we regarded as essential, we believed that the experiment was doomed to secure results which were open to disputation.

(*b*) The design of the experiment was impractical in the sense that the number of interviews envisaged for each interviewer did not conform with any organization's standard practice. It was both too small for some cases and too large for others. This point has already been developed in discussion by Mr. Mason and there is no need to say any more. But it meant that the results could have little relevance to day-to-day working conditions.

(*c*) We were opposed and are opposed to substitutes being taken in any random sample survey. It can lead only to slackness on the part of the interviewer. In the present instance it was inevitable that it would also result in the interviewer's samples being non-comparable. Again, this is a point developed by Mr. Gray and there is no need to talk further on it.

(*d*) We had anticipated that, in conformity with our own standard practice, briefing plus written instructions would produce no variations in interviewers' performances compared with only written instructions being given. This anticipation of ours has been confirmed, and we would add only the point that where the briefing is badly done it will certainly produce variations. We need not recount the experiments which have been conducted by psychologists showing the garbled recollections that result from people having lectures or verbal instructions that are not confirmed in writing. It is the written instructions that count.

Our views on the relative unimportance of briefing remain, after this paper, as they were before. Briefing should not, indeed cannot, cover up the deficiencies of a questionnaire. The ideal is for the questionnaire to be so clear and concise that even written instructions are concerned only with telling the interviewer where and when to interview.

As we said in the 1954 survey (*I.P.A. Readership*), the biggest random sample survey that has ever been done anywhere, briefing can contribute to raising interviewer morale. It can help the interviewer to feel that he is a member of a team; to make it seem worthwhile to make that extra re-call and that long trek to the other end of the town. We did not use briefing in the 1954 survey to bolster an imprecise questionnaire. Before we contemplated the briefing we made sure that the questionnaire and the written instructions were unambiguous and self-explanatory. We did a large pilot without briefing.

In such a case cost is the over-riding consideration. Briefing costs money and the researcher has to decide whether he is justified in recommending the spending of money for this purpose.

(*e*) We objected to the choice of television as the subject of the inquiry. Events have shown, as we said they would show, that the subject was not of sufficient difficulty to give sufficient scope for variations to appear in large enough quantities.

It is relevant to remark here that in practice we find the fact that there is going to be a briefing is sometimes the unspoken defence of a sloppily constructed questionnaire. "Why bother, it can all be explained to the interviewer" is the implicit attitude.

May we offer a few brief comments on the reported findings. "We do not confirm the findings of some previous investigations that there is no variability between two interviewers working under similar conditions; some variability was found but it was not very marked." This experiment, unfortunately, compounds variability due to substitution on the one hand, with variability due to the handling of the questionnaire/interview situation. We shall never know how much was due to the former, how much to the latter.

The time of day and the day of the week on which calls were made was not controlled. These are factors fundamental to determining success rates and the answers obtained to questions, e.g. if the husband is at home he may be called upon to help the wife in answering factual questions; he may impress his opinions. We do not even know how many interviews were made with or without others present. This is a very important factor in all types of surveys and is one reason why random samples are dangerous when employed for political questionnaires, or for any inquiry involving question of prestige, e.g. drinking.

Practitioners know that the sex and class of the interviewer influence results considerably. It would be interesting to get all organizations to rate their interviewers according to their social status. One of the organizations involved employs only women university graduates as interviewers. How did they fare, in Stepney for instance?

On this question of organizational differences it may be a compound of a number of things and until they are sorted out we do not know the answer: (*a*) Time and day of working (*b*) sex and class of interviewer; (*c*) degree of familiarity with formal questionnaires; (*d*) willingness to accept substitutes.

Professor KENDALL: I shall not attempt to reply verbally to all the points in any detail. I should like to thank Mr. Lydall, Mr. Gray and the others who have contributed to a constructive discussion.

There are two points I should like to mention now. A point was made by Mr. Lydall and other speakers that the questionnaire was rather too simple to throw up fully the effects we were looking for. Although it is by no means a difficult questionnaire, it is not unusually operational. Questionnaires like this are being discussed all the time. We did consider making it much tougher, and I fully agree about the desirability of doing work in that connection, but I think before one can do that one really requires the earlier stage of validation. If one is going to ask questions of a peculiar and intricate kind, one must have a much better idea than we have at present whether the right sort of answers are being obtained. This is a sequential process. We have to do a great deal of work on the validation of the answers before we can come back and ask questions of a more deep and intricate kind. The point was not, however, overlooked.

The second question was the one raised by Mr. Gray, who seemed to be firing some very heavy ammunition, but not altogether at us. He was discussing the difficulty of designing questionnaires and framing questions in the right way. This is a well-known difficulty again on which a great deal of work remains to be done. This particular questionnaire was drafted, discussed and redrafted over and over again by a committee including the representatives of the six organizations. Some of the questions were deliberately left in a rather ambiguous way, but in discussing this point I am perfectly prepared to claim that, however long the preparation takes, it is quite possible for someone to criticize the form of the questionnaire and the framing or phrasing of some of the questions. Nearly always there is some phrase or term which someone may misunderstand. It is possible to do a great deal in the way of designing questionnaires, but I do not think that everything Mr. Gray said is directed mainly to the purpose of this inquiry.

Professor KENDALL and Miss GALES later replied in writing as follows: So many points have been raised that perhaps our colleagues will forgive us for replying to them somewhat telegraphically. We thank the participants once again for a very helpful discussion.

Mr. Lydall makes the point that the questionnaires were too simple. On the whole we are inclined to agree, but we should like to point out, first that we did not know what to expect, second that the questionnaires were not easier than a good many in operational use, and third that we were trying to build up a technique. If, knowing what we know now, we were to conduct a further experiment, we should certainly try to meet Mr. Lydall's point by using questionnaires of a more complex nature, the only restrictions being our wish to compare answers to the open and closed form of questions and to obtain results of some generality.

Question 7, "Have you heard of any plans for commercial T.V.?" gave rise to a lot of discussion in the drafting. As Mr. Lydall surmises, we ultimately decided to leave it vague on purpose. It is just this sort of ambiguity which seems to give rise to variability between interviewers.

As to Question 13(*b*), the answer to Mr. Lydall is that we used multiple coding here and elsewhere if more than one answer could be given.

We regret that the information asked for by Mr. Gray about reasons for non-contact is not available. We agree with him that normally substitutes should not be allowed; and every possible effort was made to cut down their number. The variation in television set ownership may in fact have resulted from a difference in substitution rates between members of pairs of interviewers. Since the situation has however been changing so rapidly over the last few years, the different findings by different inquirers are not necessarily inconsistent.

Mr. Gray asks for some information about the briefing meetings, but he does not expect us to provide a record of them. He knows as well as anyone what they are like. It is an unfortunate feature of the scientific presentation of this sort of inquiry that consumers have to take something on trust. As we were assisted by Miss Harris, perhaps Mr. Gray will give us the benefit of the doubt.

As to the major matter of the form of question, we can only say that these questionnaires were the subject of several meetings among experts, the final form being the best that they could agree upon. We suggest not that the wording is perfect, but that no wording could have been found which could have completely escaped criticism. Mr. Gray's remarks on this topic, though sound enough, are not really directed at the subject of the inquiry.

In question 5(*b*), different codings were used for the two questionnaires. But again this should not impair a comparison of interviewers working under similar conditions. The variability in question seems to arise, as Mr. Gray suggests, through a confusion between codes 1 and 2. When these two codes are run together the variability becomes no larger than one would expect on such a factual question.

Mr. Healy queries the distinction between accuracy and variability. He is correct in saying that the replies by the interviewers incorporate inaccuracy (if it exists) as well as legitimate variability. Until we can find some way of validating individual questionnaires this must be so. But we had hoped that if there were only acceptable amounts of variability where they were to be expected, it could safely be assumed that serious inaccuracies were absent, and further that they would be absent in the other questions.

Mr. Mason wants to know how to use the results of the inquiry. We think we should have to do much more research before we could tell him. Not every experiment, even in market research, has a clear-cut result and an immediate commercial application. But we shall go on experimenting all the same.

As to Mr. Mason's query on the timing, the answer is that so far as practicable, comparable pairs of interviewers worked about the same time. Any serious temporal effect should be removed in the comparison of blocks of two.

We are glad that Miss Harris was stimulated to give an account of the coding procedure and we think that practitioners generally will agree with her conclusions.

Mr. L. W. Wilkins makes a fair point, but an examination of the V or χ^2 values does not suggest that anything is too good to be true; or so it seems to us.

As we judge Mr. Nicholson's comments, what he is really asking us is whether we have found anything that everybody in the social survey field did not know already? The trouble is that different people in that field know already so many different and conflicting things. We are trying to settle some of their points of difference by objective methods. We are, however, very grateful for his comments and a number of suggestions which he has sent us privately for lines of research which he would like to see pursued.

Dr. Durant in his written contribution seems likely to mislead those unacquainted with his eristic technique. We should like, therefore, to make clear at what stage in the planning of the inquiry Dr. Durant decided to withdraw from the project and the extent to which, in the reasons given for his non-participation, he anticipated the results.

The British Institute of Public Opinion is indeed a member of the Committee which was concerned in a general way with the organization of the inquiry, but its representative attended only two of the early meetings of the sub-committee which planned it. Dr. Durant decided to withdraw from the project before even the subject matter of the questionnaires had finally been decided upon and at a time when no decision had been taken regarding either briefings or substitutes. The reasons for which he withdrew were not concerned primarily with dissatisfaction with the questionnaires, opposition to substitutes, or his views on briefing methods. He withdrew, in fact, not because he objected to the details of a project as yet unplanned, but because he felt that a design which compared interviewers from different organizations by making them work under similar conditions could not provide his organization with results of any value.

The questionnaires were drafted by several experts in practical market research work. A number of comments made by Dr. Durant on an early draft were accepted, although he had already withdrawn from the project. The fact that there were others which were not accepted has, unfortunately but not altogether surprisingly, meant that the experiment was indeed "doomed to secure results which were open to disputation" by Dr. Durant.

As a result of the ballot taken during the meeting, the candidates named below were elected Fellows of the Society:

John Ralph Ainscough.
Patrick Bergin.
Francis Emeric Binet.
Ronald Llewellyn Bonnett.
Erika Grassau.
Ernest Gregory.
Samuel James Kilpatrick.
Janet Elizabeth Lillistone.
Himansu Nag.
Peter Pereira.
Harry Lingard Thorpe.

Corporate Representatives

Ian Gray Ross, *representing* British Nylon Spinners Ltd.
Frank D. Sando, *representing* Albert E. Reed & Co.

ECONOMICAL ACCEPTANCE SAMPLING SCHEMES

By G. HORSNELL
Ministry of Supply

[Read before the ROYAL STATISTICAL SOCIETY, February 20th, 1957, the PRESIDENT, Professor E. S. PEARSON, C.B.E., in the Chair]

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1. HISTORICAL

THE costs involved in using Acceptance Sampling and Rectifying Inspection Schemes have always been recognized as an important factor in the choice of such schemes. For example, in their earliest paper on Rectifying Inspection Schemes, Dodge and Romig (1929) recommended the adoption of schemes which, subject to given conditions, involve the *minimum* amount of inspection for lots of process average quality. Their single and double sample schemes (1944) require the specification of either:

(i) A lot tolerance percentage of defectives (L.T.P.D.), at which quality the operator of the scheme is willing to allow a probability of 0·10 of accepting a lot, if a lot is offered at that quality, or

(ii) an average outgoing quality limit of percentage of defectives (A.O.Q.L.) in all lots passed by the schemes.

The schemes apply where the items in the lots can be classified into two mutually exclusive categories, such as defective and effective, and to inspection which does not affect the product. Lots which fail the sample inspection are 100 per cent. inspected and all defective items removed and, strictly speaking, replaced by effectives. Although in the Dodge-Romig schemes, and the corresponding sequential schemes tabled by Anscombe (1949), the amount of inspection for lots of a given quality is minimized, the choice of the basic parameter determining the schemes (i.e. the L.T.P.D. or A.O.Q.L.) is left to general economic considerations, and the operation of these economic factors is not given mathematical expression.

There are a large number of industrial problems where 100 per cent. inspection of failed lots is not desirable, an obvious example being the case where inspection involves the destruction of the product. A suitable method of choosing an Acceptance Sampling Scheme in these problems is to specify a level of good quality at which product should have a low probability of rejection and a level of bad quality at which product should have a low probability of acceptance. These two levels of quality will be termed the producer's and consumer's risk points in this paper and the associated small risks of rejection and acceptance, the producer's and consumer's risks, respectively. Tables for schemes chosen in this way are given by Peach (1947), Anscombe (1949) and Horsnell (1954) among others. Although general advice is given in these references on the costs to be considered in the choice of the risk points and associated risks, these parameters are again not determined from a mathematical treatment of the costs consequent on operating the schemes, but by empirical methods.

A fourth way of specifying a sampling scheme is to concentrate on choosing the most suitable scheme for a fixed maximum amount of initial sampling inspection or average amount of initial sampling inspection, at some quality, per lot (final inspection may be 100 per cent.). The limitation on the maximum or average sample size to be taken is governed by:

(i) the expected effect that the inspection will have on the process curve,

(ii) the importance of the inspection costs relative to the amount of discrimination obtained,

(iii) the lot size,

or may be due to:

(iv) temporary or permanent restrictions on inspection facilities.

The Statistical Research Group, Columbia University (1948), provides tables from which some suitable schemes may be selected when the factors (i) and (ii) can be assessed in the form of a desirable "level of inspection" per lot. Guidance is given on the inspection levels, and on the associated empirical relationships of the sample size to the lot size for Single-sample Acceptance Schemes, which have been found useful in some experiences with Army Ordnance problems. For the various single-sample sizes and acceptable-quality levels (A.Q.L.), double and multiple-sampling schemes are tabled which have similar operating characteristic curves. Where (iv) above is the major factor of importance the S.R.G. (1948) tables may again be used. The well-known Military Standard 105A, Sampling Procedures and Tables for Inspection by Attributes, reproduced in Duncan (1952) and Grant (1952) provide similar facilities to the S.R.G. (1948) tables. Alternatively, where output is continuous and the sample size per lot must be small, Deferred Sentencing Schemes, as proposed by Anscombe *et alia* (1947) are often suitable. Such schemes are, however, more in the nature of Quality Control or Quality Assurance Schemes than Acceptance Sampling Schemes.

None of the above methods of specifying a scheme requires or is designed to use exact information regarding the distribution of the proportion of defectives in the batches or lots of the product (i.e. the process curve), although a reasonable estimate of the process average is required for a satisfactory specification of the schemes. Neither do these methods require the formulation of a cost function, in mathematical terms, which embodies the costs of inspection and the costs of wrong decisions. It is obvious that if accurate information is available in a mathematical form of the process curve of the lots which will be submitted, and of the inspection and decision costs, appropriate Acceptance Sampling and Rectifying Inspection Schemes may be chosen which will be, on the average, the most economical schemes possible in regard to their operation and consequences.

Anscombe (1951), following Barnard (1948), gives a general discussion of the above problem, and also an illustrative example in rectifying inspection. Sittig (1951), Satterthwaite (Hamaker, 1951), Champernowne (1953*a*, 1953*b*), Anscombe (1954) and Barnard (1954) propose methods of specifying schemes for a number of process curves where both the curves and cost functions are known exactly. In addition, Barnard (1954) shows that, with different process curves, it is possible to obtain quite different solutions to the problem of the optimum choice of an Acceptance Sampling Scheme, although the cost functions assumed are the same and the process curves have the same mean and variance. Moriguti (1955) provides *minimax regret* solutions for the case where decision costs are known but no knowledge of the process curve is assumed. In none of the methods proposed by the writers cited above, does the Operating Characteristic Curve (O.C. Curve), of the appropriate scheme, enter *explicitly* into the solution of the problem. Weibull (1951) adopts the more classical approach in that he recommends the choice of Rectifying Inspection Schemes for which the probability of accepting offered lots at "break-even" quality is one-half. (Break-even quality, as defined by Satterthwaite, is that quality at which, from the cost point of view, it is immaterial whether the lot is accepted without inspection or is 100 per cent. inspected.) In addition, Weibull recommends that the schemes should minimize the average amount of inspection for lots of process average quality, as recommended by Dodge and Romig. The same general principles govern Weibull's choice of Acceptance Sampling Schemes.

2. CLASS OF PROBLEM TO BE CONSIDERED AND PRACTICAL BACKGROUND

For a complete solution of the decision problem in sampling inspection, it is necessary, of course, that all costs incurred should be expressible in terms of a common unit (Anscombe, 1951). In fact, the previous work, listed above, has been concerned with the decision problem when this can be done. However, there is a large class of sampling inspection problems in which it is not possible to assess the costs of wrong decisions (usually the costs of accepting bad quality product) in the same units in which the costs of inspection may be assessed. For example, this applies to the acceptance of ammunition, when it is not known in what circumstances, or with what consequences, accepted lots of bad ammunition may be used. There may well be similar considerations involved in the purity or toxicity testing of drugs. Alternatively, although the costs of wrong decisions may be expressible in the same *units* as the costs of inspection, it may be virtually impossible to predict or evaluate these costs or to agree on a mathematical model to express them. This may well be true for example when, prior to market research, the relationship between deterioration in product quality and consumer resistance is not known and in any case may be expensive to find out.

The method of choosing an Acceptance Sampling Scheme by specifying the producer's and the consumer's risk points and their associated risks, has already been mentioned. When the ultimate costs of accepting bad quality product cannot be determined for various reasons and it is desired to operate an Acceptance Sampling Scheme, there is no other course but to choose the consumer's risk point and risk on heuristic grounds if the scheme is to be selected in the above way. This need not always be the case with the producer's risk point and risk. If a producer is passing his product through an Acceptance Sampling Scheme before finally putting it on the market, he will often be in the position where he is satisfied with the usual quality of his product and wishes to accept as much of it as possible at minimum expense to himself, subject to a fixed safeguard against passing on bad-quality product, which is unlikely to be produced. Similarly, a consumer, if he has been able to find a product at the right price and of the average quality that he wants, will select an Acceptance Sampling Scheme which will accept as much as possible of the normal product, at least cost to himself, subject to a fixed safeguard against accepting bad-quality product which, in any event he is unlikely to be asked to buy. In both of these cases the producer's risk point may conveniently be chosen to be approximately equal to a central measure of the process curve of normal production, usually the process average: the process curve or lot distribution will generally be grouped fairly tightly about this central measure and the upper tail of the process curve will be some distance away from the consumer's risk point. There will then be no contradiction in requiring a scheme to accept as much of normal production as economically practicable whilst requiring the same scheme to reject a very high proportion of any product offered at the consumer's risk point. Fig. 1 shows a typical arrangement of a process curve and the producer's and consumer's risk points, as envisaged in this paper. The lot distribution represented here by a continuous process curve, is for illustrative purposes only. It is, however, one of the type that can be obtained if the process is in statistical control with a constant mean level. We will assume that the circumstances discussed above are the ones in which it is required to design the most economical Acceptance Sampling Scheme.

When the consumer's risk point and risk and the producer's risk point have been chosen, the producer's risk remains to be specified. It is obvious from a knowledge of the behaviour of the O.C. curves of sampling schemes in general that for fixed risk points and consumer's risk, the producer's risk may be made as small as desired by increasing the average number of items to be sampled and inspected per lot. There comes a time, however, when the extra average inspection cost that will be incurred in reducing the producer's risk still further is just not worth the money that will, on the average, be saved in the extra amount of product of normal quality which is accepted. For a given type of Acceptance Sampling Scheme, the producer's risk for which this stationary value occurs will be the risk for the most economical scheme that can be obtained under the given conditions. In other words, the most economical scheme will be the one for which the sum of the average total costs of inspection and of the rejection of lots of normal quality is a minimum.

Suppose that $y(p)$ the process curve of normal production is known and that some type of lot-by-lot acceptance sampling scheme is being operated by a producer or by a consumer on lots of N items which cost Nc_m units. The average cost per lot due to the labour of applying the scheme may be written as $\bar{C}(n_s, n_i,)$ where n_s, n_i, are the number of items sampled and inspected respectively in a given lot before a decision is reached. If the scheme accepts a proportion $A(p)$ of lots containing $100p$ per cent. defective items, a proportion $\int_0^1 A(p) \, . \, y(p) \, . \, dp$ of lots of normal production will be accepted and a proportion $1 - \int_0^1 A(p) \, . \, y(p) \, . \, dp$ will be rejected. (The integral signs are to be interpreted as summation signs when $y(p)$ is a discrete distribution.) Since $y(p)$ is the process curve of normal production, it is assumed in this paper that the lots which are rejected are just as acceptable as the lots which are accepted. It is of course true that, for most process curves, the average proportion of defective items is higher in the lots which are rejected than that in the lots which are accepted. But, for normal production, the average proportion of defectives in rejected lots will still be acceptable and the difference in the two proportions will usually be small and of negligible importance. (For a process curve following the binomial law, the average proportion of defective items in rejected lots will actually be equal to that in accepted lots if inspection is destructive (Mood, 1943)). Hence, in the circumstances assumed, rejected lots represent a total loss to a producer who is operating the scheme, unless, by 100 per cent. inspection of these lots, the value of the items or components of the items salvaged is greater than the costs of salvaging. Any savings that may be effected by salvaging rejected items must be offset against the costs of operating the scheme. In addition, rejected lots may involve a producer in positive costs, other than salvaging costs, due to the resulting dislocation in production, to delay caused in filling orders, and to other reasons.

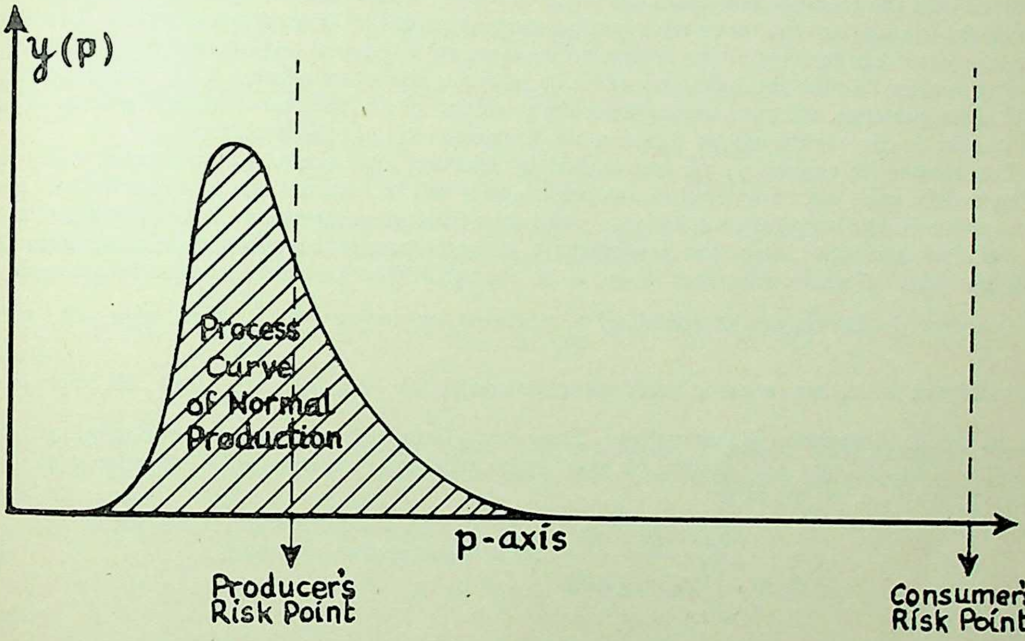

FIG. 1.—Envisaged relationship between Risk Points and Process Curve.

The situation is more complicated in the case of a consumer who is operating an Acceptance Sampling Scheme. Only in the unusual case where the consumer is unable to return rejected product to the producer, is it clear that rejected lots of normal manufacture will have no more than salvage value. In the more usual case where the consumer is able to return rejected lots to the manufacturer as not being to the consumer's requirements, the consumer may expect to receive compensation in the form of another lot or a return of the purchase price. Even with normal manufacture, the producer must expect a small proportion of lots to be returned to him if his consumers are operating Acceptance Sampling Schemes. As there will be no appreciable difference in quality between the lots which are returned and the lots which are accepted, the producer may reasonably put up his selling price to cover the loss that he may expect to suffer through the return of normal quality lots. Hence, although the consumer may suffer no apparent loss in rejecting lots of normal quality (apart from any costs arising from the inconvenience caused), the cost of rejecting good quality product may be reflected in the producer's selling price in the first instance. For example, a manufacturer may be supplying two consumers X and Y with the same product. Consumer X is not operating an Acceptance Sampling Scheme but consumer Y is. Consumer Y occasionally returns lots of good quality product under a money-back-if-not-satisfied guarantee. The manufacturer may increase his selling price to both X and Y to cover his losses on the money-back guarantee, or he may justifiably charge Y more than X for the same product. It will be assumed that the manufacturer takes the latter course, in which case the extra cost per lot to Y will be the average loss per lot to the manufacturer represented by items returned by Y. In other words, rejected lots returned to the producer may represent a net total loss to the consumer, except for any savings that the producer may effect by salvaging the lot or offering it for sale elsewhere. With these premises, the costs arising from the operation of an Acceptance Sampling Scheme by a producer or by a consumer are therefore no different in kind in the two cases.

For reasons of simplicity, the loss caused by rejecting lots, due to inconvenience, delay in filling orders, etc., will be ignored in this paper and it will be assumed too that rejected lots are of no value to the operator of a scheme. After non-destructive inspection, N items remain in a lot but after destructive inspection on average $(N - \bar{n}_{ai}(p))$ items remain in accepted lots containing $100p$ per cent. defective items where $\bar{n}_{ai}(p)$ is the average number of items inspected before such a lot is accepted. Hence, per lot submitted to a scheme, on average $N \, . \int_0^1 A(p) \, . \, y(p) \, . \, dp$ items are accepted when inspection is non-destructive and $\int_0^1 (N - \bar{n}_{ai}(p)) \, A(p) \, . \, y(p) \, . \, dp$ items are accepted when inspection is destructive. These items have been obtained at an average cost of $N \, . \, c_m + \bar{C}(n_s, n_i)$ per lot. Hence, we may define what may be called the "effective cost" of accepted items, c, by the ratios

$$c = \frac{Nc_m + \bar{C}(n_s, n_i)}{N \, . \int_0^1 A(p) \, y(p) \, dp} \text{ for non-destructive inspection} \quad . \quad . \quad . \quad (1)$$

and

$$c = \frac{Nc_m + \bar{C}(n_s, n_i)}{\int_0^1 (N - \bar{n}_{ai}(p)) \, A(p) \, y(p) \, dp} \text{ for destructive inspection} \quad . \quad . \quad (2)$$

Obviously, subject to the requirement of a fixed consumer's risk point and risk, the most economical Acceptance Sampling Scheme when the quality of normal production is being maintained is that for which c is a minimum. The properties of Single-sample Schemes for which c is a minimum will now be examined in some detail.

3. THEORETICAL MODEL AND THEORETICAL SOLUTION

The Single-sample Schemes considered will be those in which the whole sample is inspected. It will be assumed that the costs of sampling, inspection, and replacement of sampled items are linearly dependent on n, where n is the sample size, and are zero if no items are sampled. The schemes will be specified by the two parameters n and k where k is the number of defective items, which, if equalled or exceeded in a sample of n items, randomly drawn, will lead to the rejection of the sampled lot. The choice of schemes is limited to those which have a fixed probability of rejecting any lots which may be submitted containing a fraction p_1 of defective items, where $100 \, p_1$ per cent. is the consumer's risk point. Hence, the condition on n and k is that

$$A(p_1, n, k) = \beta \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

where $A(p, n, k)$ is the probability of accepting any lot offered which contains 100 p per cent. of defective items, when using the single-sample scheme with the parameters n and k, and β is the specified consumer's risk. Equation (3) defines a sequence of schemes (n, k) which may be arranged in ascending order of k. n and k need not necessarily be integral (see Hamaker (1950) for the way in which the extension to non-integral values may be made), but for all schemes considered here, k will be a positive integer and in most cases n also. The general restriction to integral values of n and k will mean that equation (3) will not be exactly satisfied, but this restriction is usual, of course, when applying a scheme in practice.

Let n_{-1}, n_0, and n_1 denote consecutive values of n in order of increasing magnitude, corresponding to $k = r - 1$, r and $r + 1$ in (3). The cost of accepting an item, c, will be a local minimum for the scheme (n_0, r) if

$$c_{n_{-1}} \geqslant c_{n_0} \leqslant c_{n_1} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (4)$$

Write

$$\int_0^1 A(p, n, k)\, y(p)\, dp = A(n, k) \quad . \quad . \quad . \quad . \quad . \quad . \qquad (5)$$

Then for non-destructive inspection, from (1), (4) becomes

$$\frac{Nc_m + n_{-1}\, c_s}{N\, .\, A(n_{-1}, r-1)} \geqslant \frac{Nc_m + n_0\, c_s}{N\, .\, A(n_0, r)} \leqslant \frac{Nc_m + n_1\, c_s}{N\, .\, A(n_1, r+1)} \quad . \quad . \quad . \qquad (6)$$

where c_s is the cost, per item sampled, of the operating costs of the scheme.

Similarly, for destructive inspection, from (2), (4) becomes

$$\frac{Nc_m + n_{-1}\, c_s}{(N - n_{-1})A(n_{-1}, r-1)} \geqslant \frac{Nc_m + n_0\, c_s}{(N - n_0)A(n_0, r)} \leqslant \frac{Nc_m + n_1\, c_s}{(N - n_1)\, A(n_1, r+1)} \quad . \qquad (7)$$

Write

$$(N - n_t)\, A(n_t, r + t) = A'(n_t, r + t) \quad . \quad . \quad . \quad . \quad . \qquad (8)$$

Then (7) is the same as (6) with A' written for A. In what follows, therefore, it will not be necessary in general to make a distinction between non-destructive and destructive inspection, and unless otherwise stated A may be read as A'.

The inequalities (6) may be re-written as

$$\frac{c_m}{c_s}\, .\, N[A(n_0, r) - A(n_{-1}, r-1)] \geqslant n_0 A(n_{-1}, r-1) - n_{-1} A(n_0, r) \quad . \quad . \qquad (9)$$

and

$$\frac{c_m}{c_s}\, .\, N[A(n_1, r+1) - A(n_0, r)] \leqslant n_1 A(n_0, r) - n_0 A(n_1, r+1) \quad . \quad . \qquad 10))$$

Now in the circumstances considered in this paper, with non-destructive inspection, $A(n_t, r + t)$ will increase strictly monotonically with t over the range of values of t of practical interest. Hence

$$A(n_{-1}, r-1) < A(n_0, r) < A(n_1, r+1) \quad . \quad . \quad . \quad . \quad . \qquad (11)$$

and therefore the left-hand sides of (9) and (10) will always be positive. For destructive inspection, the left-hand sides of (9) and (10) or (10) alone may be negative since

$$A'(n_t, r + t) = (N - n_t)\, A(n_t, r + t)$$

will increase monotonically to a maximum value. This will be attained when any increase in t, although increasing $A(n_t, r + t)$ would decrease $(N - n_t)\, A(n_t, r + t)$. In fact, after reaching this maximum, attained at $t = w$ say, $A'(n_t, r + t)$ will decrease monotonically with t to $A'(n_z, r + z)$ where n_z is the largest value of n less than N which satisfies (3). However, if the left-hand side of (10) is negative, the right-hand side will be positive, *a fortiori*, and hence (10) will be satisfied for any value of c_m/c_s since c_m/c_s is essentially positive. If the left-hand side of (9) is also negative, the right-hand side of (9) will be positive and (9) cannot be satisfied for any positive value of c_m/c_s. Hence under these latter conditions, that is, if $w < 0$, c_{n_0} cannot be a local minimum. Assuming that $w = 0$, the inequalities (9) and (10) may be replaced by

$$\frac{n_0 A(n_{-1}, r-1) - n_{-1} A(n_0, r)}{N[A(n_0, r) - A(n_{-1}, r-1)]} \leqslant \frac{c_m}{c_s} \leqslant \infty \quad . \quad . \quad . \quad . \quad (12)$$

where the left-hand member of (12) may be replaced by zero if the numerator is negative.

It happens in practice that with some process curves, the right-hand sides of (9) and (10) or (9) alone may be negative. For $w > 0$, if the right-hand side of (9) is negative and of (10) positive, (9) and (10) may be written as

$$0 \leqslant \frac{c_m}{c_s} \leqslant \frac{n_1 A(n_0, r) - n_0 A(n_1, r+1)}{N[A(n_1, r+1) - A(n_0, r)]} \quad . \quad . \quad . \quad . \quad (13)$$

If both the right-hand sides of (9) and (10) are negative ($w > 0$), obviously c_{n0} cannot be a local minimum. In summary therefore, c_{n0} cannot be a local minimum if the left-hand side of (9), or the right-hand side of (10) is negative.

By an extension of (6), (9) and (10), the conditions that c_{n0} should be an absolute minimum for all possible schemes (n, k) are

$$\frac{c_m}{c_s} \,.\, N[A(n_0, r) - A(n_{-u}, r-u)] \geqslant n_0 A(n_{-u}, r-u) - n_{-u}\, A(n_0, r) \quad . \quad . \quad (14)$$

and

$$\frac{c_m}{c_s} \,.\, N[A(n_v, r+v) - A(n_0, r)] \leqslant n_v A(n_0, r) - n_0 A(n_v, r+v) \;. \quad . \quad . \quad (15)$$

for $1 \leqslant u \leqslant r$ and $1 \leqslant v \leqslant z$ for "hypergeometric" sampling, or $1 \leqslant v \leqslant \infty$ if sampling is "binomial", for non-destructive inspection, and $1 \leqslant v \leqslant w$ for destructive inspection. It will now be shown that for (14) and (15) to hold if (9) and (10) hold, it is sufficient for the fractions

$$\frac{n_t A(n_{t-1}, r+t-1) - n_{t-1} A(n_t, r+t)}{N[A(n_t, r+t) - A(n_{t-1}, r+t-1)]} \quad . \quad . \quad . \quad . \quad (16)$$

to increase monotonically with t over the range $t = -r+1$ to $t = z$ (or ∞) for non-destructive inspection, and over the range $t = -r+1$ to $t = w$ for destructive inspection.

Consider first the inequalities (9) and (14). It has been shown that for c_{n0} to be a local minimum the left-hand side of (9) must be positive and hence the condition (9) may be written as

$$\frac{c_m}{c_s} \geqslant \frac{n_0 A(n_{-1}, r-1) - n_{-1} A(n_0, r)}{N[A(n_0, r) - A(n_{-1}, r-1)]} \quad . \quad . \quad . \quad . \quad (17)$$

Now the condition on the fractions (16) is equivalent to requiring that the fractions

$$\frac{A(n_t, r+t) - A(n_{t-1}, r+t-1)}{n_t - n_{t-1}} \quad . \quad . \quad . \quad . \quad . \quad (18)$$

decrease monotonically with t, over the range $t = -r+1$ to z (or ∞) or $t = -r+1$ to w. Hence, by adding the numerators and the denominators together separately, for $t = -1$ to $-u+1$

$$\begin{aligned} \frac{A(n_{-1}, r-1) - A(n_{-u}, r-u)}{n_{-1} - n_{-u}} &\geqslant \frac{A(n_{-1}, r-1) - A(n_{-2}, r-2)}{n_{-1} - n_{-2}} \\ &\geqslant \frac{A(n_0, r) - A(n_{-1}, r-1)}{n_0 - n_{-1}} \quad . \quad . \quad . \quad (19) \end{aligned}$$

(the last inequality following directly from (18)). Hence

$$\begin{aligned} A(n_0, r)\{(n_{-1} - n_{-u}) \,.\, [A(n_0, r) - A(n_{-1}, r-1)] \\ - (n_0 - n_{-1}) \,.\, [A(n_{-1}, r-1) - A(n_{-u}, r-u)]\} \leqslant 0 \quad . \quad (20) \end{aligned}$$

which, after re-writing, dividing through by $[A(n_0, r) - A(n_{-1}, r-1)]$ and using (17) yields

$$[n_0 \,.\, A(n_{-u}, r-u) - n_{-u} \,.\, A(n_0, r)] - \frac{c_m}{c_s} \,.\, N \,.\, [A(n_0, r) - A(n_{-u}, r-u)] \leqslant 0 \quad . \quad (21)$$

Hence if (9) holds and condition (16) is satisfied, the inequality (14) is satisfied. Similarly, it may be shown that if (10) and (16) hold the inequality (15) is satisfied. Hence the theorem is proved.

Assume that, for a particular process curve $y(p)$ and sequence of schemes (n, k) the condition (16) is satisfied and the fractions are positive for $t \geqslant -y$ and negative for $-r+1 \leqslant t < -y$ for non-destructive inspection, whilst for destructive inspection and a lot size N, the fractions are positive for $t \geqslant -l$ $(l \leqslant w)$ and negative for $-r+1 \leqslant t < -l$. Then each of the single-sample schemes of the sequence

$$(n_{-y-1}, r-y-1),\ (n_{-y}, r-y),\ \ldots\ (n_0, r),\ \ldots\ (n_T, r+T)\ \ldots\ [T \to z \text{ (or } \infty)]$$

for non-destructive inspection, and of the finite sequence

$$(n_{-l-1}, r-l-1),\ (n_{-l}, r-l),\ \ldots\ (n_0, r),\ \ldots\ (n_w, r+w)$$

for destructive inspection, with its adjacent member or members, defines bounds for the ratio of c_m to c_s for which that scheme is the most economical in the sense of this paper, for product of normal quality. Hence for a given process curve and lot size, tables may be prepared which, for a given value of c_m/c_s, will show the most economical single-sample scheme that may be used with a specified consumer's risk and consumer's risk point, where product is of normal quality and is such that it is desired to accept as much of the product as is economically possible.

4. NUMERICAL TABLES OBTAINED

In any tabulation of such schemes, the easy evaluation of $A(n, k)$ is important. Below are listed some of the discontinuous process curves $y(X)$, (X = number of defective items in a lot of size N), and continuous process curves $y(p)$, $(p = X/N)$ which may be combined with various expressions for $A(p, n, k)$ or

$$A\left(\frac{X}{N}, n, k\right),$$

together with the resulting expressions for $A(n, k)$.

TEXT-TABLE 1

Curve	$A(p, n, k)$ *or* $A(X/N, n, k)$	$A(n, k)$
1. $y(X) = {}^NC_X\, p_0^X (1-p_0)^{N-X}$	$\sum_{x=0}^{k-1} \frac{{}^nC_x\, {}^{N-n}C_{X-x}}{{}^NC_X}$	$\sum_{x=0}^{k-1} {}^nC_x\, p_0^x (1-p_0)^{n-x}$
2. $y(X) = \frac{{}^NC_X S^{(X)} T^{(N-X)}}{(S+T)^{(N)}}$	Ditto	$\sum_{x=0}^{k-1} y(x)\ N=n$
3. $y(X) = \frac{{}^NC_X (S+X)^{(X)} (T+N-X)^{(N-X)}}{(S+T+N+1)^{(N)}}$	Ditto	$\sum_{x=0}^{k-1} y(x)\ N=n$
4. $y(X) = 1,\ X = Np_0;\ y(X) = 0,\ X \neq Np_0$	$\sum_{x=0}^{k-1} {}^nC_x \left(\frac{X}{N}\right)^x \left(1-\frac{X}{N}\right)^{n-x}$	$\sum_{x=0}^{k-1} {}^nC_x\, p_0^x (1-p_0)^{n-x}$
5. Rectangular: one or two terminals $\neq 0$ or 1	$\sum_{x=0}^{k-1} {}^nC_x\, p^x (1-p)^{n-x}$	Sum of Incomplete Beta Functions
6. Triangular: Ditto	Ditto	
7. $y(p) = \frac{\Gamma(s+t)}{\Gamma(s)\,\Gamma(t)} p^{s-1} \cdot (1-p)^{t-1}$, $0 \leqslant p \leqslant 1$ $= 0$ otherwise; $s, t \geqslant 1$	Ditto	Sum of multiples of ratios of Gamma Functions
8. $y(p) = \frac{M^t}{\Gamma(t)} e^{-Mp} \cdot p^{t-1}$, $0 \leqslant p \leqslant \infty$	$\sum_{x=0}^{k-1} (np)^x \frac{e^{-np}}{x!}$,	Incomplete Beta Function $I_{M/M+n}(t, k)$

Curves 1, 2 and 3 were suggested by Mood (1943); Champernowne uses Curve 3 when N is small and Curve 7 when N is large. Sittig uses Curve 7 with $s = 1$. The simplest expressions for $A(n, k)$ are obviously those obtained with Curves 1, 4 and 8. The tables for single-sample schemes which are given in the Appendix at the end of this paper have been obtained by using the Poisson approximation to the Binomial expression for $A(n, k)$ and hence must be taken to refer to process curves such as curves 1 and 4 together with hypergeometric (sampling without replacement) or binomial sampling respectively. The Poisson approximation has also been used to solve equation (3) and hence the values of β (0·05 and 0·01) for which the schemes are tabled are nominal only. The errors in this approximation have been investigated and can be gauged by the summary of tail-area comparisons given below:

TEXT-TABLE 2

			Poisson Over-estimate, as a Percentage of Poisson Tail Area, of			
				Hypergeometric Tail Area		
Sample Size n	*Binomial Parameter* $p=X/N$	*Tail Area (Poisson)*	*Binomial Tail Area* (%)	N=10,000 (%)	N=30,000 (%)	n/N=0·10 (%)
100 . . .	0·10	0·05	16	18	—	31
		0·01	24	26	—	44
	0·05	0·05	8	9	—	23
		0·01	12	14	—	32
300 . . .	0·10	0·05	17	21	18	31
		0·01	27	33	29	48
	0·05	0·05	8	13	9	24
		0·01	13	20	15	36
1,000 . . .	0·10	0·05	16·8	—	—	—
		0·01	27·6	—	—	—

It was found necessary in a few tables to take n to one decimal place in order to satisfy condition (16).

The tables list the bounds of c_m/c_s within which a particular scheme is the most economical. Values of $A(n, k)$ or $A'(n, k)$ are also given for the schemes and the effective cost of accepted items, c, is given in terms of c_m for the schemes at the boundary values of c_m/c_s. This is termed the effective cost ratio and is given by

$$\frac{c}{c_m} = \frac{1 + n/N \;.\; c_s/c_m}{A(n, k)} \text{ for non-destructive inspection . . . } \tag{22}$$

and

$$\frac{c}{c_m} = \frac{N + n \;.\; c_s/c_m}{(N - n) \;.\; A(n, k)} \text{ for destructive inspection . . . . } \tag{23}$$

The use of the Poisson approximation to evaluate $A(n, k)$ leads to some errors and these have been investigated by a number of spot-checks with the results given in Text-table 3. Six pairs of consecutive non-destructive single-sample schemes which are included in Tables 1, 3 and 4 have been examined. The upper limit to the value of c_m/c_s for which the first scheme of each pair is more economical than the second has been determined by using the Binomial series to evaluate $A(n, k)$ and these results are compared with those obtained by using the Poisson limit. The adjacent values of the ratio c/c_m are the values for either of the schemes of the pairs when c_m/c_s is equal to the upper limit for which the first scheme is more economical. Similar spot-checks have been made in the tables for destructive inspection. It will be seen that the use of the Poisson approximation may occasionally result in the selection of one scheme higher in the sequence than the most economical scheme. However, the difference in the corresponding values of c/c_m for the two schemes would be trivial. For this reason too, the values of c_m/c_s in the tables in the Appendix, although calculated to more significant figures, are only tabled to two. However, owing to the Poisson approximation, the non-destructive tables have been curtailed when the denominator of c_m/c_s was just greater than 0·000600. No schemes are tabled for $k \geqslant 40$ or $n > 1{,}000$.

TEXT-TABLE 3

Table	*Scheme*		$A(n, k)$ ($A'(n, k)$ *for Destructive Inspection*)		c_m/c_s		c/c_m	
	n	k	*Poisson*	*Binomial*	*Poisson*	*Binomial*	*Poisson*	*Binomial*
			Non-destructive Inspection: $N = 10{,}000$					
1 .	607	12	0·978285	0·978872	0·6031	0·6152	1·125	1·122
	648	13	0·984327	0·984810	0·8630	0·8832	1·092	1·090
	689	14	0·988677	0·989069				
	890	19	0·997849	0·997966	6·11	6·40	1·0168	1·0160
	929	20	0·998477	0·998566				
3 .	60	1	0·548812	0·547157	0·00335	0·00325	5·08	5·20
	95	2	0·754147	0·754241				
4 .	60	1	0·301194	0·297553	0·00195	0·00179	13·5	14·6
	95	2	0·433749	0·431165				
4* .	837	32	0·999420	0·999494	18·2	20·4	1·0052	1·0046
	860	33	0·999546	0·999606				
			* Beyond range of table given in this paper.					
			Destructive Inspection					
			$N = 10{,}000$					
12 .	607	12	9,189·03	9,194·54	2·24	2·39	1·118	1·115
	648	13	9,205·43	9,209·94	270	∞	1·087	1·086
	689	14	9,205·57	9,209·22	∞	—	1·086	—
14 .	60	1	5,455·19	5,438·74	0·00348	0·00337	4·99	5·11
	95	2	7,469·83	7,470·76				
15 .	60	1	2,993·87	2,957·68	0·00205	0·00188	13·1	14·2
	95	2	4,296·28	4,270·69				
			$N = 20{,}000$					
22 .	607	12	18,971·9	18,983·3	0·476	0·491	1·121	1·119
	648	13	19,048·7	19,058·0	0·863	0·900	1·089	1·087
	689	14	19,092·3	19,099·9				
24 .	60	1	10,943·3	10,910·3	0·00171	0·00165	5·04	5·17
	95	2	15,011·3	15,013·2				
25 .	60	1	6,005·81	5,933·21	0·00100	0·00092	13·3	14·4
	95	2	8,633·77	8,582·34				

The tables for non-destructive inspection, although nominally for a lot size of 10,000 items, can be used for any other lot size by a suitable scale adjustment. This can readily be seen from the inequalities (9) and (10) since N enters in as a simple multiplication factor. Suppose that condition (16) is satisfied and the boundary values of c_m/c_s within which (n_0, r) is the most economical scheme for $N = 10{,}000$ have been derived from (9) and (10) by replacing the inequality signs by equality signs. For any other value of N, the equations will still hold if the boundary values of c_m/c_s for $N = 10{,}000$ are multiplied by $10{,}000/N$ to give new boundary values applicable to the new lot size. Hence tables 1–11 may in theory be used for any other lot size N by multiplying the true value of c_m/c_s for which it is required to find the most economical single-sample acceptance scheme by $N/10{,}000$ and entering the appropriate table. For example, if $N = 5{,}000$ and $c_m/c_s = 1 \cdot 0$, the appropriate table for $N = 10{,}000$ is entered with $c_m/c_s = 0 \cdot 5$. It is also seen from (22), that, at the boundary values of c_m/c_s within which (n, k) is the most economical scheme, c/c_m is independent of lot size, since any adjustment of the boundary values to compensate for changes in lot size is cancelled out by a corresponding adjustment to the factor n/N. Hence Tables 1–11 will always give an indication of the range of cost of a scheme provided that the true value of c_m/c_s is within the range for which the scheme is the most economical.

In practice, the usefulness of the tables for other lot sizes is governed by the approximations used to derive them. The schemes tabled purport to be schemes with fixed consumer's risks, whereas in fact, owing to the approximation used, they are schemes for which

$$\frac{1}{(k-1)!}\int_{np_1}^{\infty} t^{k-1}\, e^{-t}\, dt = \beta \quad . \quad . \quad . \quad . \quad . \quad (24)$$

If sampling is "hypergeometric", as is normally the case, from Text-table 2, the error incurred by using (24) when $n/N = 0 \cdot 10$ may be as much as 31 per cent. and 48 per cent. with $p = 0 \cdot 10$ and 24 per cent. and 36 per cent. with $p = 0 \cdot 05$ at the $0 \cdot 05$ and $0 \cdot 01$ levels of the Poisson distribution respectively. For $n/N > 0 \cdot 10$ the errors will increase and the differences between schemes with fixed consumer's risks and those tabled will become more important.

However, the tables for non-destructive inspection may be used for any lot size if, instead of requiring schemes with fixed consumer's risks, it is sufficient to select the most economical scheme from those schemes which would accept a fixed small proportion β of lots if these lots were distributed binomially about the consumer's risk point. This follows from curve 1 with p_1 written for p_0, since the Poisson approximation to the binomial distribution tail areas is independent of N and the errors will therefore be acceptable for all lot sizes. In other words, Tables 1–11 may be used for any lot size if Barnard's (1954) concept is accepted of a two-component binomial distribution where the two components are situated at p_0 and p_1. In the situation considered in this paper, however, the relative frequency of the two components is an unknown factor and only affects the choice of the scheme in the selection of the value of β in the first instance.

As pointed out by Peach and Littauer (1946), the parameters np_0 and p_1/p_0 specify families of Single-sample Acceptance Schemes when the Poisson approximations to the Operating Characteristic probabilities of the schemes are satisfactory. These parameters could have been used in the tabulation of the schemes in this paper, but for reasons of simplicity the present arrangement was preferred. However, the coverage of p_0 and p_1 in the tables for non-destructive inspection may readily be extended by using the parameters

$$M = np_1 \text{ and } R = p_1/p_0 \quad . \quad . \quad . \quad . \quad . \quad (25)$$

In terms of these parameters, Tables 1–11 give, for particular values of R and p_1, the boundary values of c_m/c_s corresponding to the schemes $(M/p_1, k)$. Any single line in the tables corresponds to a constant value of M since $M = np_1$ has been obtained from the solution of equation (24) for each value of k tabled. Lastly, it is seen that the inequalities (9) and (10) can be re-written in terms of M_{-1}, M_0, M_1 as

$$\frac{c_m}{c_s} \,.\, N[A(M_0/p_1, r) - A(M_{-1}/p_1, r-1)] \geqslant \frac{1}{p_1}\,[M_0 \,.\, A(M_{-1}/p_1, r-1) - M_{-1} \,.\, A(M_0/p_1, r)] \quad . \quad (26)$$

and

$$\frac{c_m}{c_s} \,.\, N[A(M_1/p_1, r+1) - A(M_0/p_1, r)] < \frac{1}{p_1}\,[M_1 \,.\, A(M_0/p_1, r) - M_0 \,.\, A(M_1/p_1, r+1)] \quad . \quad (27)$$

where

$$M_t = n_t p_1.$$

Hence for M_{-1}, M_0, M_1 constant, the boundary values are proportional to $1/p_1$, since

$$A(M_t/p_1, r + t)$$

is independent of p_1 for given R when the Poisson approximation holds.

In order to use a table with a particular value of R for an untabulated value of p_1 (or p_0), say p_1' (or p_0') where the true value of c_m/c_s is γ say, it is only necessary to enter the table with $\gamma \times p_1'/p_1$ (tabulated) or $\gamma \times p_0'/p_0$ (tabulated). The most economical scheme is obtained from that tabulated by multiplying $n = M/p_1$ by p_1 (tabulated)$/p_1'$ or by multiplying $n = M/Rp_0$ by p_0 (tabulated)$/p_0'$. For example, suppose that the most economical single-sample acceptance scheme is required, in the meaning of this paper, for non-destructive inspection where $N = 10{,}000$, $\beta = 0{\cdot}05$, $p_0 = 0{\cdot}2$ per cent., $p_1 = 1{\cdot}0$ per cent. (i.e. $R = p_1/p_0 = 5$) and $c_m/c_s = 2{\cdot}0$. The appropriate scheme may be obtained from Table 3 since, for this Table, R also equals 5. Here p_1 (tabulated) $= 5{\cdot}0$ per cent. and $p_1' = 1{\cdot}0$ per cent. Table 3 is therefore entered with

$$c_m/c_s = 2{\cdot}0 \times \frac{1{\cdot}0}{5{\cdot}0} = 0{\cdot}4$$

and the value of n obtained

$$\left(= \frac{M}{p_1}\right) \text{ is } n = 237.$$

Hence the value of n required in the example is $237 \times p_1$ (tabulated)$/p_1' = 237 \times 5 = 1{,}185$.

Text-table 4 gives the values of p_1/p_0 and p_0 for the non-destructive inspection schemes given in this paper.

TEXT-TABLE 4

Non-destructive Inspection: Tabled Values of $R = p_1/p_0$, and p_0

R	2	9/4	7/3	5/2	3	4	5
				Table			
$\beta = 0{\cdot}05$	5	7	6	4	1	2	3
$\beta = 0{\cdot}01$	—	—	—	11	8	9	10
p_0	3%	4%	3%	2%	1%	1%	1%

It should be pointed out that the values of c/c_m at the boundary values of c_m/c_s for constant values of R and M are independent of p_1 (or p_0). This follows from (22).

For destructive inspection, the inequality (9) becomes

$$\frac{c_m}{c_s} \,.\, N[A'(n_0, r) - A'(n_{-1}, r - 1)] \geqslant n_0A'(n_{-1}, r - 1) - n_{-1}A'(n_0, r) \quad . \quad . \quad (28)$$

where

$$A'(n_t, r + t) = (N - n_t) \,.\, A(n_t, r + t),$$

and similarly A' replaces A in (10). Hence for M_{-1}, M_0, M_1 and R constant, the boundary values are no longer directly proportional to $1/p_1$. However, if N is written as L/p_1 it is seen that the boundary values of c_m/c_s are now independent of p_1. Hence, tables for a given value of R and p_1 and lot size N may be used to specify schemes with the same value of R for an untabulated value of p_1 say, p_1' for lot sizes $N \times p_1/p_1'$ without any adjustment to the true value of c_m/c_s. The values of n and $A'(n, k)$ derived in this way require multiplication by p_1/p_1' before use. The transformation does not affect the values of c/c_m at the boundary values of c_m/c_s as can be seen from (23). The usefulness of the tables given in this paper, for specifying the most economical single-sample acceptance schemes when the risk points are other than those for which the schemes have been tabulated, is obviously more limited for destructive inspection than for non-destructive inspection, since for destructive inspection it is necessary to use a table for which the ratio of the tabulated lot size to the lot size of interest is the inverse of the ratio of the tabulated value of p_1 to the value of p_1, p_1' say, for which a scheme is required. For example, it is possible to deduce the most economical single-sample acceptance schemes for a lot size $N = 40{,}000$ and $p_1 = 3$ per cent., $R = 3$, since schemes are tabled for $N = 20{,}000$, $p_1 = 6$ per cent., $R = 3$. It is not possible to deduce schemes for $N = 5{,}000$, $p_1 = 3$ per cent., $R = 3$ since no schemes are tabled with $N/5{,}000$ equal to $3/p_1$.

Text-Table 5 gives the values of R, p_0 and the lot sizes for the non-destructive inspection schemes tabled in this paper.

TEXT-TABLE 5

Destructive Inspection: Tabled Values of $R = p_1/p_0$, and p_0

	Lot Size N = 10,000				*Lot Size N = 20,000*			
R . . .	2½	3	4	5	2½	3	4	5
	Table				*Table*			
$\beta = 0 \cdot 05$ . .	15	12, 16	13	14	25	22, 26	23	24
$\beta = 0 \cdot 01$ . .	20	17, 21	18	19	30	27, 31	28	29
p_0	2%	1%, 2%	1%	1%	2%	1%, 2%	1%	1%

Tables 22 and 27 could have been derived from Tables 16 and 21 respectively, but they were calculated directly by solving equation (24). The differences between the corresponding tables show the effect of rounding errors which are sometimes considerable but, so far as the specification of the most economical schemes is concerned, are not of great importance.

Finally, it should be remembered that transformation of the above kinds depend on the use of the Poisson approximation to the binomial distribution and should therefore not be used outside the ranges within which the approximation is satisfactory.

Tables 1–11, and to a lesser extent, Tables 12–31 may be used to examine the relationship of the most economical single-sample size to the lot size for non-destructive and destructive inspection respectively. The relationship will obviously depend on the value of c_m/c_s that applies in the problem under examination. In order for the Poisson approximation to apply with small lot sizes, it will be assumed that the schemes being investigated are those for which it is desired to accept only a small proportion β of lots if these lots were distributed binomially about the consumer's risk point.

Fig. 2 shows the relationship between the most economical single-sample size and lot size for various values of c_m/c_s for schemes with $p_0 = 1$ per cent. and $p_1 = 3$ per cent. defective, and schemes with $p_0 = 1$ per cent. and $p_1 = 5$ per cent. defective where inspection is non-destructive, and $\beta = 0 \cdot 05$.

It is seen that the graphs of n against $\log N$ are practically straight lines (except for the value $c_m/c_s = 0 \cdot 01$) whose slopes depend on the parameters. Any deviations from linearity can probably be mainly attributed to the discontinuous nature of n. Owing to the transformation property possessed by single-sample acceptance schemes, when derived by using the Poisson approximation discussed above, it appears that the relationship

$$n \propto (\log N) + \text{a constant} \quad . \quad . \quad . \quad . \quad . \quad (29)$$

holds for the most economical single-sample acceptance schemes where inspection is non-destructive over the greater part of the range of variation of the parameters. Of course, any variation of β with lot size has been neglected, whereas in practice it may be advisable to decrease β (or p_1) as the lot size increases.

Obviously, for product of homogeneous quality, it is more economical on average to use an Acceptance Sampling Scheme on large lots than on small when β (or p_1) is independent of lot size. This follows from (22) which, using (29) may be re-written as

$$c/c_m = \frac{1}{A(n, k)} + \frac{K \log N + K'}{N \, . \, A(n, k) \, . \, c_m/c_s} \quad . \quad . \quad . \quad . \quad (30)$$

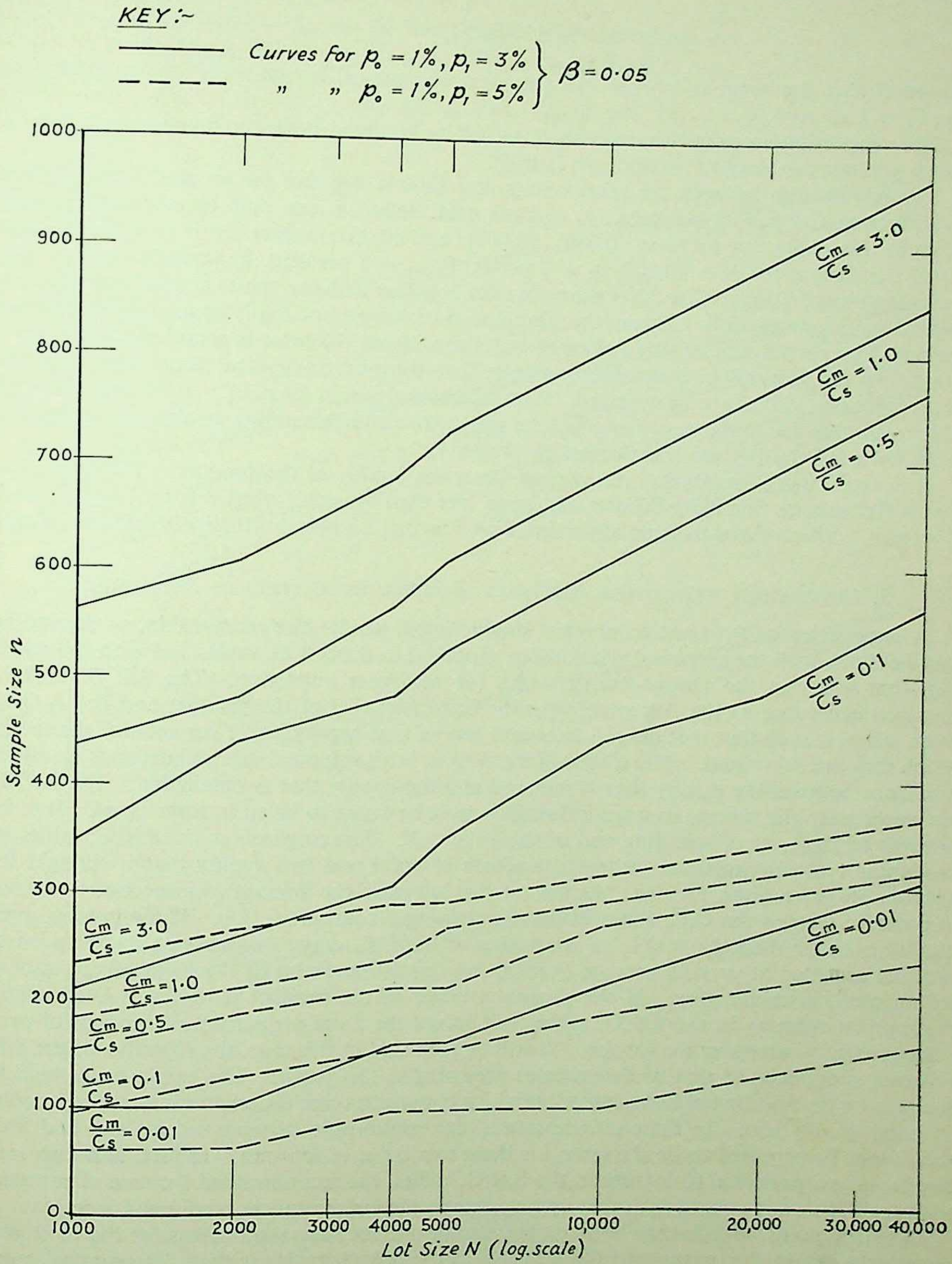

FIG. 2.—Single-sample size against Lot size for some most economical schemes: Non-destructive Inspection.

where K, K' are constants depending only on c_m/c_s, p_0, p_1 and β. Hence for any value of c_m/c_s (> 0), as N increases

$$c/c_m \to \frac{1}{A(n, k)} \to 1 \qquad (31)$$

(provided that the schemes chosen are the most economical in the sense of this paper) since $A(n, k) \to 1$ as $n \to \infty$.

The proportionate reduction in costs achieved by batching in larger homogeneous lots is of course greatest for product where c_m/c_s is least.

The relationship between the most economical sample size and the lot size, when inspection is destructive and $p_0 = 1$ per cent., $p_1 = 3$ per cent. defective, can only be examined, using the tables in this paper, for lot sizes 10,000; 20,000; and 40,000; values for $N = 40{,}000$ being obtained from those for $N = 20{,}000$, $p_0 = 2$ per cent., $p_1 = 6$ per cent. by transformation. Fig. 3 shows the plot of n against log N for these lot sizes, together with the values of n for non-destructive inspection for comparison. Again, the relationship between n and log N appears to be reasonably linear. For all but one of the values of c_m/c_s considered the most economical sample size for non-destructive inspection, is equal to or greater than the most economical sample size for destructive inspection. This is to be expected. The exceptional case is for $c_m/c_s = 0{\cdot}01$ and $N = 40{,}000$ and is probably due to the minor inaccuracies which arise when extending the destructive inspection tables by using the Poisson transformation property.

It is again more economical on average when the quality of the product is homogeneous to use an Acceptance Sampling Scheme on large lots than on small when β (or p_1) is independent of lot size. This follows from an argument similar to that for non-destructive inspection schemes.

5. COMPARISON WITH OTHER METHODS: POSSIBLE RELAXATION OF ASSUMPTIONS

A comparison of the most economical single-sample size/lot size relationship, as derived here, may be made with the empirical relationship proposed in the S.R.G. tables and with the relationship that holds in the Dodge-Romig tables for rectifying inspection. The S.R.G. tables are classified according to the Acceptable-quality Level (A.Q.L.) of the product and the A.Q.L. in those tables is such that it is desired to accept lots of that quality on 95 per cent. of the times on which they are submitted. The choice of the A.Q.L. is an empirical one and depends on effecting a balance between the quality that is required and the quality that is obtainable. The A.Q.L. is not necessarily the process average although it may be taken to be so in some cases. It is independent of the costs of sampling and of the lot size N. The emphasis in the S.R.G. tables is on the amount of discrimination between products of good and bad quality that is thought to be appropriate for a given lot size. As the lot size increases the amount of inspection is increased in order to steepen the O.C. curve whilst maintaining a constant A.Q.L. If the process average quality is better than the A.Q.L., a steepening of the O.C. curve *will* increase the proportion of product accepted at process average quality, but this is incidental to the main objective of providing more discrimination. If the process average of the product is at the A.Q.L., then the appropriate schemes in the S.R.G. tables will accept the same proportion of product of process average quality whatever the lot size. It will be seen that in this case, the objective of the S.R.G. schemes is opposite to that of the schemes presented in this paper. The emphasis in the S.R.G. tables is on the rejection of bad quality lots whilst in the latter case the emphasis is on the acceptance of good quality lots. In these circumstances, the relationship between sample size and lot size would not be expected to be the same for these two types of schemes. In fact, although sample size is not proportional to lot size in the S.R.G. tables, the recommended increase of sample size with lot size is more rapid than that appropriate in the circumstances considered here.

Another point of difference between the sample size/lot size relationships for the most economical schemes in this paper and the schemes in the S.R.G. tables is that, for practical reasons, the relationship in the S.R.G. tables has been chosen to be the same for all the various A.Q.L. classes considered, except that with A.Q.L.'s near zero, some of the smaller sample sizes cannot be used. It can be deduced from what has been said earlier, that the sample size/lot size relationship in this paper not only depends on the ratio p_1/p_0 (as Fig. 2 shows and as is to be expected) but also on the value of p_0.

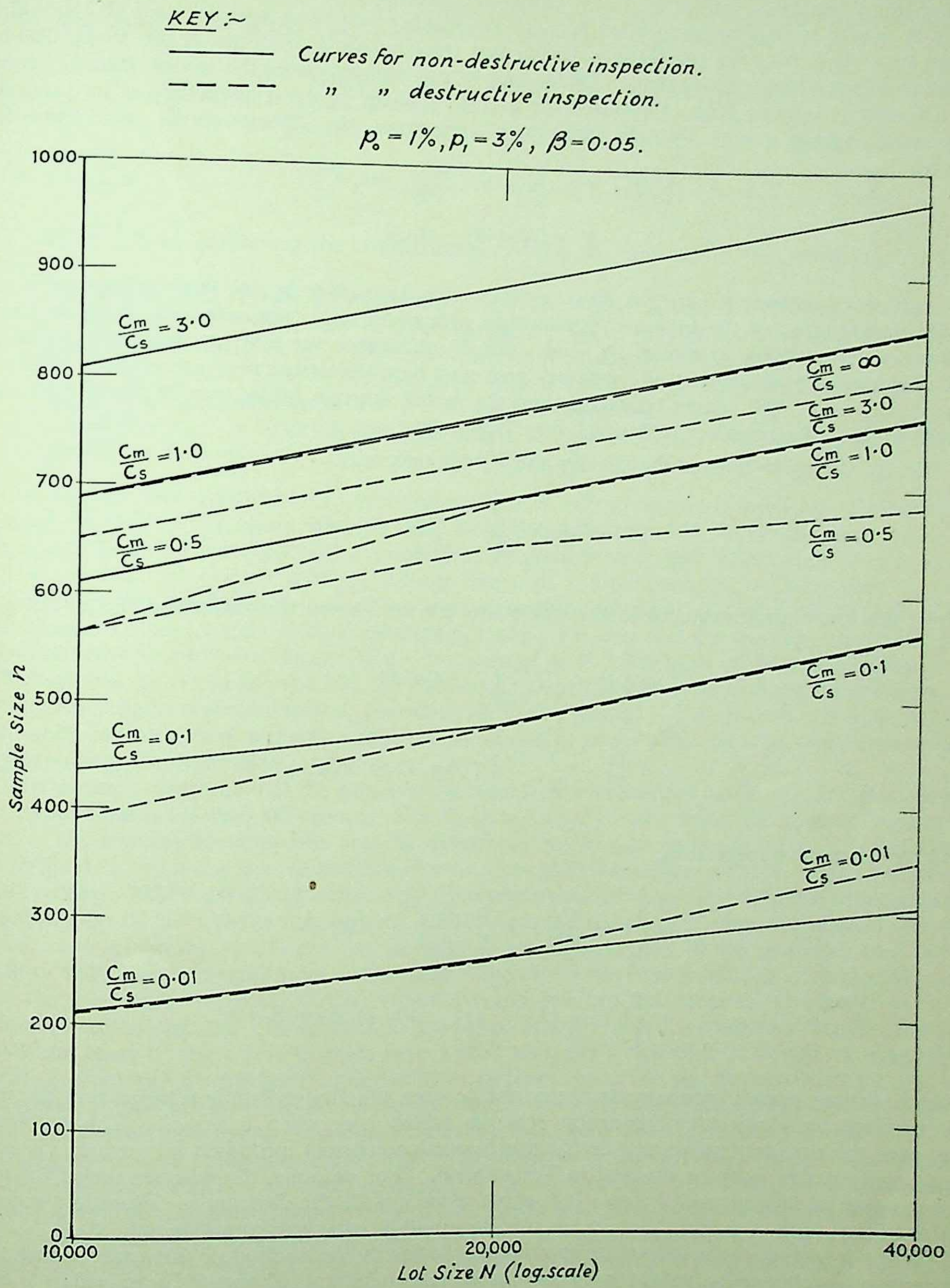

FIG. 3.—Single-sample size against Lot size for some most economical schemes: Destructive compared with Non-destructive Inspection.

Although the Dodge-Romig schemes are designed for rectifying inspection, the object of their tables based on stated values of "Lot Tolerance Percentage Defective" and fixed Consumer's Risks is similar to that recommended here for Acceptance Sampling Schemes, and an interesting connection exists between the sample size/lot size relationship appropriate in the Dodge-Romig tables and that which applies here. Dodge and Romig recommended the use of schemes which, for a fixed Consumer's Risk, minimized the average number of items inspected per lot for a lot of process average quality. In the notation of this paper, the schemes chosen were those for which

$$I = n + (N - n) \,.\, (1 - A(n, k)) \qquad (32)$$

$$= N - (N - n) \,.\, A(n, k) \qquad (33)$$

was minimized, subject to $A(p_1, n, k) = \beta$, where $A(n, k)$ is given by the Poisson approximation to the partial sums of the binomial distribution with mean np_0. The values of (n, k) obtained, obviously, (from (33)), maximize $(N - n) \,.\, A(n, k)$ and hence the schemes proposed by Dodge and Romig are equivalent to the schemes proposed here for destructive inspection in the limit when $c_m/c_s = \infty$. No direct check between the actual schemes tabled can be made, however, owing to the different values of β which have been used.

Finally, it may be noticed that Dodge and Romig maximize

$$N - I = N \,.\, \left(1 - \frac{n}{N}\right) .\, A(n, k) \qquad (34)$$

whilst the functions maximized in this paper are, for non-destructive inspection, from (22)

$$\frac{c_m}{c} = \left(1 + \frac{n}{N} \cdot \frac{c_s}{c_m}\right)^{-1} .\, A(n, k) \qquad (35)$$

$$= \left(1 - \frac{n}{N} \cdot \frac{c_s}{c_m}\right) .\, A(n, k) \text{ to order } n/N \qquad (36)$$

and for destructive inspection,

$$\frac{c_m}{c} = \left(1 - \frac{n}{N}\right)\left(1 + \frac{n}{N} \cdot \frac{c_s}{c_m}\right)^{-1} .\, A(n, k) \qquad (37)$$

$$= \left[1 - \frac{n}{N}\left(1 + \frac{c_s}{c_m}\right)\right] .\, A(n, k) \text{ to order } n/N \qquad (38)$$

Hence we may expect approximate agreement between the Dodge-Romig schemes and the most economical non-destructive inspection schemes tabled here when $c_m/c_s = 1$. The agreement between the non-destructive and destructive inspection schemes, proposed for various values of c_m/c_s and lot size, may be checked in Text-table 6. For example, the schemes tabled for non-destructive inspection and $c_m/c_s = 0 \cdot 5$ agree closely with the destructive inspection schemes tabled for $c_m/c_s = 1 \cdot 0$ and the same lot size, as would be expected from (36) and (38).

The advantages to be gained by specifying schemes by the method of this paper rather than by arbitrarily choosing a value for the producer's risk point and risk may be examined by a comparison of values of c/c_m for the two methods. Since for the schemes tabled in the Appendix, $A(n, k)$ is in fact the producer's risk at p_0, the costs for the two methods will be approximately the same for non-destructive inspection when the arbitrarily chosen producer's risk is in the neighbourhood of this value. However, a comparison of costs over the whole range of c_m/c_s is of interest and is given in Text-table 6 for values of c_m/c_s of $0 \cdot 01, 0 \cdot 1, 0 \cdot 5, 1 \cdot 0, 3 \cdot 0$ for non-destructive inspection and 0·01, 0·1, 0·5, 1·0 and ∞ for destructive inspection for $p_0 = 1$ per cent. and $p_1 = 3$, 4 and 5 per cent. defective with arbitrary producer's risks β of approximately 0·05 and 0·01. The sample sizes of the fixed producer's risk schemes are given below:

	Producer's Risk $\alpha \doteq 0\cdot05$			*Producer's Risk* $\alpha \doteq 0\cdot01$		
	$p_1=3\%$	$p_1=4\%$	$p_1=5\%$	$p_1=3\%$	$p_1=4\%$	$p_1=5\%$
$\beta = 0\cdot05$ . .	481	263	183	689	361	237
$\beta = 0\cdot01$ . .	761	400	262	1,019	537	348

For non-destructive inspection the comparisons may be summed up in the following working rules:

(1) For $c_m/c_s \leqslant 0\cdot01$, schemes with $\alpha \doteq 0\cdot05$ or $0\cdot01$ should not be used.

(2) For $0\cdot01 < c_m/c_s \leqslant 0\cdot10$ schemes with $\alpha \doteq 0\cdot05$ may be used, but with caution at the lower end of the range. Schemes with $\alpha \doteq 0\cdot01$ should not be used.

(3) For $0\cdot10 < c_m/c_s \leqslant 0\cdot50$ schemes with $\alpha \doteq 0\cdot05$ may be used, but at the upper end of the range, schemes with $\alpha \doteq 0\cdot01$ may be preferable.

(4) For $c_m/c_s \geqslant 0\cdot50$ schemes with $\alpha \doteq 0\cdot01$ may be used. Schemes with $\alpha \doteq 0\cdot05$ should not be used.

To transfer these conclusions to other lot sizes, it is only necessary to multiply the limits for c_m/c_s by $10{,}000/N$. The above working rules have also been found to apply reasonably well to the other tables for non-destructive inspection given here and also where $p_0 = 2$ per cent., $p_1 = 6$ per cent.; $p_0 = 4$ per cent., $p_1 = 8$ per cent.; $p_0 = 5$ per cent., $p_1 = 10$ per cent.

Although the comparisons for destructive inspection schemes in Text-table 6 cannot be generalized to other lot sizes without affecting p_0 and p_1 it is seen that the working rules given for non-destructive inspection roughly apply. The case of $N = 2{,}000$ has also been considered (not published) but, as would be expected, the rules break down if the sample size n for the fixed producer's risk scheme is greater than n_w for which $(N - n) \,.\, A(n, k)$ is a maximum. It seems likely that tables for destructive inspection schemes will prove of even more practical value than tables for non-destructive schemes.

It is perhaps surprising that the gain is not greater over much of the range of c_m/c_s when using the tabled schemes instead of schemes with fixed producer's risks, when the respective sample sizes of the schemes are compared and the differences are seen to be considerable. The answer lies in the division of the costs of a scheme between sampling-inspection and the rejection of good quality items. Although this division may be considerably different for different schemes, the ultimate costs of the schemes may be very similar since the cost for the most economical scheme is a shallow minimum. It is apparent, however, from the summary of the preceding cost comparisons that indiscriminate use of a particular value of the producer's risk for all values of c_m/c_s with the producer's risk point at the process average, can produce schemes which will be very uneconomical in practice. Tables such as the S.R.G. (1948) tables, the Military Standard 105A tables and those of Peach (1947) which have a fixed producer's risk of 0·05 should be used with caution. Certainly the producer's risk point, or in these cases, the Acceptable Quality Level as it is called, should be chosen to be greater than the process average when $c_m/c_s \geqslant 5{,}000/N$ if the quality of normal production is acceptable and the most economical scheme is desired. When this is so, however, it would appear preferable to fix the producer's risk point at the process average and to vary the producer's risk with c_m/c_s and N rather than vary the value of the Acceptable Quality Level and maintain a fixed producer's risk. However, if for various reasons it is necessary to limit the number of schemes which are tabled for any combination of producer's and consumer's risk points, it would be better to list schemes with producer's risks equal to 0·01 rather than 0·05, since the connection between such schemes and the most economical schemes when product of normal quality is acceptable is certainly closer than is the case with schemes having consumer's risks of 0·05, as in many problems c_m/c_s may be expected to be greater than $5{,}000/N$.

It follows from what has been said above that a scheme chosen to have a given producer's risk and sample size may often be replaced by a more economical scheme with a greater sample size, or sometimes even with a smaller size, which yields the same or even a smaller consumer's

TEXT-TABLE 6

Comparison of Costs of Most Economical Single-sample Schemes with Costs of Single-sample Schemes Chosen with Fixed Producer's Risks (α)

Process Average $p_0 = 1\%$ defective: Poisson approximation used throughout.

		Consumer's Risk $\beta = 0{\cdot}05$				*Consumer's Risk* $\beta = 0{\cdot}01$			
		Table Scheme		*Fixed Producer's Risk Schemes*		*Table Scheme*		*Fixed Producer's Risk Schemes*	
				$\alpha \doteqdot 0{\cdot}05$	$\alpha \doteqdot 0{\cdot}01$			$\alpha \doteqdot 0{\cdot}05$	$\alpha \doteqdot 0{\cdot}01$
p_1	c_m/c_s	n	c/c_m	c/c_m	c/c_m	n	c/c_m	c/c_m	c/c_m
		Non-destructive Inspection: Lot Size 10,000							
3%	0·01	210	4·8	6·2	8·0	437	7·4	9·0	11·3
	0·10	438	1·56	1·57	1·71	626	1·81	1·81	2·04
	0·50	607	1·15	1·16	1·15	848	1·20	1·21	1·21
	1·0	689	1·08	1·11	*1·08	934	1·11	1·13	1·11
	3·0	810	1·03	1·08	1·03	Not available from the tables in this paper			
4%	0·01	157	3·2	3·8	4·7	251	4·6	5·3	6·4
	0·10	263	1·33	*1·33	1·38	400	1·48	*1·48	1·55
	0·50	361	1·09	1·10	*1·09	504	1·12	1·14	1·12
	1·0	393	1·05	1·08	1·05	571	1·06	1·10	1·06
	3·0	486	1·02	1·06	1·02	636	1·02	1·07	1·03
5%	0·01	95	2·6	2·9	3·4	168	3·5	3·8	4·5
	0·10	183	1·23	*1·23	1·25	262	1·33	*1·33	1·36
	0·50	263	1·06	1·08	1·06	348	1·08	1·11	*1·08
	1·0	289	1·03	1·06	1·04	376	1·04	1·08	1·05
	3·0	314	1·01	1·05	1·02	430	1·02	1·06	1·02
		Destructive Inspection: Lot Size 10,000							
3%	0·01	210	4·9	6·5	8·6	387	7·7	9·8	12·6
	0·10	395	1·62	1·65	1·83	626	1·93	2·00	2·27
	0·50	565	1·22	1·22	1·24	761	1·31	*1·31	1·35
	1·0	565	1·15	1·17	1·16	848	1·22	1·22	1·24
	∞	689	1·09	1·11	*1·09	934	1·12	1·14	1·12
4%	0·01	157	3·3	3·9	4·8	251	4·8	5·5	6·8
	0·10	263	1·37	*1·37	1·43	364	1·53	1·54	1·64
	0·50	329	1·12	1·14	1·13	470	1·17	1·19	1·18
	1·0	361	1·09	1·11	*1·09	504	1·12	1·14	1·12
	∞	393	1·05	1·08	1·05	537	1·07	1·10	*1·07
5%	0·01	95	2·6	3·0	3·5	168	3·6	3·9	4·7
	0·10	183	1·25	*1·25	1·28	262	1·36	*1·36	1·41
	0·50	237	1·08	1·10	*1·08	320	1·12	1·14	1·12
	1·0	263	1·06	1·08	1·06	348	1·08	1·11	*1·08
	∞	289	1·03	1·06	1·04	376	1·04	1·08	1·05
		Destructive Inspection: Lot Size 20,000							
3%	0·01	258	3·1	3·7	4·7	437	4·5	5·2	6·5
	0·10	481	1·35	*1·35	1·41	716	1·50	1·51	1·60
	0·50	648	1·12	1·14	1·12	891	1·16	1·17	1·17
	1·0	689	1·08	1·11	*1·08	934	1·12	1·13	1·12
	∞	770	1·05	1·09	1·05	1,019	1·06	1·09	*1·06
4%	0·01	157	2·3	2·5	2·9	290	3·0	3·2	3·8
	0·10	296	1·20	1·21	1·22	435	1·29	1·29	1·32
	0·50	361	1·07	1·10	*1·07	551	1·09	1·12	*1·09
	1·0	393	1·05	1·08	1·05	537	1·07	1·10	*1·07
	∞	455	1·03	1·07	1·03	603	1·03	1·08	1·04
5%	0·01	126	1·9	2·0	2·2	201	2·4	2·5	2·8
	0·10	210	1·14	1·15	1·14	291	1·20	1·21	1·21
	0·50	263	1·05	1·07	1·05	376	1·06	1·10	1·06
	1·0	289	1·03	1·06	1·04	376	1·04	1·08	1·05
	∞	314	1·02	1·05	1·02	403	1·02	1·07	1·03

* Fixed Producer's Risk Scheme the same as the Most Economical Scheme.

risk than the initial scheme. Probably the factor of greatest importance in the design of Acceptance Sampling Schemes is a limitation on the sample size that may be taken. In other words the costs of sampling and inspection are not linear with n or are linear over only a small part of the possible range of n. When the costs of sampling inspection go up at intervals in steps with increase in n, the obvious solution for non-destructive inspection is to take the largest sample possible for a given level of costs. The problem of whether to proceed to another cost level can then be determined in the manner of this paper. The same treatment is possible with destructive inspection except that n must never be greater than n_w where $(N - n_w)\ A(n_w, k + w)$ is a maximum. When the costs of sampling inspection are linear but an upper limit is imposed on the value of n that may be taken, tables calculated in the manner of this paper apply, except that, at the maximum allowable value of n, the upper limit for c_m/c_s must be replaced by infinity and the corresponding value of c/c_m replaced by $1/A(n, k)$. If the limitation on n is low, compared with the protection that the scheme is expected to provide against accepting bad quality product, a smaller size than the maximum allowed will result in increased costs, unless c_m/c_s is small, at least for linear costs of sampling inspection.

Barnard (1954) has shown that the choice of the most economical Acceptance Sampling Scheme when cost functions are known is very closely dependent on the process curve. Although the method proposed here for the case when cost functions are indeterminable may be used with any process curve, the dependence of the choice of a scheme on a precise knowledge of the process curve is of considerable interest, and so in particular is the usefulness of tables for binomial lot distributions when the true process curve is not binomial. Tables for binomial lot distributions require only a knowledge of the process average, which completely specifies the process curve for a given lot size. The effect of using binomial lot distribution tables when the estimate of process average is correct but the process curve is not binomial, will now be examined.

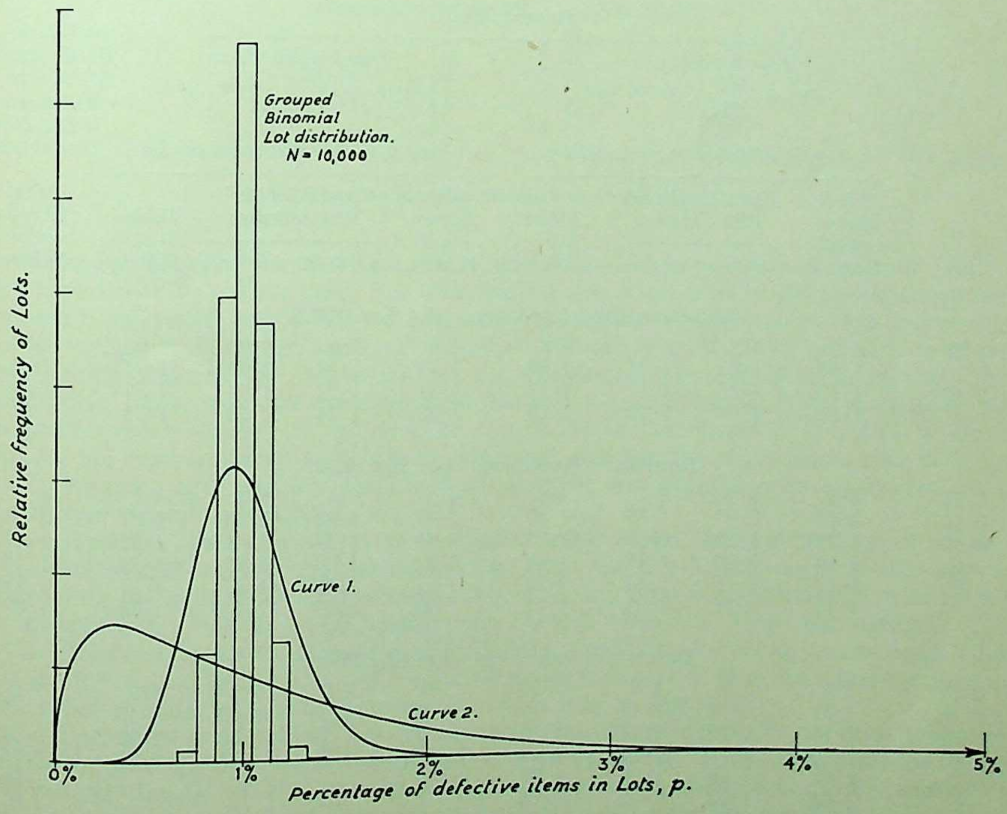

FIG. 4.—Two Alternative Process Curves.

The simplest type of process curve to choose for an investigation of this kind is the Pearson Type III curve since $A(n, k)$ may be obtained by interpolation in Incomplete Beta tables, provided that the Poisson approximation to $A(p, n, k)$ is satisfactory. Two Pearson Type III curves have been used, namely:

$$\text{Curve 1.}\quad y(p) = \frac{15^{15}}{\Gamma(15)} \cdot e^{-15p} \cdot p^{14} \quad . \quad . \quad . \quad . \quad . \qquad (39)$$

$$\text{Curve 2.}\quad y(p) = \frac{1 \cdot 5^{1.5}}{\Gamma(1 \cdot 5)} \cdot e^{-1.5p} \cdot p^{0.5} \quad . \quad . \quad . \quad . \qquad (40)$$

where p is the percentage of items defective per lot and $0 \leqslant p \leqslant \infty$. These curves are shown in Fig. 4. The process average p_0 for both curves is 1 per cent., the standard deviation for Curve (1) is $(1/15)^{\frac{1}{2}}$ per cent. and for Curve (2), $(10/15)^{\frac{1}{2}}$ per cent. The binomial lot distribution for $p_0 = 1$ per cent., $N = 10{,}000$ is shown for comparison. The standard deviation of the binomial distribution is $(0 \cdot 0099)^{\frac{1}{2}}$ per cent.

TEXT-TABLE 7 (1)

Comparison of Costs of Most Economical Single-sample Schemes when the Process Curve is a Type III Distribution, with Costs of Single-sample Schemes Chosen from Tables 1–31 or with Fixed Producer's Risks

$p_0 = 1\%$ defective.
$p_1 = 3\%$ defective.
Consumer's Risk $\beta = 0 \cdot 05$.

Values of c/c_m

c_m/c_s	*True Process Curve* $y(p) = \frac{15^{15}}{\Gamma(15)} \cdot e^{-15p} \cdot p^{14}$ *p = % of defective items per lot*				*True Process Curve* $y(p) = \frac{1 \cdot 5^{1.5}}{\Gamma(1 \cdot 5)} \cdot e^{-1.5p} \cdot p^{0.5}$ *p = % of defective items per lot*				*True Process Curve Binomial Lot Distribution*
	Most Economical Scheme	*Fixed Producer's Risk Schemes*		*Tables 1–31 Schemes*	*Most Economical Scheme*	*Fixed Producer's Risk Schemes*		*Tables 1–31 Schemes*	*Most Economical Scheme*
		$\alpha \doteqdot 05$	$\alpha \doteqdot 0 \cdot 01$			$\alpha \doteqdot 0 \cdot 05$	$\alpha \doteqdot 0 \cdot 01$		
	Non-Destructive Inspection. Lot Size 10,000								
0·01	4·8	6·3	8·1	*4·8	4·3	7·0	9·0	4·6	4·8
0·10	1·60	1·62	1·74	1·60	1·72	1·79	1·95	1·76	1·56
0·50	1·17	1·20	1·17	1·17	1·31	1·32	1·31	1·31	1·15
1·0	1·10	1·14	1·10	1·10	1·23	1·26	1·23	1·23	1·08
	Destructive Inspection. Lot Size 10,000								
0·01	4·8	6·7	8·7	4·9	4·3	7·4	9·8	4·7	4·9
0·10	1·67	1·70	1·87	*1·67	1·78	1·88	2·09	1·81	1·62
0·50	1·25	1·26	1·26	*1·25	1·39	1·39	1·41	1·39	1·22
1·0	1·18	1·20	1·19	1·18	1·32	1·33	1·32	1·32	1·15
3·0	1·13	1·16	1·13	1·13	1·27	1·29	1·27	1·27	1·11
∞	1·11	1·15	1·11	1·11	1·23	1·27	1·24	1·24	1·09
	Destructive Inspection. Lot Size 20,000								
0·01	3·2	3·8	4·8	3·2	3·0	4·2	5·3	3·2	3·1
0·10	1·39	1·39	1·44	1·39	1·52	1·53	1·60	1·53	1·35
0·50	1·14	1·17	1·14	1·14	1·28	1·29	1·28	1·28	1·12
1·0	1·10	1·14	1·11	1·11	1·23	1·27	1·23	1·23	1·08
3·0	1·08	1·13	1·09	1·08	—	—	—	—	1·06
∞	1·06	1·12	1·07	1·06	—	—	—	—	1·05

* Schemes the same as the most economical schemes.

TEXT-TABLE 7 (2)

Comparison of Costs of Most Economical Single-sample Schemes when the Process Curve is a Type III Distribution, with Costs of Single-sample Schemes Chosen from Table 1–31 or with Fixed Producer's Risks

$p_0 = 1\%$ defective.
$p_1 = 5\%$ defective.
Consumer's Risk $\beta = 0{\cdot}05$

True Process Curve: $y(p) = \dfrac{1{\cdot}5^{1.5}}{\Gamma(1{\cdot}5)} \cdot e^{-1.5p} \cdot p^{0.5}$

$p = \%$ *of defective items per lot.*

Values of c/c_m

c_m/c_s	*Most Economical Scheme*	*Fixed Producer's Risk Schemes* $\alpha \doteqdot 0{\cdot}05$	*Fixed Producer's Risk Schemes* $\alpha \doteqdot 0{\cdot}01$	*Tables* 1–31 *Schemes*	*Most Economical Scheme: Binomial Lot Distribution*
		Non-destructive Inspection: Lot Size 10,000			
0·01	2·57	3·14	3·62	*2·57	2·59
0·10	1·31	*1·31	1·33	*1·31	1·23
0·50	1·11	1·15	1·13	1·12	1·06
1·0	1·08	1·13	1·10	1·09	1·03
3·0	1·04	1·12	1·08	1·06	1·01
		Destructive Inspection: Lot Size 10,000			
0·01	2·60	3·20	3·71	*2·60	2·61
0·10	1·34	*1·34	1·36	*1·34	1·25
0·50	1·15	1·17	1·15	1·15	1·08
1·0	1·12	1·15	1·13	1·12	1·06
3·0	1·09	1·14	1·11	1·10	1·04
∞	1·08	1·13	1·10	1·09	1·03

* Schemes the same as the most economical schemes.

Tables for specifying the most economical Single-sample Acceptance Schemes were prepared for Curve 1 with $p_1 = 3$ per cent., $\beta = 0{\cdot}05$ and lot size N equal to 10,000 for non-destructive inspection and 10,000 and 20,000 for destructive inspection. Similar tables were prepared for Curve 2 with $p_1 =$ 3 per cent. and 5 per cent., $\beta = 0{\cdot}05$ and $N = 10{,}000$ for both non-destructive and destructive inspection. It was assumed that the Poisson approximation to $A(p, n, k)$ was satisfactory. These tables are not reproduced, but they were used to specify the most economical schemes for the two curves for the values of c_m/c_s shown in Text-Tables 7 (1) and (2) where the costs for the most economical schemes are compared with the costs for schemes with fixed Producer's Risks (α) approximately equal to 0·05 and 0·01 and with the costs of schemes chosen from the corresponding tables in Tables 1–31.

A comparison of the costs of the different schemes shows that, over all the range of values of c_m/c_s considered, the costs of the schemes obtained from Tables 1–31 are very little different from the costs for the most economical schemes. For $c_m/c_s \geqslant 0{\cdot}50$ it is satisfactory in most cases to use schemes with $\alpha \doteqdot 0{\cdot}01$ except possibly with Curve 2 when $p_1 = 5$ per cent. defective (Table 7 (2)). However, in all the comparisons considered, the costs of schemes chosen from Tables 1–31 are lower than, or equal to, the costs for schemes with $\alpha \doteqdot 0{\cdot}05$ or 0·01 and are generally much lower at values of c_m/c_s of the order of 0·01, as is to be expected.

The final columns of Tables 7 (1) and (2) show the costs predicted for the most economical schemes when a binomial lot distribution and "hypergeometric" sampling is assumed. These costs are less than the actual costs incurred when the true process curve is Curve 1 (except in one instance where $c_m/c_s = 0{\cdot}01$) and generally much less than when the true process curve is Curve 2 except again where $c_m/c_s = 0{\cdot}01$).

Summing up the comparisons in this section, it may be said that:

(i) For the departures from binomial lot distributions considered here (which in the case of Curve 2 is considerable), it is quite satisfactory to use schemes chosen from tables in which a binomial lot distribution and hypergeometric sampling is assumed.

(ii) When the process curve is fairly symmetrical (as with Curve 1) schemes with constant producer's risks may be used for $c_m/c_s > 0 \cdot 01$ as suggested in the previous working rules.

(iii) When the process curve is skew, it is safer to use the binomial lot distribution tables although the actual costs of the schemes may be considerably underestimated.

(iv) If the true process curve is known, it is of course preferable to use that knowledge when choosing a scheme, but it is probably unnecessary to do so unless c_m/c_s and c_m are both high.

It should be remarked that, for $p_0 = 1$ per cent. defective and process curve Curve 2, the Consumer's Risk Point would be expected to be further away than 3 per cent. defective, for the application of the method of costing employed in this paper to be correct.

6. DOUBLE-SAMPLING SCHEMES

This paper proposes specification criteria for Acceptance Sampling Schemes when these schemes are required to have a constant low probability of accepting any lots which may be offered at the Consumer's Risk Point. The operation of these criteria has so far only been detailed in connection with Single-sample Schemes and tables of these schemes have been given. The application of the criteria to Double-Sampling Schemes will now be considered briefly, where the double-sampling procedures are of the following type: take a first sample of n', accept the lot if there are d_1 or less defectives, reject if there are more than d_2; if no decision, take a second sample of n'', and accept the lot if there are d_3 or less defectives in the combined samples, otherwise reject.

For a given Consumer's Risk Point and Risk, only the rejection number, k, need be specified for a Single-sample Scheme, for the sample size n that must be taken to give the required protection is then completely determined. Double-sampling Schemes require three "decision" parameters d_1, d_2, d_3, however, and for any set of these, there is always more than one scheme having a given constant risk of acceptance at the consumer's Risk Point, since either n' or n'', the first and second sample-sizes, remains at choice. The solution to the general problem of specifying the "best" double-sampling scheme having an O.C. curve equivalent to that of a given single-sample scheme is not yet published, although Hamaker and van Strik (1955), in a preliminary paper, indicate that a solution will soon be available. Meanwhile, an indication may be given here of a rather pedestrian way of constructing tables of the most economical double-sampling schemes, in the sense of this paper.

A set of decision parameters is first chosen and a number of schemes (with varying values of n' and n'') are then obtained by trial and error methods such that each has the same Consumer's Risk at the same Consumer's Risk Point. Let the three decision parameters chosen, d_1, d_2, d_3, be represented by d and let the scheme accept a proportion $A(n', n'', d, p)$ of lots inspected containing p per cent. defective.

$$\int_0^1 A(n', n'', d, p) \,.\, y(p) \,.\, dp = A(n', n'', d)$$

say, is then calculated and the size, n, of the single-sample scheme having this value for $A(n, k)$ is obtained by interpolation in tables for single-sample schemes. The average sample-size $\bar{n}$ for the double-sampling scheme, averaged over the $y(p)$ distribution, is also obtained. The double-sampling scheme having the least "Relative Amount of Inspection" or least "Inverse Efficiency" (Hamaker, 1950), i.e. that scheme for which $\bar{n}/n$ is least, is then selected. Similarly other double-sampling schemes are chosen for other sets of the parameters d. These are then arranged in ascending order of $\bar{n}$. Any schemes having $A(n', n'', d)$ less than that of a preceding scheme in the $\bar{n}$ ranking are removed, for this indicates that the scheme is less efficient than a scheme with an alternative set of parameters. The schemes remaining in the joint $\bar{n}$ and $A(n', n'', d)$ ranking form the basic source from which the most economical schemes are derived. However, not all of these schemes are required unless the double-sampling equivalent to the single-sample condition (16) is satisfied immediately. Usually it is necessary to discard some of the schemes until the fractions

$$\frac{A(\bar{n}_t, d) - A(\bar{n}_{t-1}, d')}{\bar{n}_t - \bar{n}_{t-1}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (41)$$

decrease monotonically with t, where t is the rank order of the scheme. The theoretical device of fractional sample sizes might overcome this necessity, but this has not been investigated. The selection of schemes which have minimum inverse efficiency is an arbitrary one and is equivalent to assuming that within the range of values of $A(n', n'', d)$ that are possible with a given set of parameters d, the single-sample schemes having $A(n, k)$ equal to these values of $A(n', n'', d)$ satisfy the requirement that the ratio of the increase of $A(n, k)$ to the increase of n between adjacent schemes is a monotonically decreasing function, for this choice of double-sampling scheme to be a satisfactory one. It has been found in practice that this method of selection is satisfactory since it has been necessary to discard all other double-sampling schemes, and even some of those with minimum inverse efficiency as stated above, before (41) has been satisfied.

Text-table 8 shows the start of two typical double-sampling tables. It is assumed that the first sample is completely inspected, and in the non-curtailed schemes, the second also. In the curtailed schemes, sampling stops in the second sample as soon as the lot can be rejected but not as soon as it can be accepted. For the purposes of this paper, it is assumed that sampling and inspection are sequential in the curtailed schemes in the second sample. The schemes have been evaluated using the Poisson limit, except that the average second-sample size in the curtailed schemes has been derived using binomial probabilities. Binomial sampling from a point distribution is assumed.

TEXT-TABLE 8

Specimen Table of Most Economical Double-sampling Schemes

Non-Destructive Inspection: Nominal Lot Size 10,000.
Process Average $p_0 = 1\%$ defective.
Consumer's Risk Point $p_1 = 3\%$ defective.
Consumer's Risk $\beta = 0 \cdot 05$.

c_m/c_s	n'	n''	d_1	d_2	d_3	$\bar{n}$	$A(n', n'', d)$	*Inverse Efficiency*
				Non-curtailed Inspection				
0 –0·0017	120	49	0	1	1	137·7	0·523	0·890
0·0017–0·0097	120	93	0	1	2	153·6	0·576	0·869
0·0097–0·0206	140	121	0	2	3	211·0	0·709	0·876
0·0206–	130	183	0	2	4	237·0	0·753	0·888
				Curtailed Inspection				
0 –0·0037	114	62	0	1	1	130·9	0·516	0·861
0·0037–0·0048	120	93	0	1	2	150·6	0·576	0·852
0·0048–0·0115	127	95	0	2	2	171·6	0·637	0·842
0·0115–0·0236	130	138	0	2	3	203·1	0·707	0·847
0·0236–	130	183	0	2	4	231·4	0·753	0·866

Note.—The schemes in the above table must be treated as provisional since the complete range of possible schemes has not been investigated. There is, of course, an upper limit to c_m/c_s, at present undetermined, for which the last schemes for non-curtailed and curtailed inspection given above will be the most economical, if they are in fact included in a final table.

Text-table 8 may be used to illustrate the average reduction in costs obtained by using double-sampling schemes instead of single-sample schemes when $c_m/c_s = 0 \cdot 01$.

Table 1 shows the appropriate single-sample scheme to be (210, 3) with $A(n, k) = 0 \cdot 650$. The effective cost ratio of an accepted item under this scheme, given in Text-table 6, is 4·8. This compares with a figure of 4·4 for the non-curtailed double-sampling scheme (140, 121; 0, 2, 3) and 4·3 for the curtailed double-sampling scheme (127, 95; 0, 2, 2). No tables are available yet for most economical double-sampling schemes for higher values of c_m/c_s than those above, but the reduction in average costs that may be obtained by using double-sampling schemes instead of single-sample schemes may be examined briefly as follows.

From equation (22), the proportion of the effective cost ratio, c/c_m, contributed by the costs of physical sampling and inspection, as against the initial cost of manufacture or purchase is

$$\frac{n}{N \cdot \frac{c_m}{c_s}} : 1 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (42)$$

The values of this ratio for the most economical single-sample schemes in Tables 1–31, for various values of c_m/c_s may be obtained from Text-table 6. For example, in Tables 1 and 8 where the sampling costs are amongst the heaviest, and Table 3 where the sampling costs are least, the values of (42) together with the corresponding values of $A(n, k)$ and c/c_m for the most economical schemes for various values of c_m/c_s are given in Text-table 9.

TEXT-TABLE 9

Proportion of c/c_m Contributed by Physical Sampling Costs as Against Costs of Manufacture or Purchase for some Most Economical Single-sample Schemes

Table	c_m/c_s:	0·01	0·10	0·50	1·0	3·0
1	Proportion	2·1 to 1	0·44 to 1	0·12 to 1	0·07 to 1	0·03 to 1
	$A(n, k)$	0·650	0·923	0·978	0·989	0·996
	c/c_m	4·8	1·56	1·15	1·08	1·03
8	Proportion	4·4 to 1	0·63 to 1	0·17 to 1	0·09 to 1	Not available
	$A(n, k)$	0·725	0·897	0·973	0·985	,, ,,
	c/c_m	7·4	1·81	1·20	1·11	,, ,,
3	Proportion	0·95 to 1	0·18 to 1	0·05 to 1	0·03 to 1	0·01 to 1
	$A(n, k)$	0·754	0·961	0·994	0·997	0·998
	c/c_m	2·6	1·23	1·06	1·03	1·01

Obviously the greatest benefits from double-sampling schemes in terms of a reduction in the effective cost ratio, by reducing the average sample-size, will be obtained when $c_m/c_s \leqslant 1$, for the contribution of physical sampling costs to c/c_m in the above examples when $c_m/c_s > 1$ is proportionately small. Assuming an inverse efficiency of 0·88 (corresponding to values in Text-table 8) for the most economical non-curtailed double-sampling scheme in conditions corresponding to those of Table 8, when $c_m/c_s = 0{\cdot}01$, the sampling costs may be expected to be reduced from $(4{\cdot}4/5{\cdot}4) \times 7{\cdot}4 = 6{\cdot}0$ to $0{\cdot}88 \times 6 = 5{\cdot}3$. Hence the non-curtailed, double-sampling value for c/c_m might be expected to be about $7{\cdot}4 - 6{\cdot}0 + 5{\cdot}3 = 6{\cdot}7$, although this may be affected by some difference between the values of $A(n, k)$ and $A(n', n'', d)$ for the two schemes. For a curtailed scheme with inverse efficiency of 0·85, say, the value of c/c_m might be expected to be about 6·5. Hamaker (1950) has shown that with suitably chosen non-curtailed double-sampling schemes and the point binomial distribution, the inverse efficiency at values of $A(n', n'', d)$ of 0·90 and 0·98 may be as little as 0·76 and 0·60 respectively. Assuming that a most economical double-sampling scheme can be found for the conditions of Table 8 when $c_m/c_s = 0{\cdot}10$ with inverse efficiency of 0·76, the corresponding value of c/c_m might be expected to be about 1·64 instead of the single-sample value of 1·81. For the conditions of Table 1 when $c_m/c_s = 0{\cdot}50$, assuming an inverse efficiency of 0·60, the corresponding value of c/c_m for the most economical double-sampling scheme might be expected to be about 1·10 as against 1·15 for the single-sample scheme. When c_m/c_s is greater than 1, for the cases considered in this paper, the difference between c/c_m and unity for non-destructive inspection is almost entirely due to the physical sampling and inspection costs and hence the effect of the saving in average sample size in adopting double-sampling schemes, can be readily appreciated. With $c_m/c_s > 1$, the value of $A(n, k)$ for the most economical single-sample schemes tabled in this paper, for the ranges of parameters considered, is generally greater than 0·99, and Hamaker has shown that in this region, double-sampling schemes exist which require on average less than half the equivalent single-sample size. It therefore appears that, other things being equal, it would be advantageous to use double-sampling schemes at all values of c_m/c_s, provided that schemes can be obtained with low inverse efficiencies in the range of percentage defective, p, of normal production.

No attention has been paid in the above brief analysis to the inverse efficiency of double-sampling schemes at values of p outside the range of normal production. A reasonable requirement for any double-sampling scheme recommended may be that the inverse efficiency of the scheme should nowhere be greater than unity. This may rule out some sets of parameters d entirely *ab initio* and may vitiate the system of choice within an admissible parameter set explained earlier in this section. Such considerations must be left until a fuller analysis is prepared.

7. GENERAL REMARKS

Little has been said in this paper so far on the factors governing the choice of the Consumer's Risk and Risk Point, and in fact, in the writer's opinion, little can be said except in specific cases. The main factor in the choice in the circumstances considered here is probably the cost of accepting items of normal quality rather than the necessity for rejecting bad quality product. This cost of acceptance must perhaps be kept below a certain figure, in which case the maximum protection for that cost may be required. The obvious solution is to compare the costs of various schemes and choose the most discriminating scheme that can be afforded. Alternatively, if bad quality product is not expected very often, one of the less expensive schemes may suffice.

It has been mentioned that it may be advisable in practice to decrease β or p_1 or both with increase in lot size. It has been shown that costs (i.e. the effective cost ratios, c/c_m, when production is normal) are reduced if β and p_1 are kept constant when the lot size is increased and a binomial lot distribution is maintained. This reduction in costs may be the object in increasing the lot size. Alternatively, the same costs may be maintained when the lot size is increased, and the potential economic gain in increasing the lot size may be used, instead, to increase the protection afforded against accepting lots of bad quality, or, of course, the gain may be split between the two objectives. Either of these procedures involves decreasing β or p_1 or both. It should be mentioned here that special precautions need to be taken when small lots are grouped together to form large lots, in order to check initially that the small lots come from the same binomial lot distribution. The problem of grouping lots together when the basic normal production lot distribution is not binomial is one that would repay more detailed investigation, especially where, as in many practical cases, correlation between neighbouring lots is present. Another factor that may come into play is a possible variation of c_m with lot size. Again each case must be considered on its merits.

Obviously some data will accumulate on the relative frequency of occurrence of lots of poor quality and on the distribution of the percentage of defectives that they contain, after schemes of the type proposed here have been in operation for some time. If no poor quality lots are submitted to the schemes, and hence no poor lots are ever rejected by the schemes, there is no gain, for example, in terms of saving life, winning battles, or retaining the customer's good-will, due to the scheme, to set against the costs of operating it. In such a situation, the cost of an Acceptance Sampling Scheme will be a high price to pay for unnecessary reassurance. Schemes of the nature proposed above are meant to apply where it is fairly certain *a priori* that some bad lots will be submitted at some time.

It may be noted that the same schemes or very similar schemes occur in different tables for different ranges of c_m/c_s. For example, the scheme (263, 6) occurs in Table 2 and the scheme (262, 6) occurs in Table 10 for ranges of 0·077 to 0·13 and 0·054 to 0·10 respectively, where p_o is the same in both cases. The question may be posed whether any given scheme can be the most economical for some value of c_m/c_s provided that p_1 and β are chosen suitably. This is probably true within certain bounds, but is of course no reason for ignoring the proper determination of a consumer's risk and risk point before selecting a scheme. Tables 1–31 may be used for various investigations other than those made in this paper. For example, the effect of an inexact estimate of the process average when specifying a scheme, or a shift in the average after a scheme has been specified, may be investigated. It has been found in some instances that the actual cost at the true value of the process average, in terms of c/c_m, of a scheme which is the most economical at some other value, is not so very much more than the cost of the scheme which is the most economical at the true value, provided that the error in estimating the process average is fairly small. However, the expected costs of the schemes depend closely on a knowledge of the process average, and hence the estimate of the average should be the most accurate obtainable.

The criteria that have been recommended in this paper for specifying the most economical Acceptance Sampling Schemes are similar to a criterion proposed by Hamaker (1951, equation 11) as likely to be useful in the case when the cost of accepting a defective item is known. This criterion required that the average cost per accepted effective item should be minimized, where this average cost was equal to the sum of the costs of production and of inspection and of the damage due to accepted defective items, divided by the number of accepted effective items per lot. In this paper we have dealt with the case where the costs of accepting defective items cannot be evaluated and, in order to provide a solution to the specification problem, it has been assumed that the costs of accepting the number of defective items contained in lots of normal production are acceptable. However, the basis of measuring costs has been per accepted item, whether effective or defective. In the writer's opinion this has much to commend itself. Firstly, accepted items cannot be classified as being effective or defective without further inspection and hence to base costs on the percentage of effective items that are accepted seems unreal. In fact, to be told for a particular scheme that the average cost per accepted effective item has been minimized, leads one to ask about the average cost per accepted defective item and to wonder whether the total operational running costs of the scheme could not be reduced if slightly more defectives were to be accepted. Secondly, the costs per accepted effective item will vary widely according to the quality of the product submitted. The standpoint of this paper is that once production has settled down, there is little of a minor nature that the manufacturer can do to improve on normal quality and the only course left to a consumer to obtain product of improved quality is to buy from another source where product of a higher quality is obtainable. Hence it is unnecessary and perhaps even misleading to use a cost criterion which is closely dependent on the quality-level of the product. However, the effect of using a cost criterion, in the circumstances envisaged in this paper, when the cost basis *is* the number of effective items accepted, may be briefly examined. Since it is desired to accept as many lots of normal production as possible, the value of rejecting any of these lots is zero and apart from any salvage value, rejected lots of normal production represent a total loss. Items in accepted lots comprise defective and effective items. Again, the value of accepted defective items may be put equal to zero, for these are worthless. (It is assumed that for normal production the need for the product cancels out any positive costs caused by the acceptance of these defective items.) Hence the cost criterion (1) for non-destructive inspection is modified to

$$c = \frac{Nc_m + \bar{n}_s c_s}{N \,.\, \int_0^1 (1 - p)A(p)y(p)dp} \quad . \quad . \quad . \quad . \quad . \quad . \quad (43)$$

with a similar adjustment to the criterion for destructive inspection. For the point binomial distribution, and the single-sample scheme (n, k), the denominator of (43) reduces to

$$N \times (1 - p_0) \,.\, A(n, k) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (44)$$

Hence from (6), the bounds in terms of c_m/c_s for the most economical schemes with the cost criterion (43) will be the same as those for the criterion (1). This will also be true of destructive-inspection schemes. Hence the bounds for c_m/c_s for the single-sample schemes tabled in Tables 1–31 will be the same for the point binomial whichever form of the denominator is used in the cost criterion. However, the values of c/c_m at these boundaries will differ, and those in the tables must be divided by $(1 - p_0)$ before they apply to the cost criterion (43) or its destructive inspection equivalent. For hypergeometric sampling from a binomial distribution, the boundaries for c_m/c_s will be slightly different when the criterion (43) is used.

It is hoped that this paper will stimulate further research into what is still an incompletely explored subject. The author wishes to express his thanks to Dr. N. L. Johnson of University College, London, for his interest, advice and encouragement, also to Dr. H. C. Hamaker for providing copies of his papers, and to the Ordnance Board, Ministry of Supply, for providing various facilities.

SYNOPSIS

This paper deals with the problem of specifying Acceptance Sampling Schemes when the quality of normal production is acceptable and it is desired to operate schemes having a high probability of rejecting any lots which may be offered at worse than normal quality. It is assumed that the normal production Process Curve is known but that the proportion and distribution of lots which will be submitted at worse than normal production quality is unknown. In addition, the costs of accepting lots of worse than normal quality cannot be evaluated in the same units as the other costs of operating the schemes, or, are simply not known. The specification criterion proposed is that, for a recommended scheme, the cost of a lot plus the average cost per lot of the sampling scheme, when apportioned equally between the average number of items accepted per lot, for product of normal quality—termed the effective cost of accepted items of normal production—should be a minimum. The properties of Single-sample Acceptance Schemes which satisfy this specification criterion are examined for linear costs of manufacture (or purchase), sampling and inspection (which may be non-destructive or destructive). Tables of the schemes are provided for binomial lot distributions and a range of parameters. The application of the proposed specification criterion to Double-Sampling Schemes is also briefly considered.

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APPENDIX

TABLE 1

Non-destructive Inspection: Nominal Lot Size 10,000

Process Average $p_0 = 1\%$ defective.
Consumer's Risk Point $p_1 = 3\%$ defective.
Consumer's risk $= 0·05$.

	Scheme			
c_m/c_s	n	k	$A(n, k)$	c/c_m
0–0·003	100	1	0·368	∞–12
0·003–0·008	158	2	0·531	12–5·8
0·008–0·013	210	3	0·650	5·8–4·0
0·013–0·027	258	4	0·740	4·0–2·7
0·027–0·041	305	5	0·807	2·7–2·2
0·041–0·070	350	6	0·858	2·2–1·75
0·070–0·095	395	7	0·894	1·75–1·58
0·095–0·15	438	8	0·923	1·58–1·40
0·15–0·22	481	9	0·944	1·40–1·29
0·22–0·29	524	10	0·959	1·29–1·23
0·29–0·45	565	11	0·970	1·23–1·16
0·45–0·60	607	12	0·978	1·16–1·13
0·60–0·86	648	13	0·984	1·13–1·09
0·86–1·2	689	14	0·989	1·09–1·07
1·2–1·6	730	15	0·992	1·07–1·05
1·6–2·3	770	16	0·994	1·05–1·04
2·3–3·3	810	17	0·996	1·04–1·03
3·3–4·6	850	18	0·997	1·03–1·022
4·6–6·1	890	19	0·998	1·022–1·017

TABLE 2

Non-destructive Inspection: Nominal Lot Size 10,000

Process Average $p_0 = 1\%$ defective.
Consumer's Risk Point $p_1 = 4\%$ defective.
Consumer's Risk $= 0·05$

	Scheme			
c_m/c_s	n	k	$A(n, k)$	c/c_m
0–0·003	75	1	0·472	∞–7·1
0·003–0·008	119	2	0·666	7·1–3·6
0·008–0·022	157	3	0·791	3·6–2·2
0·022–0·042	194	4	0·868	2·2–1·69
0·042–0·077	229	5	0·917	1·69–1·42
0·077–0·13	263	6	0·949	1·42–1·26
0·13–0·23	296	7	0·968	1·26–1·17
0·23–0·38	329	8	0·981	1·17–1·11
0·38–0·65	361	9	0·988	1·11–1·07
0·65–1·0	393	10	0·993	1·07–1·05
1·0–1·7	424	11	0·996	1·05–1·03
1·7–2·9	455	12	0·997	1·03–1·02
2·9–4·9	486	13	0·998	1·02–1·01

TABLE 3

Non-destructive Inspection: Nominal Lot Size 10,000

Process Average $p_0 = 1\%$ defective.
Consumer's Risk Point $p_1 = 5\%$ defective.
Consumer's Risk $= 0{\cdot}05$.

c_m/c_s	*Scheme*		$A(n, k)$	c/c_m
	n	k		
0–0·003	60	1	0·549	∞–5·1
0·003–0·011	95	2	0·754	5·1–2·4
0·011–0·028	126	3	0·866	2·4–1·67
0·028–0·062	155	4	0·928	1·67–1·35
0·062–0·12	183	5	0·961	1·35–1·19
0·12–0·26	210	6	0·980	1·19–1·10
0·26–0·47	237	7	0·989	1·10–1·06
0·47–0·93	263	8	0·994	1·06–1·03
0·93–1·7	289	9	0·997	1·03–1·02
1·7–3·3	314	10	0·998	1·02–1·01

TABLE 4

Non-destructive Inspection: Nominal Lot Size 10,000

Process Average $p_0 = 2\%$ defective.
Consumer's Risk Point $p_1 = 5\%$ defective.
Consumer's Risk $= 0{\cdot}05$.

c_m/c_s	*Scheme*		$A(n, k)$	c/c_m
	n	k		
0–0·002	60	1	0·301	∞–14
0·002–0·003	95	2	0·434	14–8·9
0·003–0·006	126	3	0·539	8·9–6·1
0·006–0·009	155	4	0·625	6·1–4·2
0·009–0·014	183	5	0·695	4·2–3·3
0·014–0·023	210	6	0·753	3·3–2·5
0·023–0·030	237	7	0·799	2·5–2·3
0·030–0·044	263	8	0·838	2·3–1·91
0·044–0·054	289	9	0·869	1·91–1·77
0·054–0·076	314	10	0·895	1·77–1·58
0·076–0·10	339	11	0·916	1·58–1·45
0·10–0·14	364	12	0·933	1·45–1·35
0·14–0·16	389	13	0·946	1·35–1·31
0·16–0·25	413	14	0·957	1·31–1·22
0·25–0·28	438	15	0·966	1·22–1·20
0·28–0·37	462	16	0·973	1·20–1·16
0·37–0·48	486	17	0·979	1·16–1·13
0·48–0·62	510	18	0·983	1·13–1·10
0·62–0·80	534	19	0·987	1·10–1·08
0·80–0·91	558	20	0·989	1·08–1·07
0·91–1·3	581	21	0·992	1·07–1·053
1·3–1·5	605	22	0·993	1·053–1·047
1·5–2·2	628	23	0·995	1·047–1·034
2·2–2·5	652	24	0·996	1·034–1·031
2·5–3·2	675	25	0·997	1·031–1·024

TABLE 5

Non-destructive Inspection: Nominal Lot Size 10,000

Process Average $p_0 = 3\%$ defective.
Consumer's Risk Point $p_1 = 6\%$ defective.
Consumer's Risk = 0·05.

	Scheme			
c_m/c_s	n	k	$A(n, k)$	c/c_m
0–0·002	49·9	1	0·224	∞–14
0·002–0·003	79·1	2	0·314	14–13
0·003–0·004	104·9	3	0·391	13–10
0·004–0·005	129·2	4	0·458	10–7·7
0·005–0·006	152·6	5	0·517	7·7–6·5
0·006–0·009	175·2	6	0·571	6·5–5·1
0·009–0·011	197·4	7	0·619	5·1–4·4
0·011–0·015	219·1	8	0·662	4·4–3·7
0·015–0·019	240·6	9	0·700	3·7–3·2
0·019–0·024	261·8	10	0·735	3·2–2·9
0·024–0·030	282·7	11	0·765	2·9–2·5
0·030–0·036	303·5	12	0·793	2·5–2·3
0·036–0·046	324·0	13	0·817	2·3–2·1
0·046–0·054	344·5	14	0·839	2·1–1·95
0·054–0·065	364·8	15	0·858	1·95–1·82
0·065–0·076	385·0	16	0·875	1·82–1·72
0·076–0·093	405·0	17	0·890	1·72–1·61
0·093–0·11	425·0	18	0·904	1·61–1·53
0·11–0·13	444·9	19	0·915	1·53–1·47
0·13–0·15	464·7	20	0·926	1·47–1·41
0·15–0·18	484·4	21	0·935	1·41–1·36
0·18–0·21	504·0	22	0·943	1·36–1·31
0·21–0·24	523·6	23	0·950	1·31–1·28
0·24–0·28	543·1	24	0·957	1·28–1·25
0·28–0·33	562·5	25	0·962	1·25–1·22
0·33–0·39	581·9	26	0·967	1·22–1·19
0·39–0·45	601·3	27	0·971	1·19–1·16
0·45–0·51	620·6	28	0·975	1·16–1·15
0·51–0·60	639·8	29	0·978	1·15–1·13
0·60–0·70	659·0	30	0·981	1·13–1·12
0·70–0·80	678·2	31	0·983	1·12–1·10
0·80–0·93	697·3	32	0·986	1·10–1·09
0·93–1·1	716·4	33	0·987	1·09–1·08
1·1–1·2	735·4	34	0·989	1·08–1·07
1·2–1·4	754·4	35	0·991	1·07–1·063
1·4–1·6	773·4	36	0·992	1·063–1·056
1·6–1·9	792·3	37	0·993	1·056–1·049
1·9–2·2	811·3	38	0·994	1·049–1·044
2·2–2·6	830·1	39	0·995	1·044–1·038

TABLE 6

Non-destructive Inspection: Nominal Lot Size 10,000

Process Average $p_0 = 3\%$ defective.
Consumer's Risk Point $p_1 = 7\%$ defective.
Consumer's Risk = 0·05.

	Scheme			
c_m/c_s	n	k	$A(n, k)$	c/c_m
0–0·001	42·8	1	0·277	∞–14
0·001–0·002	67·8	2	0·397	14–10
0·002–0·004	89·9	3	0·494	10–6·8
0·004–0·006	110·8	4	0·575	6·8–5·1
0·006–0·008	130·8	5	0·644	5·1–4·0
0·008–0·012	150·2	6	0·702	4·0–3·2
0·012–0·017	169·2	7	0·751	3·2–2·7
0·017–0·023	187·8	8	0·793	2·7–2·3
0·023–0·031	206·2	9	0·827	2·3–2·0
0·031–0·040	224·4	10	0·857	2·0–1·83
0·040–0·052	242·3	11	0·881	1·83–1·66
0·052–0·068	260·1	12	0·902	1·66–1·53
0·068–0·085	277·8	13	0·919	1·53–1·44
0·085–0·11	295·3	14	0·933	1·44–1·37
0·11–0·14	312·7	15	0·945	1·37–1·30
0·14–0·17	330·0	16	0·955	1·30–1·25
0·17–0·21	347·2	17	0·963	1·25–1·21
0·21–0·26	364·3	18	0·969	1·21–1·18
0·26–0·33	381·3	19	0·975	1·18–1·14
0·33–0·40	398·3	20	0·980	1·14–1·12
0·40–0·49	415·2	21	0·983	1·12–1·10
0·49–0·62	432·0	22	0·986	1·10–1·08
0·62–0·75	448·8	23	0·989	1·08–1·07
0·75–0·94	465·5	24	0·991	1·07–1·06
0·94–1·1	482·2	25	0·993	1·06–1·05
1·1–1·4	498·8	26	0·994	1·05–1·042
1·4–1·8	515·4	27	0·995	1·042–1·035
1·8–2·1	531·9	28	0·996	1·035–1·029
2·1–2·7	548·4	29	0·997	1·029–1·024

TABLE 7

Non-destructive Inspection: Nominal Lot Size 10,000

Process Average $p_0 = 4\%$ defective.
Consumer's Risk Point $p_1 = 9\%$ defective.
Consumer's Risk $= 0\cdot05$.

	Scheme			
c_m/c_s	n	k	$A(n, k)$	c/c_m
0–0·001	33·3	1	0·264	∞–13
0·001–0·002	52·7	2	0·378	13–9·5
0·002–0·003	70·0	3	0·469	9·5–7·1
0·003–0·004	86·2	4	0·548	7·1–5·8
0·004–0·006	101·7	5	0·616	5·8–4·4
0·006–0·009	116·8	6	0·673	4·4–3·5
0·009–0·011	131·6	7	0·723	3·5–3·0
0·011–0·016	146·1	8	0·765	3·0–2·5
0·016–0·020	160·4	9	0·801	2·5–2·2
0·020–0·027	174·5	10	0·833	2·2–1·98
0·027–0·034	188·5	11	0·859	1·98–1·82
0·034–0·043	202·3	12	0·881	1·82–1·67
0·043–0·056	216·0	13	0·900	1·67–1·54
0·056–0·068	229·7	14	0·916	1·54–1·46
0·068–0·084	243·2	15	0·930	1·46–1·39
0·084–0·11	256·6	16	0·941	1·39.1·32
0·11–0·13	270·0	17	0·951	1·32–1·27
0·13–0·16	283·3	18	0·959	1·27–1·23
0·16–0·19	296·6	19	0·966	1·23–1·19
0·19–0·23	309·8	20	0·972	1·19–1·17
0·23–0·29	322·9	21	0·976	1·17–1·14
0·29–0·36	336·0	22	0·980	1·14–1·12
0·36–0·43	349·1	23	0·984	1·12–1·10
0·43–0·51	362·1	24	0·987	1·10–1·09
0·51–0·64	375·0	25	0·989	1·09–1·07
0·64–0·76	388·0	26	0·991	1·07–1·06
0·76–0·90	400·9	27	0·992	1·06–1·05
0·90–1·1	413·7	28	0·994	1·05–1·044
1·1–1·3	426·5	29	0·995	1·044–1·037
1·3–1·7	439·3	30	0·996	1·037–1·031
1·7–2·0	452·1	31	0·996	1·031–1·026

TABLE 8

Non-destructive Inspection: Nominal Lot Size 10,000

Process Average $p_0 = 1\%$ defective.
Consumer's Risk Point $p_1 = 3\%$ defective.
Consumer's risk $= 0\cdot01$.

c_m/c_s	*Scheme*		$A(n, k)$	c/c_m
	n	k		
0–0·001	335	4	0·569	∞–45
0·001–0·008	387	5	0·654	45–9·4
0·008–0·018	437	6	0·725	9·4–4·7
0·018–0·029	486	7	0·782	4·7–3·4
0·029–0·052	533	8	0·830	3·4–2·5
0·052–0·076	580	9	0·867	2·5–2·0
0·076–0·11	626	10	0·897	2·0–1·77
0·11–0·16	671	11	0·921	1·77–1·54
0·16–0·23	716	12	0·939	1·54–1·40
0·23–0·30	761	13	0·953	1·40–1·31
0·30–0·39	805	14	0·964	1·31–1·25
0·39–0·55	848	15	0·973	1·25–1·19
0·55–0·75	891	16	0·980	1·19–1·14
0·75–1·0	934	17	0·985	1·14–1·11
1·0–1·3	977	18	0·988	1·11–1·09

TABLE 9

Non-destructive Inspection: Nominal Lot Size 10,000

Process Average $p_0 = 1\%$ defective.
Consumer's Risk Point $p_1 = 4\%$ defective.
Consumer's Risk $= 0\cdot01$.

c_m/c_s	*Scheme*		$A(n, k)$	c/c_m
	n	k		
0–0·004	210	3	0·650	∞–9·3
0·004–0·013	251	4	0·755	9·3–3·8
0·013–0·030	290	5	0·832	3·8–2·2
0·030–0·051	328	6	0·885	2·2–1·86
0·051–0·094	364	7	0·923	1·86–1·50
0·094–0·15	400	8	0·949	1·50–1·33
0·15–0·25	435	9	0·966	1·33–1·21
0·25–0·38	470	10	0·978	1·21–1·15
0·38–0·58	504	11	0·986	1·15–1·10
0·58–0·98	537	12	0·991	1·10–1·06
0·98–1·4	571	13	0·994	1·06–1·05
1·4–2·3	603	14	0·996	1·05–1·03
2·3–3·7	636	15	0·998	1·03–1·02

TABLE 10

Non-destructive Inspection: Nominal Lot Size 10,000

Process Average $p_0 = 1\%$ defective.
Consumer's Risk Point $p_1 = 5\%$ defective.
Consumer's Risk = 0·01.

	Scheme			
c_m/c_s	n	k	$A(n, k)$	c/c_m
0–0·001	133	2	0·610	∞–31
0·001–0·010	168	3	0·762	31–3·5
0·010–0·025	201	4	0·855	3·5–2·1
0·025–0·054	232	5	0·914	2·1–1·57
0·054–0·10	262	6	0·949	1·57–1·32
0·10–0·20	291	7	0·971	1·32–1·18
0·20–0·35	320	8	0·983	1·18–1·11
0·35–0·62	348	9	0·990	1·11–1·07
0·62–1·1	376	10	0·995	1·07–1·04
1·1–2·0	403	11	0·997	1·04–1·02
2·0–3·4	430	12	0·998	1·02–1·01

TABLE 11

Non-destructive Inspection: Nominal Lot Size 10,000

Process Average $p_0 = 2\%$ defective.
Consumer's Risk Point $p_1 = 5\%$ defective.
Consumer's Risk = 0·01.

	Scheme			
c_m/c_s	n	k	$A(n, k)$	c/c_m
0–0·001	262	6	0·574	∞–48
0·001–0·006	291	7	0·635	48–8·9
0·006–0·009	320	8	0·687	8·9–6·6
0·009–0·017	348	9	0·734	6·6–4·2
0·017–0·021	376	10	0·774	4·2–3·6
0·021–0·032	403	11	0·810	3·6–2·8
0·032–0·038	430	12	0·840	2·8–2·5
0·038–0·065	456	13	0·867	2·5–1·96
0·065–0·074	483	14	0·888	1·96–1·86
0·074–0·099	509	15	0·907	1·86–1·67
0·099–0·13	535	16	0·923	1·67–1·53
0·13–0·15	561	17	0·936	1·53–1·48
0·15–0·22	586	18	0·947	1·48–1·34
0·22–0·24	612	19	0·956	1·34–1·31
0·24–0·31	637	20	0·964	1·31–1·25
0·31–0·39	662	21	0·970	1·25–1·21
0·39–0·49	687	22	0·976	1·21–1·17
0·49–0·61	712	23	0·980	1·17–1·14
0·61–0·76	737	24	0·984	1·14–1·11
0·76–0·85	762	25	0·987	1·11–1·10
0·85–1·2	786	26	0·989	1·10–1·08
1·2–1·3	811	27	0·991	1·08–1·07
1·3–1·9	835	28	0·993	1·07–1·053
1·9–2·0	860	29	0·994	1·053–1·048
2·0–2·6	884	30	0·995	1·048–1·040
2·6–3·2	908	31	0·996	1·040–1·033

TABLE 12

Destructive Inspection: Lot Size 10,000

Process Average $p_0 = 1\%$ defective.
Consumer's Risk Point $p_1 = 3\%$ defective.
Consumer's Risk = 0·05.

	Scheme			
c_m/c_s	n	k	$A'(n, k)$	c/c_m
0–0·003	100	1	3,642	∞–11
0·003–0·008	158	2	5,230	11–5·6
0·008–0·015	210	3	6,360	5·6–3·8
0·015–0·030	258	4	7,212	3·8–2·6
0·030–0·047	305	5	7,822	2·6–2·1
0·047–0·083	350	6	8,276	2·1–1·72
0·083–0·12	395	7	8,591	1·72–1·55
0·12–0·20	438	8	8,825	1·55–1·38
0·20–0·33	481	9	8,982	1·38–1·27
0·33–0·48	524	10	9,084	1·27–1·22
0·48–1·0	565	11	9,154	1·22–1·15
1·0–2·2	607	12	9,189	1·15–1·12
2·2–270	648	13	9,205	1·12–1·087
270–∞	689	14	9,206	1·087–1·086

TABLE 13

Destructive Inspection: Lot Size 10,000

Process Average $p_0 = 1\%$ defective.
Consumer's Risk Point $p_1 = 4\%$ defective.
Consumer's Risk = 0·05.

	Scheme			
c_m/c_s	n	k	$A'(n, k)$	c/c_m
0–0·003	75	1	4,688	∞–6·8
0·003–0·009	119	2	6,583	6·8–3·6
0·009–0·024	157	3	7,787	3·6–2·1
0·024–0·046	194	4	8,509	2·1–1·67
0·046–0·088	229	5	8,964	1·67–1·40
0·088–0·17	263	6	9,238	1·40–1·25
0·17–0·33	296	7	9,396	1·25–1·16
0·33–0·69	329	8	9,483	1·16–1·10
0·69–2·4	361	9	9,525	1·10–1·07
2·4–∞	393	10	9,537	1·07–1·05

TABLE 14

Destructive Inspection: Lot Size 10,000

Process Average $p_0 = 1\%$ defective.
Consumer's Risk Point $p_1 = 5\%$ defective.
Consumer's Risk = 0·05.

	Scheme			
c_m/c_s	n	k	$A'(n, k)$	c/c_m
0–0·003	60	1	5,455	∞–5·0
0·003–0·012	95	2	7,470	5·0–2·4
0·012–0·030	126	3	8,553	2·4–1·66
0·030–0·069	155	4	9,135	1·66–1·34
0·069–0·15	183	5	9,438	1·34–1·22
0·15–0·37	210	6	9,590	1·22–1·10
0·37–0·99	237	7	9,657	1·10–1·06
0·99–57	263	8	9,682	1·06–1·033
57–∞	289	9	9,682	$1{\cdot}033^{+}$–$1{\cdot}033^{-}$

TABLE 15

Destructive Inspection: Lot Size 10,000

Process Average $p_0 = 2\%$ defective.
Consumer's Risk Point $p_1 = 5\%$ defective.
Consumer's Risk = 0·05.

	Scheme			
c_m/c_s	n	k	$A'(n, k)$	c/c_m
0–0·002	60	1	2,994	∞–13
0·002–0·004	95	2	4,296	13–8·6
0·004–0·006	126	3	5,319	8·6–5·9
0·006–0·010	155	4	6,152	5·9–4·1
0·010–0·015	183	5	6,822	4·1–3·2
0·015–0·025	210	6	7,373	3·2–2·5
0·025–0·033	237	7	7,802	2·5–2·2
0·033–0·050	263	8	8,161	2·2–1·87
0·050–0·061	289	9	8,440	1·87–1·74
0·061–0·089	314	10	8,673	1·74–1·56
0·089–0·13	339	11	8,853	1·56–1·43
0·13–0·18	364	12	8,990	1·43–1·34
0·18–0·22	389	13	9,094	1·34–1·30
0·22–0·37	413	14	9,179	1·30–1·21
0·37–0·45	438	15	9,235	1·21–1·19
0·45–0·69	462	16	9,280	1·19–1·15
0·69–1·1	486	17	9,310	1·15–1·12
1·1–2·3	510	18	9,329	1·12–1·10
2·3–8·5	534	19	9,338	1·10–1·08
8·5–∞	558	20	9,341	1·08–1·07

TABLE 16

Destructive Inspection: Lot Size 10,000

Process Average $p_0 = 2\%$ defective.
Consumer's Risk Point $p_1 = 6\%$ defective.
Consumer's Risk = 0·05.

	Scheme			
c_m/c_s	n	k	$A'(n, k)$	c/c_m
0–0·002	50	1	3,660	∞–11
0·002–0·004	79	2	5,272	11–5·7
0·004–0·007	105	3	6,428	5·7–3·9
0·007–0·015	129	4	7,308	3·9–2·5
0·015–0·020	153	5	7,928	2·5–2·2
0·020–0·035	175	6	8,426	2·2–1·78
0·035–0·058	197	7	8,778	1·78–1·53
0·058–0·091	219	8	9,028	1·53–1·37
0·091–0·12	241	9	9,203	1·37–1·30
0·12–0·19	262	10	9,335	1·30–1·22
0·19–0·26	283	11	9,424	1·22–1·17
0·26–0·53	303	12	9,489	1·17–1·11
0·53–0·78	324	13	9,524	1·11–1·09
0·78–2·4	344	14	9,548	1·09–1·06
2·4–7·4	365	15	9,556	1·06–1·052
7·4–∞	385	16	9,559	1·052–1·046

TABLE 17

Destructive Inspection: Lot Size 10,000

Process Average $p_0 = 1\%$ defective.
Consumer's Risk Point $p_1 = 3\%$ defective.
Consumer's Risk = 0·01.

c_m/c_s	*Scheme*		$A'(n, k)$	c/c_m
	n	k		
0–0·003	335	4	5,502	∞–23
0·003–0·010	387	5	6,289	23–7·7
0·010–0·023	437	6	6,933	7·7–4·2
0·023–0·036	486	7	7,443	4·2–3·2
0·036–0·065	533	8	7,857	3·2–2·3
0·065–0·099	580	9	8,169	2·3–1·94
0·099–0·15	626	10	8,409	1·94–1·70
0·15–0·23	671	11	8,590	1·70–1·50
0·23–0·37	716	12	8,719	1·50–1·37
0·37–0·56	761	13	8,806	1·37–1·29
0·56–0·91	805	14	8,867	1·29–1·23
0·91–2·0	848	15	8,905	1·23–1·17
2·0–12	891	16	8,924	1·17–1·13
12–∞	934	17	8,927	1·13–1·12

TABLE 18

Destructive Inspection: Lot Size 10,000

Process Average $p_0 = 1\%$ defective.
Consumer's Risk Point $p_1 = 4\%$ defective.
Consumer's Risk = 0·01.

c_m/c_s	*Scheme*		$A'(n, k)$	c/c_m
	n	k		
0–0·005	210	3	6,360	∞–8·2
0·005–0·015	251	4	7,365	8·2–3·6
0·015–0·034	290	5	8,077	3·6–2·3
0·034–0·059	328	6	8,562	2·3–1·82
0·059–0·12	364	7	8,898	1·82–1·48
0·12–0·20	400	8	9,109	1·48–1·32
0·20–0·38	435	9	9,243	1·32–1·20
0·38–0·73	470	10	9,318	1·20–1·14
0·73–1·8	504	11	9,359	1·14–1·10
1·8–∞	537	12	9,375	1·10–1·07

TABLE 19

Destructive Inspection: Lot Size 10,000

Process Average $p_0 = 1\%$ defective.
Consumer's Risk Point $p_1 = 5\%$ defective.
Consumer's Risk = 0·01.

c_m/c_s	*Scheme*		$A'(n, k)$	c/c_m
	n	k		
0–0·001	133	2	6,021	∞–24
0·001–0·011	168	3	7,497	24–3·3
0·011–0·028	201	4	8,381	3·3–2·1
0·028–0·061	232	5	8,927	2·1–1·55
0·061–0·12	262	6	9,246	1·55–1·31
0·12–0·27	291	7	9,426	1·31–1·18
0·27–0·61	320	8	9,517	1·18–1·11
0·61–2·0	348	9	9,559	1·11–1·06
2·0–∞	376	10	9,572	1·06–1·04

TABLE 20

Destructive Inspection: Lot Size 10,000

Process Average $p_0 = 2\%$ defective.
Consumer's Risk Point $p_1 = 5\%$ defective.
Consumer's Risk $= 0\cdot01$.

	Scheme			
c_m/c_s	n	k	$A'(n, k)$	c/c_m
0–0·002	262	6	5,589	∞–27
0·002–0·008	291	7	6,167	27–7·8
0·008–0·011	320	8	6,653	7·8–5·9
0·011–0·020	348	9	7,087	5·9–3·9
0·020–0·025	376	10	7,450	3·9–3·4
0·025–0·038	403	11	7,772	3·4–2·6
0·038–0·046	430	12	8,039	2·6–2·4
0·046–0·080	456	13	8,273	2·4–1·90
0·080–0·093	483	14	8,451	1·90–1·80
0·093–0·13	509	15	8,607	1·80–1·62
0·13–0·18	535	16	8,732	1·62–1·50
0·18–0·20	561	17	8,831	1·50–1·45
0·20–0·33	586	18	8,916	1·45–1·32
0·33–0·39	612	19	8,976	1·32–1·29
0·39–0·55	637	20	9,026	1·29–1·24
0·55–0·83	662	21	9,062	1·24–1·19
0·83–1·3	687	22	9,088	1·19–1·16
1·3–2·5	712	23	9,104	1·16–1·13
2·5–8·7	737	24	9,113	1·13–1·11
8·7–∞	762	25	9,115	1·11–1·10

TABLE 21

Destructive Inspection: Lot Size 10,000

Process Average $p_0 = 2\%$ defective.
Consumer's Risk Point $p_1 = 6\%$ defective.
Consumer's Risk $= 0\cdot01$.

	Scheme			
c_m/c_s	n	k	$A'(n, k)$	c/c_m
0–0·001	167	4	5,620	∞–26
0·001–0·005	193	5	6,435	26–8·1
0·005–0·012	218	6	7,108	8·1–4·0
0·012–0·018	243	7	7,633	4·0–3·1
0·018–0·026	267	8	8,067	3·1–2·5
0·026–0·043	290	9	8,420	2·5–1·99
0·043–0·067	313	10	8,689	1·99–1·69
0·067–0·087	336	11	8,892	1·69–1·56
0·087–0·13	358	12	9,055	1·56–1·40
0·13–0·20	380	13	9,173	1·40–1·30
0·20–0·30	402	14	9,258	1·30–1·22
0·30–0·47	424	15	9,318	1·22–1·17
0·47–0·64	446	16	9,358	1·17–1·14
0·64–1·5	467	17	9,387	1·14–1·10
1·5–2·7	489	18	9,400	1·10–1·08
2·7–160	510	19	9,407	1·08–1·063
160–∞	531	20	9,407	$1\cdot063^{+}$–$1\cdot063^{-}$

TABLE 22

Destructive Inspection: Lot Size 20,000

Process Average $p_0 = 1\%$ defective.
Consumer's Risk Point $p_1 = 3\%$ defective.
Consumer's Risk $= 0\cdot05$.

	Scheme			
c_m/c_s	n	k	$A'(n, k)$	c/c_m
0–0·002	100	1	7,321	∞–11
0·002–0·004	158	2	10,544	11–5·7
0·004–0·007	210	3	12,856	5·7–3·9
0·007–0·014	258	4	14,616	3·9–2·6
0·014–0·022	305	5	15,890	2·6–2·1
0·022–0·038	350	6	16,852	2·1–1·73
0·038–0·053	395	7	17,536	1·73–1·57
0·053–0·085	438	8	18,055	1·57–1·39
0·085–0·13	481	9	18,418	1·39–1·28
0·13–0·18	524	10	18,670	1·28–1·23
0·18–0·31	565	11	18,856	1·23–1·16
0·31–0·48	607	12	18,972	1·16–1·12
0·48–0·86	648	13	19,049	1·12–1·09
0·86–1·9	689	14	19,092	1·09–1·07
1·9–7·3	730	15	19,112	1·07–1·052
7·3–∞	770	16	19,117	1·052–1·046

TABLE 23

Destructive Inspection: Lot Size 20,000

Process Average $p_0 = 1\%$ defective.
Consumer's Risk Point $p_1 = 4\%$ defective.
Consumer's Risk $= 0\cdot05$.

	Scheme			
c_m/c_s	n	k	$A'(n, k)$	c/c_m
0–0·002	75	1	9,412	∞–6·9
0·002–0·004	119	2	13,246	6·9–3·6
0·004–0·012	157	3	15,697	3·6–2·1
0·012–0·022	194	4	17,187	2·1–1·68
0·022–0·041	229	5	18,138	1·68–1·41
0·041–0·074	263	6	18,725	1·41–1·26
0·074–0·13	296	7	19,078	1·26–1·16
0·13–0·25	329	8	19,288	1·16–1·11
0·25–0·51	361	9	19,406	1·11–1·07
0·51–1·1	393	10	19,465	1·07–1·05
1·1–10	424	11	19,491	1·05–1·028
10–∞	455	12	19,494	1·028–1·026

TABLE 24

Destructive Inspection: Lot Size 20,000

Process Average $p_0 = 1\%$ defective.
Consumer's Risk Point $p_1 = 5\%$ defective.
Consumer's Risk = 0·05.

c_m/c_s	*Scheme*		$A'(n, k)$	c/c_m
	n	k		
0–0·002	60	1	10,943	∞–5·0
0·002–0·006	95	2	15,011	5·0–2·4
0·006–0·015	126	3	17,215	2·4–1·67
0·015–0·033	155	4	18,415	1·67–1·34
0·033–0·068	183	5	19,052	1·34–1·19
0·068–0·15	210	6	19,385	1·19–1·10
0·15–0·32	237	7	19,548	1·10–1·06
0·32–0·91	263	8	19,625	1·06–1·03
0·91–6·8	289	9	19,652	1·03–1·020
6·8–∞	314	10	19,656	1·020–1·018

TABLE 25

Destructive Inspection: Lot Size 20,000

Process Average $p_0 = 2\%$ defective.
Consumer's Risk Point $p_1 = 5\%$ defective.
Consumer's Risk = 0·05.

c_m/c_s	*Scheme*		$A'(n, k)$	c/c_m
	n	k		
0–0·001	60	1	6,006	∞–13
0·001–0·002	95	2	8,634	13–8·8
0·002–0·003	126	3	10,706	8·8–6·0
0·003–0·005	155	4	12,400	6·0–4·2
0·005–0·007	183	5	13,771	4·2–3·3
0·007–0·012	210	6	14,905	3·3–2·5
0·012–0·016	237	7	15,793	2·5–2·2
0·016–0·023	263	8	16,542	2·2–1·89
0·023–0·029	289	9	17,131	1·89–1·76
0·029–0·041	314	10	17,628	1·76–1·57
0·041–0·057	339	11	18,017	1·57–1·44
0·057–0·079	364	12	18,320	1·44–1·34
0·079–0·093	389	13	18,556	1·34–1·30
0·093–0·15	413	14	18,754	1·30–1·21
0·15–0·17	438	15	18,893	1·21–1·19
0·17–0·24	462	16	19,009	1·19–1·15
0·24–0·34	486	17	19,095	1·15–1·12
0·34–0·49	510	18	19,158	1·12–1·10
0·49–0·73	534	19	19,203	1·10–1·08
0·73–0·93	558	20	19,234	1·08–1·07
0·93–2·4	581	21	19,257	1·07–1·05
2·4–4·1	605	22	19,266	1·051–1·046
4·1–∞	628	23	19,272	1·046–1·038

TABLE 26

Destructive Inspection: Lot Size 20,000

Process Average $p_0 = 2\%$ defective.
Consumer's Risk Point $p_1 = 6\%$ defective.
Consumer's Risk = 0·05.

	Scheme			
c_m/c_s	n	k	$A'(n, k)$	c/c_m
0–0·001	50	1	7,339	∞–11
0·001–0·002	79	2	10,586	11–5·7
0·002–0·003	105	3	12,924	5·7–3·9
0·003–0·007	129	4	14,711	3·9–2·5
0·007–0·010	153	5	15,978	2·5–2·3
0·010–0·017	175	6	17,002	2·3–1·79
0·017–0·027	197	7	17,733	1·79–1·53
0·027–0·042	219	8	18,257	1·53–1·38
0·042–0·056	241	9	18,633	1·38–1·30
0·056–0·085	262	10	18,921	1·30–1·22
0·085–0·11	283	11	19,123	1·22–1·18
0·11–0·20	303	12	19,274	1·18–1·12
0·20–0·27	324	13	19,368	1·12–1·10
0·27–0·52	344	14	19,436	1·10–1·06
0·52–0·73	365	15	19,474	1·06–1·05
0·73–1·5	385	16	19,500	1·05–1·04
1·5–5·9	405	17	19,513	1·04–1·028
5·9–∞	425	18	19,516	1·028–1·025

TABLE 27

Destructive Inspection: Lot Size 20,000

Process Average $p_0 = 1\%$ defective.
Consumer's Risk Point $p_1 = 3\%$ defective.
Consumer's Risk = 0·01.

	Scheme			
c_m/c_s	n	k	$A'(n, k)$	c/c_m
0–0·001	335	4	11,196	∞–30
0·001–0·004	387	5	12,831	30–8·4
0·004–0·010	437	6	14,182	8·4–4·4
0·010–0·016	486	7	15,267	4·4–3·3
0·016–0·029	533	8	16,157	3·3–2·4
0·029–0·043	580	9	16,841	2·4–1·99
0·043–0·062	626	10	17,379	1·99–1·73
0·062–0·095	671	11	17,798	1·73–1·52
0·095–0·14	716	12	18,109	1·52–1·38
0·14–0·20	761	13	18,338	1·38–1·30
0·20–0·28	805	14	18,510	1·30–1·24
0·28–0·43	848	15	18,636	1·24–1·18
0·43–0·71	891	16	18,720	1·18–1·14
0·71–1·3	934	17	18,773	1·14–1·10
1·3–2·4	977	18	18,802	1·10–1·08
2·4–∞	1,019	19	18,817	1·08–1·06

TABLE 28

Destructive Inspection: Lot Size 20,000

Process Average $p_0 = 1\%$ defective.
Consumer's Risk Point $p_1 = 4\%$ defective.
Consumer's Risk = 0·01.

	Scheme			
c_m/c_s	n	k	$A'(n, k)$	c/c_m
0–0·002	210	3	12,856	∞–8·7
0·002–0·007	251	4	14,919	8·7–3·7
0·007–0·016	290	5	16,394	3·7–2·3
0·016–0·027	328	6	17,415	2·3–1·84
0·027–0·052	364	7	18,131	1·84–1·49
0·052–0·086	400	8	18,598	1·49–1·33
0·086–0·15	435	9	18,906	1·33–1·21
0·15–0·25	470	10	19,096	1·21–1·15
0·25–0·44	504	11	19,215	1·15–1·10
0·44–1·1	537	12	19,283	1·10–1·06
1·1–2·7	571	13	19,312	1·06–1·046
2·7–∞	603	14	19,324	1·046–1·035

TABLE 29

Destructive Inspection: Lot Size 20,000

Process Average $p_0 = 1\%$ defective.
Consumer's Risk Point $p_1 = 5\%$ defective.
Consumer's Risk = 0·01.

	Scheme			
c_m/c_s	n	k	$A'(n, k)$	c/c_m
0–0·0004	133	2	12,124	∞–27
0·0004–0·005	168	3	15,122	27–3·4
0·005–0·013	201	4	16,934	3·4–2·1
0·013–0·029	232	5	18,066	2·1–1·56
0·029–0·056	262	6	18,741	1·56–1·32
0·056–0·11	291	7	19,134	1·32–1·18
0·11–0·22	320	8	19,349	1·18–1·11
0·22–0·47	348	9	19,462	1·11–1·07
0·47–1·3	376	10	19,518	1·07–1·040
1·3–∞	403	11	19,538	1·040–1·024

TABLE 30

Destructive Inspection: Lot Size 20,000

Process Average $p_0 = 2\%$ defective.
Consumer's Risk Point $p_1 = 5\%$ defective.
Consumer's Risk = 0·01.

c_m/c_s	*Scheme*		$A'(n, k)$	c/c_m
	n	k		
0–0·001	262	6	11,328	∞–35
0·001–0·003	291	7	12,519	35–8·3
0·003–0·005	320	8	13,526	8·3–6·3
0·005–0·009	348	9	14,430	6·3–4·0
0·009–0·011	376	10	15,191	4·0–3·5
0·011–0·018	403	11	15,871	3·5–2·7
0·018–0·021	430	12	16,439	2·7–2·5
0·021–0·036	456	13	16,942	2·5–1·93
0·036–0·041	483	14	17,331	1·93–1·83
0·041–0·056	509	15	17,675	1·83–1·65
0·056–0·075	535	16	17,957	1·65–1·51
0·075–0·085	561	17	18,187	1·51–1·46
0·085–0·13	586	18	18,388	1·46–1·33
0·13–0·15	612	19	18,536	1·33–1·30
0·15–0·20	637	20	18,666	1·30–1·24
0·20–0·26	662	21	18,767	1·24–1·20
0·26–0·36	687	22	18,846	1·20–1·16
0·36–0·49	712	23	18,906	1·16–1·13
0·49–0·70	737	24	18,950	1·13–1·11
0·70–0·85	762	25	18,982	1·11–1·10
0·85–1·8	786	26	19,008	1·10–1·075
1·8–2·6	811	27	19,020	1·075–1·068
2·6–∞	835	28	19,029	1·068–1·051

TABLE 31

Destructive Inspection: Lot Size 20,000

Process Average $p_0 = 2\%$ defective.
Consumer's Risk Point $p_1 = 6\%$ defective.
Consumer's Risk = 0·01.

c_m/c_s	*Scheme*		$A'(n, k)$	c/c_m
	n	k		
0–0·0005	167	4	11,335	∞–30
0·0005–0·002	193	5	12,997	30–6·9
0·002–0·006	218	6	14,374	6·9–4·1
0·006–0·009	243	7	15,457	4·1–3·1
0·009–0·012	267	8	16,355	3·1–2·6
0·012–0·020	290	9	17,092	2·6–2·0
0·020–0·031	313	10	17,660	2·0–1·70
0·031–0·040	336	11	18,094	1·70–1·57
0·040–0·059	358	12	18,446	1·57–1·41
0·059–0·086	380	13	18,709	1·41–1·30
0·086–0·12	402	14	18,905	1·30–1·23
0·12–0·18	424	15	19,048	1·23–1·17
0·18–0·23	446	16	19,153	1·17–1·15
0·23–0·40	467	17	19,233	1·15–1·10
0·40–0·53	489	18	19,283	1·10–1·09
0·53–0·89	510	19	19,320	1·09–1·06
0·89–1·8	531	20	19,342	1·06–1·05
1·8–6·5	552	21	19,354	1·05–1·038
6·5–∞	573	22	19,357	1·038–1·033

DISCUSSION ON MR. HORSNELL'S PAPER

Professor G. A. **BARNARD**: It is a great pleasure to move a vote of thanks to Mr. Horsnell, for his paper seems to me to represent a major advance in the theory of sampling inspection schemes. It is important to recognize quite clearly wherein this advance lies, but before coming to that I have a few critical comments to make on pages 153–155, which were a little barren. We at Imperial College did some work on trying to find out what sort of process curves do in fact turn up in industry and none we have seen bears the slightest resemblance to those tabulated in Text-Table I. It seems to me therefore that the discussion on pages 153–155 becomes rather academic. The conditions which are derived are such that one can never know in practice whether they are fulfilled or not.

Apart from these minor criticisms the paper is a valuable discussion of the principles involved and the particular advance which is its outstanding feature is the way in which the problem of design of an inspection scheme is formulated. It is not that the particular procedures of sampling which are given in Mr. Horsnell's tables are any more economical than other sampling schemes. Indeed such single sampling schemes as he has mentioned are usually less economical than sequential schemes. A single sampling scheme which calls for a sample of 50 from each batch, with an acceptance number of 2, has its own costs and other properties, independently of the way these numbers, 50 and 2, were arrived at. The special merit of Mr. Horsnell's work lies in the way the schemes are presented. He has, in effect, struck a happy mean between the old-fashioned approach, in which the producer's risk point and safe point, together with their associated probabilities, were specified, and the more modern approach, involving the specification of the process curve and the relevant cost functions. At Imperial College we have devoted a good deal of effort to the exploration of this latter approach, but experience of applications shows that the quantities which are required to be known in this approach are rarely known with sufficient accuracy. I have on occasion argued that in practical cases where this was so, since it was usually the costs of various types of decision which could not be evaluated, we should throw the burden of determining these things on to the industrial accountants. I now feel sure, however, that this would be to impose a burden on a body of people quite unable to bear it. In fact, I think we should reverse the process. What Mr. Horsnell has succeeded in doing is to fix attention on the cost of what happens in normal production, while at the same time formulating the problem of what happens in abnormal circumstances in the form more customary with life assurance, where one assesses the risks as probabilities. I do not believe that Mr. Horsnell would claim that his approach is necessarily the last word in this line. I do not think that anyone could hope to achieve anything like a final answer. He himself remarks, in connection with the relation between batch size and lot size, that the risk of accepting goods of bad quality should fall as the batch size increases; so that the sample size should rise more rapidly than log N. But the effect of this will not be very serious. This confirms a result arrived at by Dr. Vagholkar, starting from the purely economic point of view.

It occurred to me that it might be worth while, particularly in view of the need to emphasize the economic advantages of sequential schemes, to examine how Mr. Horsnell's principle would apply to sequential schemes. The result seems sufficiently simple to be worth putting on record.

As is well known, in designing a sequential inspection scheme, we usually begin with the process average p_0, the producer's risk point p_1, and the associated risks α and β respectively. Then at any stage of sampling, when we have obtained r defectives and s non-defective, we continue sampling if the inequality

$$\frac{\alpha}{1-\beta} < \frac{p_0{}^r q_0{}^s}{p_1{}^r q_1{}^s} < \frac{1-\alpha}{\beta}$$

is satisfied. If it is broken on the right we accept, and if it is broken on the left we reject. Taking logarithms, the inequality becomes

$$\log\frac{\alpha}{1-\beta} < s\log\frac{q_0}{q_1} - r\log\frac{p_1}{p_0} < \log\frac{1-\alpha}{\beta}$$

and the quantity in the centre can now be interpreted as a score which increases by an amount $\log q_0/q_1$ each time we get an effective item, and decreases by $\log p_1/p_0$ each time we get a defective item. At the process average the mean increase in score per item inspected is then

$$\Delta = q_0\log\frac{q_0}{q_1} - p_0\log\frac{p_1}{p_0}$$

The mean sample size at p_0 is then

$$S(\alpha) = \left\{(1 - \alpha) \log \frac{1 - a}{\beta} - \alpha \log \frac{1 - \beta}{\alpha}\right\} \Big/ \Delta$$

provided α and β are small.

If we take the cost of inspecting one item as our unit, and in such units the cost of rejecting a batch of process average quality is B, Horsnell's principle says we should minimize $\alpha B + S(\alpha)$. Differentiating in the usual way, we find the necessary relationship is

$$\log \frac{1 - \alpha}{\alpha} + \log \frac{1 - \beta}{\beta} = B\Delta$$

In words, the log odds against wrongly rejecting, plus the log odds against wrongly accepting, must equal $B\Delta$. This means that the design of such a scheme is quite easy if we use the Ministry of Supply Target Handicap Charts for Sequential Schemes, which are worked in terms of a handicap H and a penalty b, where

$$b = \frac{\log p_1/p_0}{\log q_0/q_1} \text{ and } H = \frac{2}{\log \dfrac{q_0}{q_1}}$$

so that

$$\Delta = 2(q_0 - bp_0)/H$$

To take an example, let us suppose that p_0 is 1 per cent., and p_1 is 5 per cent. We find from the charts that b equals 39 and H equals 112. From this we calculate Δ which turns out to be 3/280. If, to make the arithmetic simple, we take B to be 280, we find that the sum of the log odds must be 3, which means that if we want each log odds of wrongful acceptance to be 100 to 1, then our log odds of wrongful rejection should be 10 to 1. Thus the scheme we want should be specified by the inequalities

$$\frac{H}{2}\left(\log \frac{1/11}{100/101}\right) < S - 39r < \left(\log \frac{10/11}{1/101}\right) \cdot \frac{H}{2}$$

or

$$-\frac{H}{2} < S - 39r < H$$

nearly.

Miss Keen: Usually, I feel justified in dismissing schemes concerned with the economics of sampling without overmuch consideration, because they usually contain parameters for which it is impossible to substitute practical values. The present schemes, in avoiding this difficulty, have forced me to study them in more detail and I feel that I ought to be grateful to Mr. Horsnell for providing this goad even though deploring the time spent on reacting to it.

The probability of his schemes being used is increased not only by this practical point but by the provision of adequate tables, a pre-requisite for use in factories which is not always met. I hesitate to ask anyone to add to tables but I wonder whether they could be extended to cover process average levels of, say, 10, 15, 20 and 25 per cent. defective? These may sound high process averages but, during recent statistics classes run by my Company, I had occasion to mention high and low proportions of defects. It became obvious from subsequent discussion that there were two distributions of auditors: those who thought "high" meant about 5 per cent., and those who thought 10 per cent. was a low process average. This suggests that there may be two distinct types of production which fall into these categories.

The tabular values given may be scaled up for use with batches in excess of 10,000. We have found in practice that natural homogeneous batches frequently seem to be of the order of 2,000 or 3,000 in size. However much it is emphasized that the advantages of sampling from larger batches can be enjoyed only if the larger batches are collections of smaller batches of similar quality, the temptation remains great to make up large batches from any heterogeneous collection of odds and ends. If it is succumbed to, then the value of these schemes may well be reduced by the difficulties of selecting a random sample from the resulting large non-homogeneous batch.

Although Mr. Horsnell has left in his equations only "easily obtainable" costs, I still think that at the present stage of sophistication we shall find it difficult to evaluate these costs. The effective cost of an accepted item depends, among other things, on the direct manufacturing cost and the inspection cost: the manufacturing cost ought not to be difficult to obtain, although in practice one often finds that costing systems are not necessarily designed to provide the cost of individual components or assemblies. Inspection costs are another matter: they often seem to be considered, not as part of the cost of the article, but as an overhead; in many cases, one department inspects many different types of article and it is difficult to separate the gross cost of the inspection into the costs of inspecting the separate types. In some cases where the cost *is* known, the conventional splitting of inspection costs from production costs results in its not being included in the manufacturing cost. We have met cases where over-all costs could be substantially reduced by adjusting inspection schemes in which no action is taken because the official cost figures would not reflect the reduction.

I am afraid that Mr. Horsnell may be another prophet too much in advance of his times. We are still too frequently in the position of trying to move people away from 100 per cent. or fixed sampling fraction inspection to something as sophisticated as an ordinary single sampling scheme. In these cases we find that agreement is finally reached after shunting to and fro from producer's and consumer's risks to sample size. Since most customers do not know what a sampling plan implies, the producer's risk point is usually fixed at the process average and the consumer's risk point at a value at which it is felt that even a consumer will turn. I suggest that two things will force manufacturers to use the most economic schemes: their inclusion in government specifications, and an informed market which knows what a consumer's risk is. Mr. Horsnell says that the Columbia S.R. schemes differ from his own in concentrating on the acceptance of normal quality batches, rather than on the rejection of poor quality batches. Whilst the market continues, in general, not to do its own inspection, the Columbia type of scheme buttressed by free replacement of defective articles is probably the easiest type to use.

One immediate sphere for the use of such schemes is probably in a department which acts as supplier to another part of the same factory. It is in this situation, if any, that accurate costing should be possible and small savings worth making on a long-term basis.

I have much pleasure in seconding the vote of thanks for this stimulating paper.

The vote of thanks was put to the meeting and carried unanimously.

Mr. R. H. S. Lesser: Mr. Horsnell has given us a thought-provoking paper, but I am wondering how these schemes will work out in practice. Everything depends on the choice of consumer's risk point, which is arbitrary. I think there will be cases where this will lead us away from the cheapest arrangement, unless we are prepared to vary the lot tolerance with changing process conditions. This in turn would introduce difficulties in incorporating mass quality requirements in specifications, a feature I regard as important.

From the viewpoint of discriminating between lots of good and bad quality, the choice of scheme must surely depend on the expected distribution of submitted lots. This, after all, is a measure of the job to be done. The prospect of finding the best way to do the job without defining the job itself seems to me to be something of a mirage. True, the process curve is never known exactly (neither are costs when delayed production and inspectors with partial slack time are considered) but, as Professor Barnard has said, to the extent that our knowledge is approximate our scheme can only be approximate.

Mr. Horsnell seems unduly pessimistic about the ability of a sampling plan to exert pressure for process improvement. He underlines this by suggesting that the producer will pass all costs to the consumer anyway. This surely is over-simplification. In a competitive situation it behoves the producer to reduce his cost price as much as possible. Once a price has been agreed, rejected lots represent a real loss to him, which he will try to eliminate by process improvement. A minor side issue here seems to be that with these schemes the producer's right price depends on the sampling plan while the plan itself depends on the price he adopts.

I should be interested to know whether the consumer, besides fixing his own risk and risk point, should also impose some limitation on the process average. If not, then as the process average approaches the consumer's risk point we should be taking larger and larger sample sizes in order to accept as much process average quality as possible. Yet in this case the average outgoing quality passing to the consumer becomes progressively worse. Here surely is the dilemma of the "fixed consumer's safeguard". Without recourse to the distribution of submitted lots, I do not see how there can be any such thing. On the other hand, if the consumer does impose a limitation on the process average, then he is back to specifying an acceptable quality level after all.

One of the most useful ways in which sampling schemes can be made more economic is, I think, by relating acceptance and sentencing conditions to previous quality history. Considerable improvement in operating characteristics can be obtained by what we call "switching mechanisms", that is, schemes involving normal and tightened inspection, etc. In one case recently we arrived at a scheme involving sampling plans of the following type:

	AQL 1 *per cent.*		
	Sample Size	*Accept*	*Reject*
Normal inspection . . .	150	4	5
Tightened inspection . . .	150	2	3

"use tightened inspection whenever 2 or more lots in the last 5 lots exceed 3 rejects. Otherwise use normal inspection".

Three is referred to here as the "critical number". This scheme obviates some of the difficulties of the switching mechanism of MIL STD 105A, which involves a moving average of the last 10 lots. This aspect of the military standard is cumbersome to operate and is open to the drawback that a single very bad lot may bring tightened inspection into force when it is not needed and then make it persist for an unduly long time. Tightened inspection is of course not intended as protection against very bad lots, but as protection against a general deterioration in the quality level.

Another method we have tried involves variable sentencing. On an assembly line with continuous production we were working with samples of 1 from lots of 10. Results were plotted sequentially and current quality rated "good", "fair" or "poor" according to the position of the random walk. On finding a defective, sentencing depended on the quality rating as follows:

Quality Rating	*Action*
Good . . .	Inspect current lot, unless last 10 samples were clear of defects in which case take no further action.
Fair . . .	Inspect current lot.
Poor . . .	Inspect current lot and 2 further lots.

This scheme was designed to maintain an A.O.Q.L. under conditions of local homogeneity, but longer term drift in the process average. It avoided certain practical drawbacks of the continuous flow plans by H. F. Dodge.

This rather suggests that we need not only information about the distribution of submitted lots, but also knowledge of how that distribution varies with time.

Mr. Horsnell has said that the field needs further exploration and I agree with him. He has himself provided a valuable mine of information which can only be of great benefit in these enquiries.

Dr. Johnson: As I have been rather closely associated with the author during the period of preparation of this paper I find it difficult to discuss the material presented in a properly objective manner. The idea of controlling the average cost of accepted items, rather than the probability ("producer's risk") of rejecting lots of average quality, is based on the author's experience of practical requirements, and the tables he provides should be of value to a number of users.

It may be noted that the cost c in equation (1) is not likely to depend critically on the form of $y(p)$. For c depends on $y(p)$ only in the term $\int_0^1 A(p, n, k)\, y(p)\, dp$ in the denominator. This is c weighted average of the $A(p, n, h)$'s and the weights $y(p)$ satisfy the condition $\int_0^1 py(p)\, dp = p_0$. In the neighbourhood of $p = p_0$, $A(p, n, h)$ will be approximately a linear function of p and so the weighted average of $A(p, n, h)$ will remain approximately the same as $y(p)$ varies, provided the average value of p remains at p_0. It follows that the parameters of the most economical scheme will also vary but little with the form of $y(p)$.

There are two points of criticism, or rather emendation, which might be made. The most economical procedure depends on the assumed level of quality of normal production, which may be denoted for brevity by p_0. If p_0 should change then the approximate most economical scheme will also change. It is of course possible that for a fixed consumer's risk, the most economical scheme may vary only slowly with p_0. Perhaps the author can give us some information on this point.

The second point is that the retention of an arbitrary "consumer's risk" seems a little illogical. Perhaps it has been retained merely as a reasonable second condition to fix the values of n and k. This condition is just as arbitrary as the fixed producer's risk conditions which the author has removed. A possible alternative condition might be obtained by assuming the consumer applies his own acceptance test with probability of acceptance equal to $A_1(p)$. Then the proportion of lots finally accepted would be

$$\int_0^1 y(p)\, A(p,\ n,\ d)\, A_1(p)\, kp.$$

If the cost to the producer of accepting a lot which the consumer rejects be c' (e.g. a penalty and/or extra costs of transit), then the cost per finally accepted item would be

$$\frac{Nc_m + nc + c' \int_0^1 y(p)\, A(p, n, k)(1 - A_1(p))\, dp}{N \int_0^1 y(p)\, A(p, n, k)\, A_1(p)\, dp}$$

n and k might then be chosen to minimize this quantity.

It seems, in fact, that the "consumer's risk" condition is really a condition to ensure that the occasional "wild" lot, with a proportion of defectives possibly well in excess of the nominal "consumer's risk" level is not accepted.

Mr. I. D. Hill: Mr. Horsnell is to be congratulated on some interesting ideas, and on having given us not merely the algebraic consequences of those ideas, but also some arithmetical tables by which we can judge their practical results.

Everyone agrees, I think, that in considering any sampling inspection plan, the scheme adopted and the process curve are both of great importance, but there are two different approaches to the solution of the problems involved, according to which of these is considered to be the cause and which the effect. Mr. Horsnell's approach is to consider the process curve as the cause, that is, the process curve is taken as fixed and the sampling scheme is calculated to give optimum results for that fixed process. My own approach is from the other direction, namely, to consider the process curve as being largely caused by the sampling scheme adopted. The job of choosing the sampling scheme then becomes that of deciding what scheme will give the manufacturer an economic incentive to offer the desired process curve. The job of the inspector, from this viewpoint, becomes less that of the man who sorts things out (who does the best he can with what he is given), but more that of the man who has a real influence on what he will be given. The good inspector is not the man who is proud of the quantity of product he rejects, but the man who rarely has to reject anything because, by his economic persuasion, he sees to it that the producer is giving the right process curve.

Mr. Horsnell does not take up this aspect of the subject, indeed he says in Section 7 "If no poor quality lots are submitted to the scheme, and hence no poor lots are ever rejected by the scheme, there is no gain . . . due to the scheme, to set against the costs of operating it. In such a situation, the cost of an Acceptance Sampling Scheme will be a high price to pay for unnecessary reassurance". Is this really so? I think not. In this argument there is an implicit assumption that the probability of a poor quality lot is independent of whether the scheme is being operated or not. Let me put an analogy: In 1939 the civilian population of this country were issued with gas masks. In fact we never had a gas attack and the masks were never used. Was all the money spent upon them wasted? Not necessarily; it may well be that the fact that the population had these masks was an important factor in the enemy's decision not to use gas. If so, then the masks did their job, and did it in the best possible way. Similarly, if you are using an inspection scheme, and never, or rarely, find a defective, that inspection scheme may be doing a first-class job—it does not follow that if the scheme were changed or inspection abolished there would still be no defectives.

Turning to the way in which Mr. Horsnell's scheme would work, I can see the possibility of some heart-broken manufacturers. Suppose we have the following case:

$c_m/c_s = 0{\cdot}060$ $p_1 = 5$ per cent. Consumer's Risk $= 0{\cdot}05$

A manufacturer is putting up a process average of 2 per cent. Table 4 is the appropriate one and we find the scheme

$$n = 314,\ k = 10.$$

The manufacturer finds that 89·5 per cent. of his product is being accepted and 10·5 per cent. rejected. He decides that he cannot afford to have so much rejection and makes great efforts to improve his production and succeeds in reducing the process average to 1 per cent. $n = 314$, $k = 10$ would now give 99·8 per cent. accepted. Unfortunately, Mr. Horsnell now looks at Table 3 instead, and changes his scheme to $n = 155$, $k = 4$ and the acceptance rate is only 92·8 per cent.; hardly any better than at the start.

It seems to me that this is working in opposition to the incentives that a sampling scheme should offer to a manufacturer. One reason why I favour the type of plan in the S.R.G tables and the American Military Standard is that they work to intensify the incentive, by tightening inspection when the process average becomes bad, whereas Mr. Horsnell tightens it when the process average improves.

The following contributions were received in writing after the meeting.

Miss Maurice: I should like to question whether Mr. Horsnell's sampling schemes are in all cases the most economical.

In Section 2 it is stated that the average number of items sampled and inspected should be such that any increase in the number would not be worth the saving in extra acceptance of normal quality product. That is to say, for any increase in (n_s, n_i) the increase in the total cost of sampling would be greater than the increase in the total gain. This represents a stationary (minimum) value for the function "total cost" minus "total gain". Or, if one takes the saving in extra acceptance as a reduction in cost, a stationary (minimum) value for total cost of sampling *plus* total cost of rejecting product. This requires the latter cost to be "expressible in terms of a common unit" with the first, and this cannot be done without bringing into the cost function the loss to the producer of rejecting items, i.e. their selling price. Writing this unit price as X, the total cost to be minimized is

$$\bar{C}\,(n_s n_i) + NX(1 - \int_0^1 A(p)\,y(p)\,dp).$$

Since NX is a constant this is at a minimum when

$$\frac{d\bar{C}\,(n_s n_i)}{d(n_s n_i)} - \frac{NXd}{d(n_s n_i)}\,(\int_0^1 A\ (p)\,y(p)\,dp) = 0$$

This is not the same as Mr. Horsnell's most economical acceptance sampling scheme at the end of Section 2, which depends on the minimization of c, or (for the case of non-destructive inspection) putting

$$N\int_0^1 A(p)\,y(p)\,dp\ .\ \frac{d[\bar{C}(n_s n_i) + Nc_m]}{d(n_s n_i)} - [N\,c_m + \bar{C}(n_s n_i)]\,\frac{d}{d(n_s n_i)}\,(N\int_0^1 A(p)\,y(p)\,dp) = 0.$$

$$\frac{d\,\bar{C}(n_s n_i)}{d(n_s n_i)} - N\,c\,\frac{d}{d(n_s n_i)}\,(\int_0^1 A(p)\,y(p)\,dp) = 0$$

The two conditions are the same only if $c = X$.

This is the economists' result that, if the entrepreneur is to maximize his profit he must maximize total revenue minus total cost by producing until marginal cost is equal to marginal revenue. Marginal cost is equal to average cost when the latter is at a minimum. Production will continue beyond this point so long as marginal revenue is greater than marginal cost. Mr. Horsnell's solutions relate to the special case (of perfect competition) when marginal revenue (price) is equal to marginal cost when it is equal to minimum average cost. They may also be appropriate when the product is not for sale (e.g. the quoted case of ammunition).

It would be interesting to see solutions for cases when $X > c$ which is necessary if the producer is to make a profit. In Section 3 Mr. Horsnell has shown that c_{no} is an absolute minimum if the whole sample is inspected Such a situation would therefore always require a larger sample size than the one he has recommended.

Dr. H. C. HAMAKER: I am somewhat reluctant to comment on Mr. Horsnell's paper because I feel strongly at variance with his approach to the problem and with the results to which it has led him.

In studying the economics of any problem, we must decide what we wish to achieve, and then try to minimize the cost for a given amount of achievement. While buying materials the consumer's evident purpose is to buy effective items, not defectives. If along with the effectives, he has to accept a certain percentage of defectives as well, these will cause trouble and additional costs. To put the value of accepted defective items at zero because they are worthless, as Horsnell proposes (p. 174), is not in my opinion correct. A defective resistor going into a radio set may result in a set that does not function or does not satisfy specifications, and to find the source of the trouble and correct it may involve considerable costs. Sampling inspection is frequently carried out to avoid the troubles and costs due to accepted defectives and in an economic study of the problem it is an error to neglect these, even if it may often be impossible numerically to assess the costs associated with an accepted defective. Horsnell tries to get away from this difficulty by prescribing a consumer's risk and consumer's risk point, and then minimizing the joint costs of production and inspection per accepted item.

Now, if the cost per accepted item is the right criterion, the simple, straight-forward solution would be "no inspection"; the cost per accepted item is then the cost of production c_m and is lower than any cost under sampling inspection. But if we impose a consumer's risk and risk point, this means that we prescribe sampling inspection anyway and that the solution "no inspection" is *a priori* ruled out. This, it is to be feared, may distort the problem, and from an inspection of Horsnell's tables, I think it has done so. Table 3 in the Appendix, for example, refers to the case where

process average = 1 per cent.,
consumer's risk-point = 5 ,, ,, ,
consumer's risk = 5 ,, ,, .

The practical worker in the field will at once conclude that the level of production is so much better than the consumer's requirements that no sampling inspection is called for, certainly not when the cost of inspection is high. Yet for

$$c_m/c_s = 0\cdot003 \text{ to } 0\cdot011,$$

that is, when the cost of inspection is 100 to 300 times as large as the cost of production, Horsnell's table prescribes a sample size of 95, rejection of about 25 per cent. of the lots submitted for inspection, and a sampling procedure which would increase the cost per accepted item by a factor 2·4 to 5·1.

Coming to the bottom of the same table, I do not feel much happier. When the cost of inspection is one half to one third of the cost of production (still very high fractions compared with common industrial situations) a sample size of 314 is prescribed, and only one lot in 500 is rejected. Now industry will maintain sampling inspection only when it serves a useful purpose and does occasionally give a timely warning that something went wrong with the materials received or produced. It will very soon discontinue sampling inspection when the only reward for inspecting 300 items per lot is that on the average out of 500 lots received only one lot is rejected, which is, moreover, by one of Horsnell's basic assumptions, not appreciably different from the 499 lots accepted. It is obviously simpler and cheaper to discard sampling inspection altogether and to accept that one lot in 500 as well.

I fear that a statistician who would attempt to put these sampling schemes into practice would very soon lose his job, because his procedures would be too obviously in error.

Another unsatisfactory feature of Horsnell's theory is, in my opinion, that he does not discuss in detail what happens to rejected lots. His formulae imply, however, that rejected lots are considered a complete loss and that the costs of producing the rejected lots are charged against the accepted lots. In practice, rejected lots can usually be re-worked, screened or disposed of at a reduced price, and if that is so the theory has to be drastically revised.

These few remarks may suffice to indicate my feelings with respect to the basic assumptions underlying Horsnell's sampling schemes; I cannot accept them as reasonable.

Mr. HORSNELL (in reply): Professor Barnard has said that the process curves that he has come across in industry bear not the slightest resemblance to those in Text-table 1 of my paper. My own experience suggests that curves 7 and 8 (Beta and Gamma distributions) can be quite useful and I have found good fits to the same sample data using either curve. The other curves listed have been included not because I thought that they would arise in practice but because they have been proposed by others or are those to which mathematicians might turn as a first approximation. The discussion at the end of Section 5 (and Dr. Johnson's opening remarks) suggests that the choice of curve may not seriously affect the choice of an economical scheme, in my sense, provided that the curves have the correct process average. I agree with Miss Keen that the ranges of process averages covered are not sufficient. My own work has also at times been in the region of $p_0 = 10$ per cent. The difficulty in producing tables for p_0 greater than about 5 per cent. is that the Poisson approximation to the Binomial breaks down as p increases and the work therefore becomes cumbersome. Until the philosophical bases of the schemes are accepted, it does not seem worth while producing tables for high values of p_0 except in special cases.

I agree that this paper considerably over-simplifies the problem and I am very glad for the various practical points put by Miss Keen. Mr. Lesser and Mr. Hill, which I think will bring more reality into the subject. I do not regard this paper as the final word. Its aim is to put forward a few new ideas and to test reactions.

Mr. HORSNELL subsequently replied in writing as follows:

Perhaps the severest criticisms of my paper are contained in Dr. Hamaker's written contribution (which I am very glad to have) and I will deal with these first. Much of our apparent disagreement stems, I think, from a misunderstanding of when I would advocate the application of my schemes. I do not say, here is a set of schemes, apply them in all circumstances. Neither do I say that they should be applied without common sense and considerable thought. Both Dr. Hamaker and Mr. Lesser, it appears, would like to do away with the necessity of choosing a consumer's risk point and risk but, as Professor Barnard has pointed out, this is a very valuable degree of freedom. Used properly, it can ensure that sensible schemes are chosen, for the choice of β and p_1 determines the cost of the most economical scheme in given circumstances. In Dr. Hamaker's first example, no sensible person is going to increase the cost of a product by a factor of 2·4 to 5·1 if he does not think that a consumer's risk point of 5 per cent. and risk of 0·05 is worth the cost of the scheme in terms of abnormally bad quality lots rejected from time to time. I agree with Dr. Hamaker and disagree with Mr. Hill that in the continuous operation of an acceptance sampling scheme some lots should be rejected. If they are not, because no bad lots are ever produced, I maintain that the application of an acceptance sampling scheme on a lot-by-lot basis is a complete waste of time and money. What is required in such circumstances is a quality surveillance scheme of the "variables" type described by Mr. Lesser where such can be applied, or a deferred sentencing scheme of the "attribute" type which I have worked on in the Ministry of Supply. Dr. Hamaker says in his example that the practical worker will conclude that the level of production is so much better than the consumer's requirements that no sampling inspection is called for, certainly not when the cost of inspection is high. This surely presupposes that the *a priori* distribution of abnormally bad quality lots does not exist (and by my own and Dr. Hamaker's submission, no sampling should be done in this case), that if it does exist it is much nearer p_0 than p_1 and hence does not greatly matter, or that the costs of accepting defective items in whatever proportion they are likely to arise is not so high as the costs of operating the schemes. If any of these submissions are true, that particular value of consumer's risk point and risk should never have been specified in the first place. The consumer's risk point is not the quality that the consumer requires but the quality that he is frightened of getting. If, in fact, he is not frightened of getting bad quality product, then no consumer's risk point should be specified and the answer is, accept all product submitted. If he is really afraid, then he must pay the full price of a scheme to assuage his fears almost completely. My approach to the specification of the consumer's risk point and risk, therefore, combines vague *a priori* feelings about the distribution and proportion of abnormally bad quality lots, a qualitative assessment of the effects of accepting such product and a balance of these factors with the costs of running a scheme on normal quality product. I agree with Mr. Lesser that schemes selected in this way are not the best possible, but considering the limited amount of information which I assume to be available, provided that we do our job properly, they will be somewhere near the best possible schemes.

I am glad that Mr. Lesser and Mr. Hill have pointed out the consequences of using the proposed schemes when the product quality alters. This should certainly have been mentioned in the paper. I do not regard it as the function of the inspector to attempt (by his schemes) to effect an improvement in normal quality nor to decide what quality of product is acceptable. These problems should be dealt with by the management. It is the duty of the inspection department to keep the management and the production and buying departments informed of changes in quality and the inspector should therefore keep a running plot of his sample results. For a control-of-quality organization, in its widest sense, which approximates to what I regard as ideal, reference should be made to Mr. van Rest's article in *Applied Statistics* (1953) on Quality Control in the U.S.A. It seems possible that only in an organization where the overall control of quality is regarded as a management function, and is costed accordingly, will the rejection of any normal quality product (in order to reduce sampling costs) be regarded as a sensible thing to do. This is Mr. Hill's difficulty. He would prefer to operate a scheme which does not reject product of an improved quality rather than operate a scheme which is most economical overall. Alternatively, if the quality of a product deteriorates, it is for the management to decide whether the new quality is still temporarily acceptable. If it is, then the management will presumably want an acceptance sampling scheme which is most economical overall at the new level of p_0 whilst the management does all it can to restore the product to its previous quality.

Dealing with Mr. Hill's example, it should be remembered that reduced inspection with the S.R.G. schemes is obtained by decreasing the sample size whilst maintaining the A.Q.L. and hence the slope of the O.C. curve is lessened. It would seem reasonable to increase the consumer's risk β at p_1 if normal quality improves, but Mr. Hill has not done so. However, a comparison of the tables with $\beta = 0{\cdot}01$ and $0{\cdot}05$ suggests that $A(n, k)$ for the most economical schemes is much the same for these two values of β and hence Mr. Hill's criticisms still hold if, for example, β increased from $0{\cdot}01$ to $0{\cdot}05$ when quality improved. The incentive to improve quality does not come from the small increase in $A(n, k)$ to be expected but in the reduction in overall costs that ensues. In the example c/c_m is reduced from $1{\cdot}70$ to $1{\cdot}36$ whilst maintaining a constant β and p_1, and would be even more decreased if β was justifiably increased. This is surely a sufficient incentive in an ideal set-up. Incidentally the most economical scheme at $p_0 = 1$ per cent. defective would still encourage improved quality in the sense that less product at $p_0 = \frac{1}{2}$ per cent. defective would be rejected. The S.R.G. reduced inspection schemes accept so much product at improved quality that it is necessary to alter the A.Q.L. in order to apply an incentive to improve quality still further.

It should perhaps be pointed out that if we *are* looking for most economical schemes, then we must be prepared to divest ourselves of some of our preconceived ideas of what the schemes should or should not do, although I agree that we must only use schemes which are generally accepted to be reasonable, in our given circumstances. I have already referred to the circumstances which I regard as ideal and it seems to me that we in this country should work towards that ideal in order to reap the benefits that would undoubtedly be obtained.

Finally, I would like to deal with the criticisms of the cost function that has been used and I am particularly grateful for the contribution from Miss Maurice as well for those of Dr. Johnson and Dr. Hamaker. In formulating a cost function in a general case, one has to beware of introducing so many parameters that the argument becomes clouded by unknowns. I must admit that I chose my cost function intuitively but I am glad to see that Miss Maurice has invested it with some economic respectability. My intuition has no doubt been conditioned by my background and my idea of profits is that some arbitrary percentage is imposed on the cost of the finally accepted article, in fact just as much as the consumer will bear! To bring questions of profit into the formulation of economical acceptance sampling schemes will, I fear, complicate matters inordinately but there is no harm in someone having a try and Miss Maurice's contribution points the way. Dr. Johnson has very ingeniously suggested an alternative to the fixed consumer's risk condition but this seems to introduce more than one extra parameter and although worth pursuing in a particular case it would be difficult to treat in full generality. I have set out the philosophical bases of the cost criteria that I have adopted in the paper and these I am afraid Dr. Hamaker cannot accept as reasonable. Again I would point out that the cost function that I have adopted is the simplest that I can think of. It is a simple matter to allow for the salvage value of rejected lots in the numerator of the function, but in my opinion it is not worth introducing another parameter except in circumstances where it can be replaced by a known value. When this can be done the prospective user of a scheme may work out his own set of tables. I am afraid that my criticisms of Dr. Hamaker's criterion (Section 7) are open to misunderstanding. I meant them mainly to apply within the basic assumptions of the paper. These are that "normal" production (as I call it) is acceptable and the schemes are not required to do the almost impossible task of sorting out lots according to quality from what is assumed to be a tightly grouped distribution. In fact I assume that accepted and rejected lots of normal production differ very little in quality whatever the sample size of the scheme. In my application of Hamaker's criterion, which may be written as

$$c = \frac{Nc_m + nc_s + Nf_{da}\,D}{Nf_{ea}}$$

where D is the damage caused by an accepted defective and f_{da} and f_{ea} are the average proportions per accepted lot, of defectives and effectives respectively, in accordance with the above, I assume that the ratio f_{da}/f_{ea} is approximately constant whatever the sampling scheme adopted. Hence the addition of $Nf_{da}\, D$ in the numerator has no effect on the choice of the most economical scheme and I put D equal to zero. This is of course not legitimate when estimating the basic costs of the scheme (in terms of c/c_m), and hence the costs estimated are those *in excess* of those of accepting defective items. I assume that the operator of the scheme is willing to accept the costs of accepting defectives in order to buy the effectives that go with them. Obviously a less approximate solution to the specification problem will be obtained if the costs of defectives can be taken into account, and this should be done wherever possible.

I hope that these explanations will go some way to meeting the stimulating criticisms that have been made, for which I am most grateful.

As a result of the ballot taken during the meeting the candidates named below were elected Fellows of the Society:

Joyce Almond.
Frederick Harold Baker.
Ronald Bamberg.
Fenwick Emerson Bell.
Henri Breny.
John Stanley Chapman.
Keith Raymond Clough.
Sheila Cohen.
John Gurland.
Supan Jotikabukkana.
Peter Brian Kenny.
Francis Roy Latham.
Stella Elaine Mann.
Ewart Kingsley Read.
Wilbert Douglas Roberts.
Henry James Scullion.
Rupert Lee Sutton.
Peter Walker.

Corporate Representatives

John Roger Swann *representing* Production-Engineering Ltd.

William Douglas Wall *representing* The National Foundation for Educational Research in England and Wales.

CHANGES IN THE PROPORTIONS OF MALE COLOUR TYPES DURING THE PERIOD 1914–39

By A. J. LEA

AT a recent discussion of the Royal Statistical Society the following statement was made: "It seems clear that, in the future, studies of population trends, of migration and of the mingling of populations, of the break-up of isolates, can by no means ignore the genetic changes that are probable or inevitable concomitants". (Benjamin, *et al.*, 1955). It is suggested that the evidence here presented indicates the existence of such a genetic change in Great Britain.

The data were collected during an investigation of the hypothesis that the blond type of individual has, in general, a higher blood pressure than has the dark type. To investigate this hypothesis the eye and hair colours of ex-service men of the 1914–18 War, recorded by recruiting or discharge boards, and readings of blood pressure, recorded at medical exminations up to 1954 were examined. The data were taken from a random series of 2,288 men invalided from the forces because of injuries, as distinct from diseases. No support for this hypothesis was found, but when the proportions of the different colour types in the 1914–18 men were compared with those of a series of men from the 1939–45 war some surprising differences were noted. This 1939–45 series consisted of 5,127 cases of injury, and was prepared as a control series for investigating the possibility of an association between colour and disease (Lea, 1952). The classification of colour types used was that of MacConail (1942–43); blond = blue or grey eyes and fair hair, dark = brown eyes and dark hair, glaucope = blue or grey eyes and dark hair, cyanope = brown eyes and fair hair. For the purpose of this work, blue, blue-grey and grey eyes were classed as blue, all other as non-blue (= brown in MacConail's classification). Fair, light, blond, flaxen and light brown hair were classed as fair, all other colours including red and its varieties, as non-fair (= dark in MacConail's classification). These data are given in Table 1.

TABLE 1

Male Colour Types in the Armed Forces 1914–18 *and* 1939–45 *Wars*

(Cases of Injury)

Colour Type	1914–18 *War*		1939–45 *War*	
	Number	*Percentage*	*Number*	*Percentage*
Blond	359	15·7	977	19·1
Dark	777	34·0	1,835	35·8
Glaucope	1,068	46·7	2,063	40·2
Cyanope	84	3·7	252	4·9
Totals	2,288		5,127	

It will be seen that, in the interval between the two wars, blonds, darks and cyanopes had all increased at the expense of the glaucope type. A comparison of these over-all figures gave a value for P of much less than 0·001.

It was appreciated that the distribution of age and birthplace, factors known to affect the proportions of colour types, might differ in the two series and these points were investigated. ("Age" is here understood to refer to the age of the man at the date of recording his eye and hair colour.) The mean age in the 1914–18 series was 24·96 years, and in the 1939–45 series 23·26 years, the difference of the means being 1·70 ± 0·15 years. The differences in birthplaces were also marked; the extreme case being the large proportion of men born in Ireland in the 1914–18 series as compared with the 1939–45 series. The two series were then standardized for age and geographical distribution and compared, P again being much less than 0·001.

The question then arose, was this a change in the country as a whole or was it due to even more marked changes in certain areas? The British Isles were divided into arbitrary areas, as shown in Table 2. (These definitions are adhered to throughout this paper.)

Table 2

Birthplaces

Birthplace	*Definition*
North Scotland	All north of the line joining Edinburgh and Glasgow.
South Scotland	All south of the above line, including the two cities.
North England	The counties of Northumberland, Durham, Cumberland and Westmorland.
Lancashire	The county.
Yorkshire	The county.
Midlands	The counties of Chester, Stafford, Warwick, Worcester, Nottingham, Derby, Leicester, Northampton, Hereford and Salop.
Eastern Counties	The counties of Norfolk, Suffolk, Cambridge, Bedford, Huntingdon, Rutland and Lincoln.
London	The counties of Middlesex, Essex, Kent, Surrey, Sussex, Hertford and Buckingham.
Southern Counties	The counties of Berks, Oxford, Gloucester, Wilts, Southampton and Isle of Wight.
West of England	The counties of Somerset, Dorset, Devon and Cornwall.
Wales	The whole Principality.
Ireland	The whole island and the Isle of Man.

The two series were then compared by areas, the values of χ^2 and P being shown in Table 3.

Table 3

Comparison of Proportions of Colour Types in the 1914–18 *and* 1939–45 *Series, by Birthplaces*

(Three Degrees of Freedom in Each Case)

Area	χ^2	P	*Number of Men,* 1914–18	*Number of Men,* 1939–45
North Scotland	8·287	0·01–0·001	92	305
South Scotland	1·796	0·1–0·2	157	401
North England	0·006	0·90–0·95	149	412
Lancashire	2·279	0·1–0·2	241	537
Yorkshire	0·754	0·3–0·5	166	510
Midlands	1·650	0·1–0·2	353	633
Eastern Counties	1·279	0·2–0·3	131	238
London	19·408	Less than 0·001	551	1,054
Southern Counties	6·535	0·01–0·02	140	337
West of England	0·000	1	90	213
Wales	1·133	0·2–0·3	87	314
Ireland	0·658	0·3–0·5	131	173

It was evident that the change in proportions of the colour types was significant only in the areas of London and North Scotland, with the possibility of a significant change in the Southern Counties. The data in these areas were then subdivided into age groups, as shown in Tables 4, 5 and 6. It will be seen that in each of these areas, at every age group, there has been a decrease in the proportion of glaucopes, and the figures for "all ages" show that the other three types have increased.

TABLE 4

Men Born in the Area of London
Colour Type by 5-year Age Groups, 1914–18 *and* 1939–45 *Series of Injuries*

Age Group		*Blond*	*Dark*	*Glaucope*	*Cyanope*	*Totals*
15–19 years	*1914–18*	12	17	29 (45·3)	6	64
	1939–45	80	95	90 (31·3)	23	288
20–24 years	*1914–18*	33	92	108 (43·5)	15	248
	1939–45	79	153	113 (31·1)	18	363
25–29 years	*1914–18*	32	49	65 (44·5)	0	146
	1939–45	31	82	85 (39·7)	16	214
30 years and over . .	*1914–18*	16	27	46 (49·5)	4	93
	1939–45	36	71	68 (36·0)	14	189
All ages	*1914–18*	93 (16·9)	185 (33·6)	248 (45·0)	25 (4·5)	551
	1939–45	226 (21·4)	401 (38·0)	356 (33·8)	71 (6·7)	1,054

(Figures in brackets are percentages)

TABLE 5

Men Born in the Area of the Southern Counties
Colour Types by 5-year Age Groups, 1914–18 *and* 1939–45 *Series of Injuries*

Age Group		*Blond*	*Dark*	*Glaucope*	*Cyanope*	*Totals*
15–19 years	*1914–18*	3	2	9 (64·3)	0	14
	1939–45	33	50	48 (34·0)	10	141
20–24 years	*1914–18*	10	18	36 (56·3)	0	64
	1939–45	19	37	42 (41·2)	4	102
25–29 years	*1914–18*	4	23	11 (28·2)	1	39
	1939–45	12	26	13 (25·0)	1	52
30 years and over . .	*1914–18*	4	7	11 (47·8)	1	23
	1939–45	5	18	16 (38·1)	3	42
All ages	*1914–18*	21 (15·0)	50 (35·7)	67 (47·9)	2 (1·4)	140
	1939–45	69 (20·5)	131 (38·9)	119 (35·3)	18 (5·3)	337

(Figures in brackets are percentages)

TABLE 6

Men Born in the Area of North Scotland
Colour Types by 5-year Age Groups, 1914–18 *and* 1939–45 *Series of Injuries*

Age Group		*Blond*	*Dark*	*Glaucope*	*Cyanope*	*Totals*
15–19 years	*1914–18*	0	1	5 (83·3)	0	6
	1939–45	26	27	41 (40·2)	8	102
20–24 years	*1914–18*	7	7	22 (55·0)	4	40
	1939–45	24	31	55 (47·4)	6	116
25–29 years	*1914–18*	2	7	20 (69·0)	0	29
	1939–45	7	12	22 (50·0)	3	44
30 years and over . .	*1914–18*	1	5	11 (64·7)	0	17
	1939–45	3	16	22 (51·2)	2	43
All ages	*1914–18*	10 (10·9)	20 (21·7)	58 (63·0)	4 (4·3)	92
	1939–45	60 (19·7)	86 (28·2)	140 (45·9)	19 (6·2)	305

(Figures in brackets are percentages)

A method of analysis capable of dealing with the necessary subdivision into age groups was suggested by Dr. C. A. B. Smith of the Galton Laboratory, University College, London. Let x_a = number of glaucopes in one age group in the 1914–18 series, y_a = number of other colour types in the same group, and $n_a = x_a + y_a$. Let x_b = number of glaucopes in the same age group in the 1939–45 series, y_b = number of other colour types in the same group, and $n_b = x_b + y_b$. A table for each age group may then be constructed:

	Glaucope	*Non-glaucope*	*Totals*
1914–18 series	x_a	y_a	n_a
1939–45 series	x_b	y	n_b
Totals	X	Y	N

Then the difference in the proportions of glaucopes in the two series $= \frac{x_a}{n_a} - \frac{x_b}{n_b}$, and the variance $= \frac{XY}{(N-1)\, n_a\, n_b}$. This is repeated for each age group, and the results summed. The standard error is given by the square root of the sum of the variances. These results follow from consideration of the moments of the well-known Fisher-Irwin-Yates exact distribution for 2 × 2 tables.

It was found that the difference in the proportions of glaucopes in the two series, by 5-year age groups, was

London	$0 \cdot 4476 \pm 0 \cdot 1123$, 4·0 times the standard error.
Southern Counties . .	$0 \cdot 5826 \pm 0 \cdot 2241$, 2·6 times the standard error.
North Scotland . . .	$0 \cdot 8324 \pm 0 \cdot 2956$, 2·8 times the standard error.

Eye and hair colour were then investigated separately. For all ages, all areas, the comparison of blue with non-blue eyes gave $\chi^2 = 6 \cdot 243$, P being between 0·01 and 0·02. For fair and non-fair hair, all ages, all areas, $\chi^2 = 19 \cdot 235$, P being less than 0·001. The data for these characters were then broken down into areas of birthplace, as previously defined, and the two series again compared. The results are shown in Table 7.

TABLE 7

Comparison of the Proportions of Eye and Hair Colours in the 1914–18 *and* 1939–45 *Series, all Ages, by Birthplaces*

(1 degree of freedom in each case)

Birthplace	χ^2 *Eyes*	*P, Eyes*	χ^2 *Hair*	*P, Hair*
North Scotland	2·245	0·1–0·2	4·550	0·02–0·05
South Scotland	2·749	0·05–0·1	0·106	0·7–0·8
North England	2·008	0·1–0·2	1·047	0·3–0·5
Lancashire	0·881	0·3–0·5	0·735	0·3–0·5
Yorkshire	0·404	0·5–0·7	4·006	0·02–0·05
Midlands	0·266	0·5–0·7	4·122	0·02–0·05
Eastern Counties	0·577	0·3–0·5	3·485	0·05–0·1
London	6·569	0·01–0·02	8·652	Less than 0·01
Southern Counties	1·984	0·1–0·2	4·928	0·02–0·05
West of England	0·618	0·3–0·5	0·131	0·7–0·8
Wales	0·102	0·7–0·8	2·229	0·05–0·1
Ireland	0·136	0·7–0·8	0·610	0·3–0·5

It was found that eye colour showed only a possibly significant change in the London area but that hair colour showed a significant change in this area and possibly significant changes in the areas, North Scotland, Yorkshire, Midlands and Southern Counties. In each of these areas the change was an increase in the proportion of fair hair. Finally the data for the London area were divided into 5-year age groups, and the change in the proportion of fair hair calculated by the method used for the estimation of the change in glaucopes. The difference in the proportion of fair hair in the two series was found to be $0 \cdot 1999 \pm 0 \cdot 1025$, which is not significant. As it was not possible to show a significant increase in the blond type, but only a highly significant decrease in glaucopes, it was not surprising that the result for fair hair was not significant. However, the result showed a marked trend in the direction of increasing fairness.

These results are interpreted to mean that during the period 1914–18 to 1939–45, one generation, there has been a significant decrease in the proportion of glaucopes in the male population born in the areas London, Southern Counties and North Scotland, and that probably the same change has occurred on a smaller scale throughout England and Wales generally.

The most obvious explanation, changes in the definitions of eye and hair colours during this period, becomes highly improbable when it is realized that the effects of such changes would have to be limited to men born in certain areas only, yet examined by many different examiners scattered throughout the country. The records of eye and hair colour were made by persons who cannot be thought of as having any special interest in these characters. No guidance was given to these persons on the interpretation of such terms as "hazel" or "mid-brown", and it is certain that the records reflect the views of individual examiners as opposed to a concerted plan of action. It would undoubtedly be absurd to discuss any fine degrees of colour variation, for example, blue, grey-blue and grey eyes, on such evidence. However, certain collateral evidence exists which is thought sufficiently strong to justify the use of these records for discussion of differences between blue and non-blue eyes, and fair and dark hair. This additional evidence is:—

(1) It is known that the proportion of dark hair in the population increases with advancing age (Steggerda, 1941). Both the 1914–18 and the 1939–45 series show this change. Figs. 1 and 2 show the data for fair hair and the blond type respectively, in 5-year age groups, for both series.

(2) It is known that the neoplasm melanoma has a higher incidence in blonds than in the other colour types. Forty-one cases of this tumour from the 1939–45 war, in which the eye and hair colours were recorded, have been collected from the records of the Ministry of Pensions and National Insurance. Of these 41 cases 18 had fair hair, 23 non-fair, and there were 15 blonds and 26 non-blonds. Comparison of these cases with the 1939–45 series of injuries gave, for fair versus non-fair hair, $\chi^2 = 8\cdot807$, and for blond versus other colour types $\chi^2 = 8\cdot553$. With 1 degree of freedom P was less than $0\cdot01$ in each case. This was in fact an underestimate of the difference as the mean age of these 41 cases at the date of recording the data was 29·46 years, whereas the mean age of the injury series was 25·26 years. This produced a bias in the direction of a decrease in blonds and fair hair in the melanoma series. It, therefore, seemed reasonable to assume that the data were sufficiently accurate for making the distinction between fair and non-fair hair, and blonds and non-blonds.

The mean year of birth of the 1914–18 series was 1891, for the 1939–45 series 1919, a difference of 28 years. It seems that the observed changes are those of one generation. It must be emphasized that the men were born in the areas stated, so that there is no possibility of the changes being due to their migration. On the other hand it seems that the most probable explanation of these changes is migration of the parents of the 1939–45 series, i.e. in the years between the mid points 1891 and 1919. Unfortunately no evidence has been found, for either national or international migration during this period, which allows any conclusions to be drawn about the colour types of the migrants. It is certainly true that the majority of migrants tended to settle in or around London, so that changes such as those described here would be expected to occur in that area. There is also evidence of some interchange of population between North East Scotland and Scandinavia.

If such genetic changes are occurring, then it is reasonable to expect that there must be greater changes in the incidence of other hereditary characters in these areas than in the rest of the country. It was hoped that additional evidence on this point might be provided by a study of the Registrar General's Annual Statistical Reviews. These Reviews do not give incidence rates, but it was thought that death rates might prove a reliable guide. The attempt, however, proved to be a failure. In the case of diabetes melitus, a disease with a known hereditary basis, modern treatment has so revolutionized the prognosis that the recorded deaths from this condition no longer provide a reliable estimate of its incidence. In cirrhosis of the liver, where a hereditary basis has been postulated (Lea, 1952), one of the most important environmental factors, the consumption of alcohol, has changed so much during the period under investigation that comparisons are not

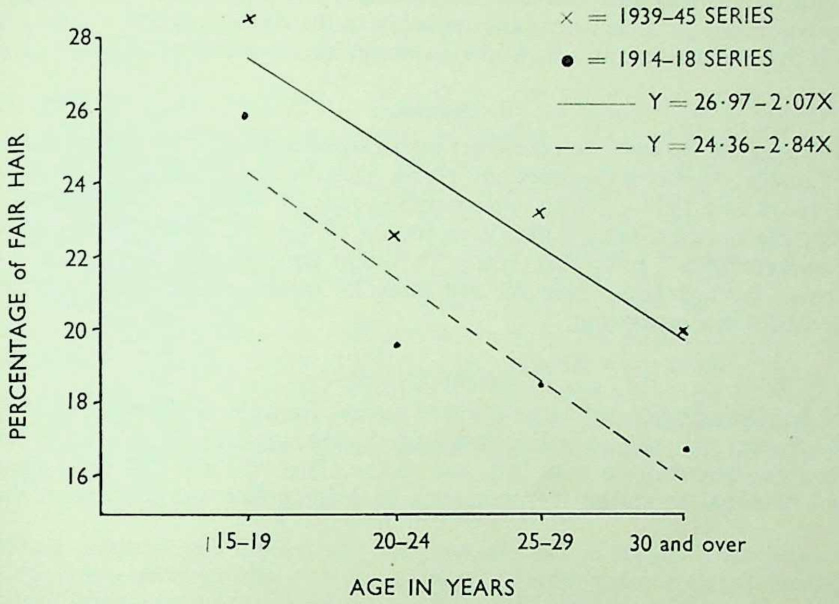

FIG. 1.—Variation in proportion of fair hair with age.

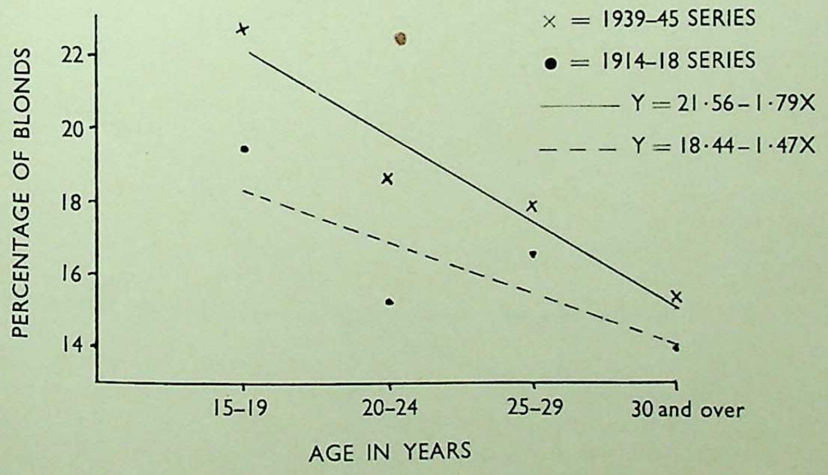

FIG. 2.—Variation in proportion of the blond type with age.

valid. In certain rarer diseases, still practically unaffected by treatment and in which environmental factors are minimal and the death rates could therefore provide reliable information on the incidence, e.g. haemophilia, porphyria, the records are unobtainable for the years before 1940.

The practical importance of such changes in eye and hair colours lies in the speed with which they have occurred. If it is accepted that these changes are real, then it follows that any survey of hereditary characters in these areas, and probably in the country generally, has a very limited useful life. If these trends continue such surveys would have to be repeated about every 20 years.

Summary

Evidence is presented to show that there has been a significant fall in the proportion of glaucopes born in the London, Southern Counties and North Scottish areas during the period between the two wars, 1914–18 and 1939–45, with corresponding rises in the three other colour types. It is suggested that the most probable cause is migration of the parent generation, and that other hereditary "abnormalities", using this term in its widest sense, may have an altered incidence in the same areas. Such changes limit the usefulness of large-scale genetical surveys of human characters to about one generation.

Acknowledgments

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A METHOD OF ESTIMATING SURPLUS LABOUR IN PEASANT AGRICULTURE IN OVER-POPULATED UNDER-DEVELOPED COUNTRIES

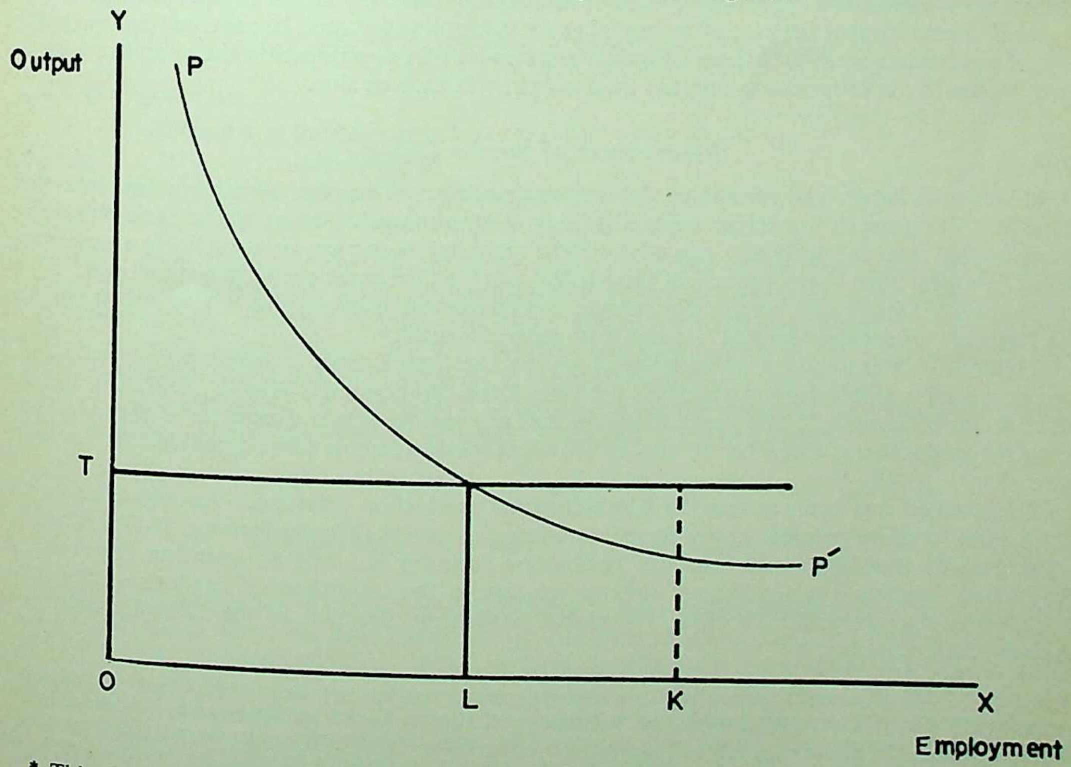
By N. K. SARKAR

MANY of the under-developed countries are characterized by a stagnant economic structure within which has occurred in recent years a rapid rise in population. The result has been a heavy overcrowding in peasant agriculture. Economists have shown how such overcrowding acts as a drag on agricultural progress. For example, the pressure of population on land is directly responsible for the fragmentation of land, the uneconomic size of holdings, the reduction in fertility due to the difficulty of crop rotation and introduction of better farming technique, reduction in agricultural surplus, indebtedness, absentee landlordism and many other evils which now vitiate farming in these countries (Ghosh, 1946, p. 29; Lewis, 1955, p. 326). Many authors regard the removal of this surplus population from agriculture as one of the most important conditions for achieving agricultural progress. (Datta, 1952, p. 125; Mandelbaum, 1955, p. 3.)

It is important, therefore, to devise a measure for estimating the extent of "labour surplus" that prevails in these countries. Such a measure will not only have a theoretical interest but will provide a sound basis for integrating agricultural development effort with other economic development projects.

Definition of Surplus Labour

Surplus labour exists if the value of the marginal product of labour is less than the rate of wages paid. The amount of surplus is to be measured by the excess of actual employment over the employment which equates marginal productivity and the wage rate.*



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* This definition is due to Professor R. G. D. Allen.

Thus if *PP′* be the marginal productivity curve of labour and *OT* the wage rate, then employment of labour up to *OL* is economically justifiable. If employment is carried on beyond this point to any point *K* then *LK* is the volume of surplus labour.

If production is carried on by wage-labour, it is unlikely that employment will ever be pushed beyond the marginal productivity level. There are, of course, instances where many non-economic considerations prevent the employer from dismissing labourers even when their contribution to the total output is less than they receive. However, employment beyond the point of marginal worthiness of labourers is not likely to continue over the long period or even for the short period beyond a certain limit. In general, it is true to say that when wage-labour is employed the volume of surplus labour will be more or less identical with that of unemployed labour. Measuring the volume of surplus labour under this condition resolves then into the problem of assessing the volume of rural unemployment.

In many underdeveloped countries, however, the mode of employment in peasant agriculture is not wage-labour but labour of family members who do not receive any wages but share in the common family pool; and community labour where the peasants form into groups (known in Ceylon as "*Attam*") for mutual aid, working in each other's fields. This is the major form of labour employment in paddy cultivation in Ceylon. The person on whose land the mutual-aid labour gang works on a particular day has to provide the morning and the mid-day meal for the labourers. He is not required to make any other payment in cash but is expected to work in the neighbours' fields for an equal number of days. The system probably originated in the early days owing to labour shortage. To-day it persists for opposite reasons, namely shortage of land and working capital. To-day the labour supply is high relative to land and the alternative occupations are scarce, so that the idle labour joins in the cultivation because it has nothing else to do. If the peasant had enough working capital and his hands were full with jobs to be done on his own land, he would have hired labourers on a wage basis instead of inviting an unlimited number of persons who form the mutual-aid labour-gang to work in his field.

It should be noted that the temporary unemployment of agricultural labour due to the seasonal character of agricultural production does not enter into our definition of surplus labour. In seasonal unemployment the cost of maintaining the unemployed should be regarded as a necessary cost of production because if these labourers are removed from production their services will not be available in the busy season and the total output will thus decline.

Measurement of Surplus Labour

Under both family and community labour, measurement of surplus labour becomes extremely difficult. Everyone in the village lends a helping hand to some or other of the agricultural processes so that there is hardly any one who can be regarded as surplus or completely unemployed. When everyone joins in the process of production everyone becomes partially unemployed. Thus in the case of family and community labour, the phenomenon of surplus labour shows itself in the form of "under-employment" rather than unemployment.*

Though the phenomenon of under-employment has been known to economists over the last two decades no satisfactory method has yet been found to measure it (Datta, 1952, Ch. 5). An attempt has been made in this paper to bridge that gap and to obtain a measure of surplus labour in paddy production in a number of villages in Ceylon where a sample survey provided the necessary data.

The method has been to use the Cobb-Douglas production function. An objection to the production function pointed out by Bronfenbrenner and others (Bronfenbrenner, 1944; Mendershausen, 1938) is that it is an interfirm production function and not an intrafirm function. A production function has significance only in relation to the conditions of production and combinations of factors within the firm. A global production function of the Cobb-Douglas type has very little meaning.

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* In addition to unemployment and under-employment, surplus population may lead to "disguised unemployment". For example, people may sell matches or run tea-kiosks, sell balloons or become domestic servants. Such persons may appear fully employed in form, though they must be regarded as under-employed. The volume of disguised unemployment, however, is likely to be small in the rural areas, the scope of this kind of business being very small.

In spite of this very serious limitation, the Cobb-Douglas function has been used in the present study for two reasons: firstly, the technique of cultivation in Ceylon, as in many Asian countries, is ancient and traditional with very little variation; the ratio of combination of factors and the procedure followed varies little from farm to farm, and in this situation the global production function may be regarded as accurately reflecting the conditions of production of each of the farms. Secondly, an intra-farm study can be made only if data for each of the farms over a long period of time are available. In most of these countries no dependable time series are available on any aspect of peasant farming. Hence our computation was limited to a cross-section study based on data collected by the Economics Department of the University of Ceylon in a sample survey.

Nature of the Data

This survey was conducted in a subdivision of a district in which the peasant problem was acute. A multi-stage stratified random sample of nine villages was taken. Information on paddy cultivation was collected and the data were tabulated under the following heads: size of holding; labour-days spent; and non-labour cost. Size of holding was measured in acres; a unit of labour-day was defined as eight hours' work by males and sixteen by females. The wage differential that exists at present in the neighbouring estates was used to determine this ratio between male and female labour. The non-labour cost was measured in money units and consisted of items such as food provided to the neighbours who worked on a mutual-aid basis, manure, seeds, rent, taxes and water rates, etc.

Regression Equation

A Cobb-Douglas function was fitted to the observed data by the least-square method. The equation obtained was:

$$y = 0\cdot0678\, x_1 + 0\cdot6204\, x_2 + 0\cdot1599\, x_3 + \bar{1}\cdot988345 \quad . \quad . \quad . \quad (1)$$

A weighted regression equation, obtained by the method described in the Appendix was:

$$y = 0\cdot0802\, x_1 + 0\cdot5570\, x_2 + 0\cdot2453\, x_3 + \bar{1}\cdot985901 \quad . \quad . \quad . \quad (2)$$

In these equations:

x_1 stands for log of land in acres;
x_2 ,, ,, ,, ,, labour-days;
x_3 ,, ,, ,, ,, non-labour cost in rupees;
y ,, ,, ,, ,, output in bushel.

Though these two equations do not differ much from each other, the weighted regression equation has a better rational for acceptance because it takes into consideration the error-variances of the observations, and also because it is better suited for measuring structural relationships which is our main concern here. We will therefore use equation (2) in our subsequent analysis as representing the conditions of production in paddy cultivation in the sample.

Differentiating equation (2) with respect to labour we get the marginal productivity of labour. Thus if R is the marginal productivity of labour measured in bushels, then

$$\log R = 0\cdot0802\, x_1 + 0\cdot2453\, x_3 + \log 0\cdot5570 - 0\cdot4430\, x_2 + \bar{1}\cdot985901$$

$$\therefore \quad x_2 = 0\cdot1810\, x_1 + 0\cdot5537\, x_3 + \bar{1}\cdot3944842 - \frac{\log R}{0\cdot4430} \quad . \quad . \quad . \quad (3)$$

Equation (3) will thus give us the required number of labour-days which under the given conditions of production should be employed in a typical farm of size x_1 and in which x_3 amount of non-labour cost has been incurred. If in any farm more labour is employed that labour could be regarded as surplus.

In equation (3), the value of x_2, it will be noticed, will depend on the assumed value of R. Under conditions of wage-labour, R can be taken to be equal to the prevailing wage rate. When production is carried on by family or community labour R has to be arbitrarily assumed. When labour supply is excessive and is increasing, with the avenues of employment remaining limited and non-expanding or expanding at a slower rate, the marginal productivity is usually assumed to be tending towards the subsistence level (Lewis, 1954). The concept of subsistence level, however, does not lend itself readily to statistical measurement and no two authorities will perhaps agree as to what constitutes it. In fact, as has been observed during times of scarcity and famine, human beings can subsist for a considerable time on food which at normal times would be regarded as grossly inadequate. This ambiguity about the subsistence level is rather unfortunate because, as happens here, the marginal productivity curve may have only a very slow rate of decline, in which case a small difference in the subsistence level will give widely varying figures of surplus labour.

A way out of this difficulty may be suggested by regarding the subsistence level as the standard of living of the lowest income group at a particular point of time. In many under-developed countries family budgets of the lower income groups have by now been collected and it is thus possible to obtain data on their consumption level. For example, a family budget survey conducted by the Central Bank of Ceylon in 1953 gives detailed information on *per capita* expenditure by the lowest income group with a monthly income of 0–50 rupees. The total expenditure per head by this group was given as Rs. 17.80 per month. The number of persons per consumption unit (which is more or less the same as households in rural areas) was 4·77 and the number of earners was 1·56. Since all members of the family except the small children help in paddy cultivation, it is reasonable to take the number of earners for the paddy cultivators to be higher than for the rest of the families in Ceylon. We will assume it to be 2. The number of working days in the year can be taken as 268, omitting the 52 Sundays and 45 holidays in the 365 days. Thus each earner must earn Rs. 1.90 per day to maintain the family at the subsistence level. We can take R to be equal to Rs. 1.90, or, in terms of marginal physical productivity, 0.19 bushels of paddy, the price of paddy being taken as Rs. 10/- per bushel.

Equation (3) then reduces to:

$$x_2 = 1{\cdot}02258 + 0{\cdot}1810\, x_1 + 0{\cdot}5537\, x_3 \quad . \quad . \quad . \quad . \qquad (4)$$

Substituting in equation (4) the average value of x_3 for the corresponding value of x_1, we can obtain the optimum number of labour-days which must be used under the conditions of production typified by equation (2).

The difference between the actual number employed and the optimum number would then give us the surplus labour employed in that holding. In the table below the actual number and the surplus number of labour-days for different sizes of land are given. The ratio of the total surplus to the total labour-days actually employed will give us an overall index of labour wastage.

Surplus Labour in Paddy Cultivation

Size of Land Holding (Acres)	*Number of Land-holdings*	*Actual Labour-days Employed*	*Surplus Labour-days*	*Percentage of Surplus Labour-days to Actual Labour-days Employed*
Less than 0·25	3	136·50	43·50	31·68
·25 and less than ·75	48	3558·25	1018·25	28·62
·75 ,, ,, ,, 1·25	17	2029·00	603·00	29·72
1·25 ,, ,, ,, 2·00	6	855·00	255·00	29·82
2·00 ,, over	6	1252·50	305·50	24·39
	80	7831·25	2225·25	28·42

It will be observed that the proportion of surplus labour employed remains much the same for all land-holdings of less than two acres. For larger plots it is substantially less, as is to be expected. The average proportion of surplus labour is 28·42 per cent.

Our conclusion is, therefore, that even under the given technique, 28 per cent. of the labour-days now employed in paddy cultivation is superfluous.

APPENDIX

Method of Weighted Regression

The problems involved in obtaining a structural equation of the type we have used in our study have been discussed in great detail in the references given below. It is now generally admitted that the weighted regression gives a better estimate of the structural relationships than the regression obtained by the simple least-square method.

Many methods have been suggested for obtaining the weighted regression equation. We have closely followed that given by Tintner (1952) which requires first of all an estimate of the error-variances of the variates under study. Tintner suggests the use of the variate-difference method for obtaining the error-variances. This method, however, has serious limitations, as pointed out by Kendall. In our application it suffers from two further limitations. First, it can be applied with greater facility in time-series studies, where observations are taken at an equal interval. One of the variates is thus errorless and equidistant in observation, while all other variates are measured in reference to it. In cross-section studies no such errorless variable is available; neither are the observations placed at an equal interval. In our study we have taken the size of land-holding as the errorless variable with reference to which the other variates, namely, the labour input, the non-labour cost and the output have been measured. The size of the land-holding cannot be regarded as absolutely errorless as time, but certainly its error-variance can be regarded as small compared with those of the other variates.

Our second difficulty in applying the variate-difference method is that the sizes of land-holdings do not fall at an equal interval as the variable time does in the case of time series. The value of the other variates has to be obtained by interpolation, therefore, if this method is to be applied at all. We have followed the simplest method of linear interpolation between two successive observations in order to obtain a complete series of values of a variate with reference to the size of land-holdings at an equal interval. It will be apparent that by the process of interpolation we have eliminated some of the variances and thus have probably obtained an underestimate of the error-variance. Since, however, we are interested only in the relative value of the variances this under-estimation should leave our equation unaffected.

The third difficulty that we meet with in applying the variate-difference method to cross-section data is the presence of multiple observations at the same point in the reference variate. In our case, for example, a number of peasants may have the same size of land-holding. This difficulty can, however, be easily solved by using a simple relation between the variance of a series and the variances of its components given by Yule and Kendall (1937, p. 143).

Once the error-variance is found the rest of the procedure is simple as has been detailed, for example, in Tintner.

Needless to say, we have ignored the problems of multicollinearity and identification in our computation, and we have not attempted to determine the confidence limits to our estimates.

The values of various parameters needed in our computation are given below:

The Variance-Co-Variance Matrix

	x_1	x_2	x_3	y
x_1	11·3537	−33·0213	−15·0748	−21·1807
x_2		303·9928	236·0771	223·7252
x_3			185·4317	175·2978
y				167·6209

The smallest latent root = 45·58.
The weighted regression equation is:

$$y = 0\cdot0802\, x_1 + 0\cdot5570\, x_2 + 0\cdot2453\, x_3 + \bar{1}\cdot985901$$

Acknowledgment

I am grateful to the Ceylon University for the financial grant that made this work possible and to Professor R. G. D. Allen for his suggestions and criticisms.

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UNIT VALUE AND VOLUME INDEX NUMBERS OF INTER-AREA TRADE

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I

DISCUSSION of the alternative methods available for the calculation of unit value and volume index numbers for imports and exports, and of changes in the terms of trade, are well-known features of the literature on international trade statistics. A recent authoritative exposition of the problems involved is given by Allen (1953). What do not appear to have been discussed so far are the alternative methods which can be used to compute the corresponding unit value and volume index numbers for trade with particular countries or areas.

Such indices are required for consideration of changes in the trade and payments relations of different areas. A special application is the derivation of changes in inter-area terms of trade. Until recently, there were few published unit value and volume series for trade between different areas, and all of these related to the post-war period only. Some notable examples are the series published by the Board of Trade for price and volume of United Kingdom trade with the Dollar Area and the Sterling Area, and of Sterling Area trade with the non-Sterling world.

The publication of a new work by Kindleberger (1956) on changes in Europe's terms of trade has drastically altered this position. Professor Kindleberger and his assistants have produced what is undoubtedly a classic work in the literature on this subject. They present, for the first time, unit value index numbers (for imports, exports and the terms of trade) for each of the main industrial countries of Europe in their trade with different areas of the world. The areas distinguished generally follow those used by Hilgerdt (1944). They are: Industrial Europe; Other Europe; the United States; Areas of Recent Settlement; and the Rest of the World. It should be noted that the countries included in "Rest of the World" include Japan as well as "underdeveloped" countries. Kindleberger does not present the corresponding volume index numbers, but these can, in fact, be derived in the usual way by deflating the values of imports and exports (also given by Kindleberger) by the relevant unit value series. The years chosen for the calculation were 1872, 1900, 1913, 1928, 1938, and 1952. Some of these were years of greater prosperity than others. In particular, it would seem that 1952 is rather "out of line" in view of the recession in world trade in textiles and other consumer goods in that year. It was no doubt the latest year for which figures were available.

The interesting, and often illuminating, sections of Professor Kindleberger's work, in which he tests various economic theories of the terms of trade by the use of his long-period series clearly depend largely on the statistical techniques that he uses to derive these series. It is, therefore, of some importance to examine these techniques in relation to the problem as it presented itself to him. In the following section his method is discussed in relation to a possible alternative method. Since all Kindleberger's index numbers of unit values are of the fixed-base current-weighted type, the following discussion is confined for simplicity of exposition to this form of index; similar comparisons could be made for other types of unit value index numbers but the conclusions would not be affected.

II

Index numbers of unit value and volume for trade between a given country and the rest of the world can be expressed, in the usual way, as follows:—

$$\text{Unit value index} = \frac{\Sigma\,(p_1 q_1)}{\Sigma\,(p_0 q_1)} \qquad \text{Volume index} = \frac{\Sigma\,(p_0 q_1)}{\Sigma\,(p_0 q_0)} \quad . \quad . \quad (1)$$

where p relates to unit value (price), and q to quantities, of individual commodities traded, and the subscripts 0 and 1 to two periods of time.

As given in formula (1), the unit value index has current weights, the volume index base weights, and the two are "compatible" in the sense that by multiplying them together we derive an index of total value, i.e.

$$\frac{\Sigma (p_1 q_1)}{\Sigma (p_0 q_0)}.$$

In practice, this calculation has to be done over the whole range of commodities traded, so that even without an area sub-division it represents a considerable amount of statistical calculation for any given country.

Now, suppose that we require an area sub-division in addition to the commodity sub-division. We then have a matrix, for any given period, of the exports from one country, showing the quantities and values of each commodity exported to each area. A similar matrix can be constructed for imports. The remainder of the discussion in this note relates to exports, but generally speaking the same points apply to both exports and imports. For the typical i^{th} commodity exported to the j^{th} area, we can derive a unit value index

$$\frac{p_{1_{ij}}}{p_{0_{ij}}}$$

and by treating exports to that area as if they constituted the universe, we can derive a unit value index for all exports to that area simply by applying formula (1) as follows:

$$\left.\begin{aligned} \text{Unit value index for exports to } j &= \frac{\Sigma (p_{1_{ij}} q_{1_{ij}})}{\Sigma (p_{0_{ij}} q_{1_{ij}})} \\ \text{Volume index for exports to } j &= \frac{\Sigma (p_{0_{ij}} q_{1_{ij}})}{\Sigma (p_{0_{ij}} q_{0_{ij}})} \end{aligned}\right\} \quad . \quad . \quad . \quad (2)$$

For the present, let us assume that formula (2) provides the "best" answer to the problem of how to measure changes in unit values and volume for trade with a given area on the basis of the fixed-base forms used in formula (1), simply because it takes into account the fullest commodity detail available. (This assumption is discussed in some detail later on.) To compute index numbers by area on this basis therefore requires the computation of separate unit values for *each* commodity to *each* area chosen for the analysis. Professor Kindleberger had chosen 5 areas, 6 years, and 8 exporting countries, while for each country there were many hundreds of commodity headings to be separately treated. The number of separate unit value computations would therefore have been in the hundreds of thousands had Kindleberger chosen to apply formula (2) *in extenso*.

For most practical purposes, the volume of computations involved in applying a detailed formula such as (2) would be prohibitive in terms of time and cost. Kindleberger's investigation proved no exception, and he found himself compelled to take short-cuts so as to complete his study within a reasonable period of time. Essentially, his short-cut method consists of aggregating commodity-headings wherever this seemed justifiable, and treating the aggregate as if it were a single homogeneous commodity.

To make the discussion more concrete at this point, let us consider the exports of cotton piece-goods. In the United Kingdom export statistics, to take one example, 10 separate types of cotton piece-goods are distinguished in the monthly *Trade Accounts*, so that by aggregation of these, Kindleberger reduced his statistical computations by roughly ten-fold. Using our previous notation, the Kindleberger index for exports of cotton piece-goods to j can be expressed as:

$$\text{Unit value index} = \frac{P_{1_j} Q_{1_j}}{P_{0_j} Q_{1_j}} \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

where the capital P's and Q's relate to cotton piece-goods as a whole. Thus,

$$P_{0j} = \frac{\Sigma (p_{0_{ij}} q_{0_{ij}})}{Q_{0_j}} \text{ where } Q_{0_j} = \Sigma (q_{0_{ij}})$$

and

$$P_{1_j} = \frac{\Sigma(p_{1_{ij}}q_{1_{ij}})}{Q_{1_j}} \text{ where } Q_{1_j} = \Sigma(q_{1_{ij}})$$

so that (3) can be re-stated as—

$$\text{Unit value index} = \frac{\Sigma(p_{1_{ij}}q_{1_{ij}})}{\Sigma(p_{0_{ij}}q_{0_{ij}})} \cdot \frac{Q_{0_j}}{Q_{1_j}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

Similarly, the "compatible" volume index would be:

$$\text{Volume index} = \frac{P_{0_j}Q_{1_j}}{P_{0_j}Q_{0_j}}$$

which reduces to

$$\frac{Q_{1_j}}{Q_{0_j}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

i.e. the volume index is really a ratio of quantities.

This method of "commodity-aggregation" is widely used in the Kindleberger investigation. A notable example is the use of the aggregate figures for machinery as a whole in the calculation of the unit value index numbers for the United Kingdom, France, Germany and Italy.

The validity of this method depends on the validity of the implicit assumptions of (1) zero variance in the quantity relatives

$$\frac{q_{1_{ij}}}{q_{0_{ij}}}$$

of the items which are aggregated, and (2) equal base prices per unit of the aggregated items. Where changes in the volume of trade have taken place over a period of years, considerable dispersion of quantity relatives will be a normal occurrence and the commodity-aggregation method is likely to produce misleading results.

The point can perhaps be seen more clearly if we assume that the unit value index numbers for each item aggregated are all equal. For each item, then, $p_1 = \pi p_0$, where π is the unit value index. On the basis of the assumption made, any valid aggregation formula should produce a unit value index equal to π. Formula (2), for example, gives the following result:

$$\frac{\Sigma(p_{1_{ij}}q_{1_{ij}})}{\Sigma(p_{0_{ij}}q_{1_{ij}})} = \pi \cdot \frac{\Sigma(p_{0_{ij}}q_{1_{ij}})}{\Sigma(p_{0_{ij}}q_{1_{ij}})} = \pi$$

The Kindleberger formula (4), however, gives a different result:

$$\frac{\Sigma(p_{1_{ij}}q_{1_{ij}})}{\Sigma(p_{0_{ij}}q_{0_{ij}})} \cdot \frac{Q_{0_j}}{Q_{1_j}} = \pi \cdot \frac{\Sigma(p_{0_{ij}}q_{1_{ij}})}{\Sigma(p_{0_{ij}}q_{0_{ij}})} \cdot \frac{Q_{0_j}}{Q_{1_j}} \quad . \quad . \quad . \quad . \quad . \quad (6)$$

$$= \pi \cdot \frac{\text{volume index}}{\text{weight index}}$$

The volume and weight index numbers will diverge* whenever the price per unit in the base period (in this case p_{0ij}) differs as between the items being aggregated, *and* there is some dispersion in the quantity relatives

$$\frac{q_{1_{ij}}}{q_{0_{ij}}}.$$

If the base price per unit is the same for all items, (6) becomes:

$$\pi \cdot \frac{p_{0_{ij}}\,\Sigma(q_{1_{ij}})}{p_{0_{ij}}\,\Sigma(q_{0_{ij}})} \cdot \frac{Q_{0_j}}{Q_{1_j}} = \pi$$

---

* See Maizels (1949) for further discussion of the relationship between weight and volume index numbers.

while a similar result is achieved if the quantity relatives are all equal, even though base prices are different. It can readily be seen also that a dispersion in the unit value index numbers of the aggregated commodities does *not* influence the result of formula (4) or formula (5).

Over a long period of time, the assumption that the quantities of individual items traded with a particular area all change to the same extent is most unlikely to be a valid one. This assumption is possibly more valid over a very short period, though even here it may be somewhat doubtful. This conclusion throws us back to the original problem of finding a practical short-cut method which does not also significantly increase the margin of error in the results.

If we reconsider the commodity-area matrix mentioned above it is evident that there is another way of aggregating the full detail, namely, by assuming that the unit value index numbers for export of individual commodities *are valid for all areas*. The justification for this assumption is that individual commodities would each be subject to the same cost conditions in the producing country, irrespective of the eventual area of destination. This assumption would not necessarily hold, of course, if demand conditions were very different in different areas, e.g. if in one area there were intensely competitive conditions, while in another there was a supplier's monopoly. But apart from cases of this sort, the assumption appears to be a reasonable one to make. Even if a single heading includes several varieties of a commodity, there is still reasonable ground for supposing that their prices would move substantially together.

The first advantage of making this assumption is that it allows use of the maximum commodity detail (or as much as is required), without increasing the burden of statistical calculation. In fact, the only unit value series needed are those for *total* exports of each commodity, thus eliminating entirely the calculation of area unit values. The second advantage is that it makes explicit allowance for the differences in the commodity pattern of exports by area. On the previous notation, these index numbers can be expressed as follows:

$$\text{Unit value index for exports to } j = \frac{\Sigma\,(p_{1_{ij}}q_{1_{ij}})}{\Sigma\left[p_{1_{ij}}q_{1_{ij}}\left(\dfrac{p_{0_i}}{p_{1_i}}\right)\right]} \quad . \quad . \quad . \quad (7)$$

$$\text{Volume index for exports to } j = \frac{\Sigma\left[p_{1_{ij}}q_{1_{ij}}\left(\dfrac{p_{0_i}}{p_{1_i}}\right)\right]}{\Sigma\,(p_{0_{ij}}q_{0_{ij}})} \quad . \quad . \quad . \quad (8)$$

If the commodity-pattern of exports to *j* is the same as that of total exports, these index numbers reduce to those given in formula (1). If the commodity-pattern differs, but the unit value index numbers for each commodity do not differ as between areas, (7) and (8) then reduce to (2), the index numbers based on the full matrix detail.

The ratio of the Kindleberger "commodity-aggregation" unit value index to the corresponding "country-aggregation" index is (4) : (7) and this is necessarily equal to the ratio (8) : (5), i.e. to a volume index divided by a quantity index, the latter usually in fact being an index of weight (in metric tons, etc.). This result sums up the relative merits of the two short-cut methods. The "commodity-aggregation" method is a hybrid index, including an element (possibly a substantial element) of a crude weight index; the "country-aggregation" method, on the other hand, yields a genuine volume index.

There may, however, be some justification for Kindleberger's method of calculating separate unit values for trade with each area, in so far as he was interested in seeing whether price discrimination occurred as between the different areas. Even so, the point still arises as to whether a comparison of unit values of commodity-aggregates is meaningful in this connection.

III

We must now reconsider the basic assumption that index numbers based on the full commodity-area detail, i.e. formula (2), provide the most reliable results; if this were indeed the case, the merit of index numbers calculated by short-cut methods could be judged by their divergence from the detailed results. However, in practice, it is quite a commonplace to find excessively large fluctuations in the calculated monthly unit values of individual commodities exported to particular countries or areas. It would, of course, be much more difficult when making a long-term comparison to isolate those commodity/area headings which should be rejected in a unit value calculation.

The abnormal movements which occur in computed unit values can generally be traced to one cause—changes in the commodity-structure of the statistical heading. Apart from numerous short-term changes due to different qualities or types being traded in different periods, there are long-term changes due, for example, to trends in quality which affect the value without influencing the recorded quantity.

Because there are normally numerous variations in quality, grades and types of goods included in any statistical foreign trade heading, a unit value index relating to *total* exports recorded under a single heading will frequently be a more reliable guide to the underlying price movement than will be the comparable index numbers based on trade with particular countries or areas. This is because the larger number of transactions in the total figure allows for a substantial amount of "offsetting" of one grade or type against another. Where trade with an individual country is relatively small or where it changes markedly in size and character, there is little scope for such "offsetting", and the abnormal fluctuation in the calculated unit value index may be considerable. Where, therefore, the pattern of trade is roughly similar for each area, the assumption that changes in unit values for individual statistical headings are identical by area may well be the best working assumption that can be made.

Where, however, there are significant differences, as between different areas, in the types of goods included in a single statistical heading, this assumption would not be a valid one. Kindleberger himself provides some examples from the Swedish import returns, and others could be quoted. In such cases as these, the only satisfactory solution is to pursue the calculation by using the full commodity/area detail. Though country-aggregation would not provide the correct result, neither, of course, would commodity-aggregation. It might also be pointed out that where such cases are of real importance, one can expect the national statistical authorities to consider creating additional headings in their returns to separate out the different types previously included in the same heading. To the extent that this is done, the country-aggregation method can be introduced.

There is one important case in which the calculation based on the full commodity/area detail breaks down altogether. This is where there has been a substantial shift over time in the items traded. For example, the most important items in United Kingdom exports of machinery to the United States in 1952 consisted of accounting machinery and machine tools, neither of which were exported to the United States in 1938 (when the most important item was textile machinery). A revaluation of the trade by items would thus be forced to use the country-aggregation method outlined above. The unit value index based on commodity-aggregation is likely to be even less reliable in a case like this than where the items traded have remained the same in the two years being compared.

For these reasons, the country-aggregation method is more than a short-cut approximation. It stands very largely in its own right as a valid method of computing unit value and volume index numbers by area over probably a relatively wide area of trade.

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REVIEWS OF STATISTICAL AND ECONOMIC BOOKS

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1.—*Scientific Inference*. By Sir Harold Jeffreys. 2nd ed. Cambridge University Press, 1957. viii, 236 pp. 8½″. 25*s*.

Scientific Inference is not likely to be of such interest to Fellows as is the same author's *Theory of Probability*, but it is well worth their attention because of the detailed examination it makes of the role of probability in scientific thinking; and it therefore supplements the more specifically statistical details of the other book. The first edition appeared in 1931. The present edition has been almost completely rewritten. "The general standpoint, that scientific method can be understood if and only if a theory of epistemological probability is provided, remains unaltered". There are ten chapters and three appendices. The first chapter is in the form of a dialogue between a botanist and a logician in which the gap between deductive logic and scientific inference is shown to exist and investigated. The second chapter introduces the axioms of probability as a "degree of reasonable belief" and introduces the simplicity postulate. The third and fourth chapters discuss topics more fully dealt with in the other book, sampling and errors. By this stage in the book it has been shown how scientific laws can be established with high probability, and in the fifth chapter an investigation is begun of the relations between some of these laws and the observational evidence that supports them. Physical magnitudes, mensuration, Newtonian dynamics, light and relativity are the topics discussed. Chapter 10 is quite new and deals similarly with statistical mechanics and quantum theory. An oddly-placed ninth chapter is entitled "Miscellaneous Questions" and deals with some fallacies, infinite sets, frequency theories of probability, psychology and general philosophical questions. The three appendices deal with technical points: one of them is devoted to an ingenious proof that π is irrational which Miss Cartwright had the temerity to set in the preliminary (i.e. first year) examination in Mathematics at Cambridge.

The first comment that should surely be made is that Sir Harold Jeffreys is far too little read by statisticians. Mention a prior probability to most of them and they will consider you not quite respectable. But do these people realize that in the *Theory of Probability* practically useful techniques are developed for many of the problems which statisticians consider, and forceful arguments are adduced to show that many of the usual statistical methods cannot do what they claim to do? In the book under review the ideas, rather than the techniques, are explored. The argument appears to your reviewer to be forceful but not complete. The frequency theory of probability comes in for some attacks that seem quite unwarranted. It is very well argued that the frequency theory alone is not adequate to explain induction, but to say that the theory is, within its limited field, unsound is surely carrying things too far. Admittedly no one has carried out an experiment with an infinite number of tosses of a coin, but Kerrich has performed a large number, Chevalier de Méré did enough to detect the difference between ½ and ·491, and insurance houses proper. Has anyone ever shown that light travels with a constant velocity? No, because there is a limit to our measurements (one of Jeffreys' major points) just as there is a limit to the time Kerrich had to toss his pennies. To say that frequency probability does not apply to a single trial is like saying length does not apply to a single object because it is a concept based on a lot of measurements. Furthermore the frequency form of probability is that which occurs in the scientific laws themselves, rather than the probability which, he rightly insists, enters when we talk about these laws. The "degree of belief" of Chapter 2 becomes the "happen with probability α" of Chapter 3. The frequency theory too, is the one that suggests the product law which "degrees of belief" theorists have so much difficulty in justifying.

The discussion in the final chapter is most interesting and the emphasis on the conditional nature of probability illuminating. The author tries to show that it is possible to talk about the joint probability distribution of position and momentum, a fact which some physicists deny. The argument is rather terse and the first introduction of a joint distribution (equation (2) on p. 215) seems quite unjustified. The following point is not discussed. If the wave function ψ has the probabilistic interpretation that $\psi\psi^* d\tau$ is the probability that the position of the system at time t lies in $d\tau$ then why is it that if two streams of electrons combine the probabilities do not add but the wave functions ψ do? The interesting point about statistical mechanics and quantum theory is that probability enters into the statement of the laws. It is a pity that Sir Harold Jeffreys did not examine a science, such as genetics, where the nature of the probability is better understood than it is in those he discusses. In fact the book deals with the physical sciences, to the almost complete exclusion of the others, despite the intrusion of the botanist into the first chapter.

But these are minor criticisms. His main point (the first quotation given above) is well established. Attention is drawn in §2.5 to the major outstanding problem still unsolved in statistics: what do we mean by "ignorance"? Essentially this is the Bayes' postulate problem, and is still with us. I would suggest that we are never "ignorant" but always have some information which our prior probabilities represent. Not that the question affects practical applications, because the information is hardly ever equivalent to more than an observation or two. The book is deserving of the most serious attention from all statisticians who think about the foundations of their subject. The quotations at the beginnings of the chapters are well up to the author's usual high standards.

D. V. Lindley.

2.—*Statistical Methods*. By George W. Snedecor. 5th ed. Ames: Iowa State College Press, 1956. xiii, 534 pp. 9¼″. $7.50.

"Snedecor" has been for a long time one of the most popular text-books for workers in biology and agriculture, and it is still among the best in its field. The fifth edition contains some new material, notably on non-parametric techniques and on multiple tests of significance. Discussion of factorial arrangements has been collected together in a rather formidable chapter which starts with the definition of a comparison or contrast, and finishes with the method of fitting constants to a 2-way table with unequal cell frequencies. An excellent chapter on sampling techniques is contributed by W. G. Cochran.

The style of the book is leisurely and discursive, with liberal use of the first person singular. The abundant worked examples are treated in great detail, and a large number of further examples are provided as exercises. The whole approach is admirably calculated to appeal to the non-mathematician. For a statistician, the "flavour" may best be appreciated by a typical quotation, dealing with the comparison of the means of two samples whose observed variances differ substantially.

"My recommendation is this: If it becomes evident that there is a large discrepancy between the mean squares, re-examine the experimental set-up to discover any possible explanation. It may be that you will find some cause for expecting the standard deviations to be different even though this cause had been overlooked in designing the experiment. In that case, use the Behrens-Fisher test or Cochran's approximation. If no reason for the discrepancy is found, stick to the regular tests but use greater caution in making decisions about the means because the sample estimates of σ^2 are questionable".

M. J. R. Healy.

3.—*Nonparametric Statistics for the Behavioral Sciences*. By Sidney Siegel. New York and London: McGraw-Hill, 1956. xvii, 312 pp. 9″. 49*s*.

The behavioral sciences (a transatlantic collective for social psychology, sociology, and some other disciplines concerned with human behaviour) are very dependent on non-parametric statistical methods for analysis of their data. Few of the variables under consideration can be quantified, without gross, and usually unjustifiable, assumptions. Spearman's "Footrule" and his coefficient of rank correlation demonstrate the psychologist's requirements even at the beginning of this century and many of the advances made in this branch of statistical methodology in the last twenty years are owed to psychological and sociological demand. As also this development has been most marked in recent years (34 out of the author's 97 references are to articles dated 1952 or later) it would appear obviously to the advantage of the research student in these disciplines to have a recent comprehensive volume at his disposal.

However, recency is no substitute for critical judgment nor constructive exposition. The author states in his introduction "we have tried to convey an 'intuitive' understanding of the rationale underlying a test, and have thought that this understanding will be more useful than an attempt to follow the derivation". After a five-page general introduction there is an eleven-page chapter on the uses of statistical tests in research, which rushes through a discussion of the null hypothesis, levels of significance and the sampling distributions. There is then a chapter on measurement, of the nature now usual in American psychological texts, and a brief outline of the types of data suitable to non-parametric treatment. The reader is then launched into a catalogue of non-parametric tests. They are conveniently arranged: the one-sample case; the case of two related samples; two independent samples; k related samples; k independent samples, and measures of correlation. Each test is presented in almost identical manner—a paragraph or two on function (sometimes including rationale), a page on method, an example, a summary of procedure, a statement of the power-efficiency of the test, and some references. It is difficult to believe that many students will derive an "intuitive" understanding of the rationale underlying a test from this guide-book-like procedure.

The recommendations in the sign test with two related samples are particularly unsatisfactory. Only $+s$ and $-s$ with respect to the hypothesis are considered; all occasions of equality are dropped completely from the analysis. This reaches a climax in the recommended treatment of a before and after opinion study. An imaginary example is given, of the effect of a film on opinion concerning desirable severity of punishment for juvenile delinquents. The figures quoted are—out of 66 individuals who favoured more punishment before the film, 7 still favoured more but 59 now favoured less. Out of 34 who initially favoured less punishment, 8 remained of the same opinion, and 26 now favoured more punishment. Thus 85 individuals changed their opinion, and 15 did not. The author ignores these 15, states his null hypothesis that you would expect half of the 85 to have changed from more to less, and half from less to more (despite the fact that only 34 individuals favoured less punishment in the first enquiry), and procedes to test as a binomial distribution with $n = 85$. This reviewer is left puzzling as to what conclusion could reasonably be formulated after performing such calculations, but he fears that the behavioral sciences are in for some peculiar conclusions if this statistical advice is followed. J. W. WHITFIELD.

4.—*Variabilità e Concentrazione.* By Corrado Gini. 2nd ed. Roma: Libreria Eredi Virgilio Veschi. 1956. xxviii + 738 pp.

Between 1910 and 1930 Professor Gini published a number of substantial memoirs on various aspects of measures of location, dispersion and statistical relationship. These papers were issued in 1938 in collected form as volume 1 of a series of Memoirs on Statistical Methodology. The book has been out of print for some time and has now been reissued in a second edition which contains a number of supplementary notes by Professors Pizzetti and Salvemini describing more recent work in the field of variability and concentration. Some articles on transvariation which appeared in the first edition have been removed and may be issued separately.

Until very recently statistical methodology in Italy evolved almost independently of Anglo-Saxon influence. In three pages of the Index to Authors we find only a few references to Bowley, Pearson and Yule and practically none to any living English statisticians except myself—and the references to me are mainly to contest something I have said about Italian work. If we examined the index of any current English book (myself again excepted) we should find a similar ignoration of Italian authors. This state of affairs is not due to a perverse nationalism, although perhaps national characteristics on both sides have done something to perpetuate it. Rather is it indicative of different traditions in the two countries.

The English reader of this book will thus find that it has a somewhat elusive quality. It ought, he will feel, to deal with a subject with which he is familiar. But he will meet many expressions which he has never encountered before, at least in English text-books: notably a large number of coefficients, for example, of equidistribution, homophily, evolution and mean trigonometrical deviation. He should not be put off by this or by the size of the book. A great deal of this material is discussive and illustrative. The hard core of methodological work contains a good deal of interest.

The first memoir of the series (191 pages) develops Gini's own measure of *concentration*, that is to say of the extent to which a divisible magnitude such as income is shared out unequally among the possible recipients—the kind of thing which is studied by English economists by means of the Lorenz curve. The second (about the same length) develops measures of dispersion (variabilità) and variation (mutabilità). The best known of these, of course, is the mean difference. The third applies some of the ideas to the variation of qualitative characteristics (attributes); and the remaining articles deal with sundry related topics such as curves of concentration.

It is a typical feature of Gini's work that he is primarily interested in measuring the properties of distributions and thinks more of the intrinsic properties of his measures than of their sampling variability. His applications are usually made on demographic material for which the sample is so large that standard errors are not regarded as very important. So far as concerns such a field, he may be right; but to an English writer the absence of a theory of estimation or testing hypotheses is likely to hinder the application of Gini's methods in fields where the samples are smaller.

The coefficient of mean difference is a case in point. Some years ago I pointed out that the measure $\sum_{i,j} |x_i - x_j|/N^2$ was inferior to the variance $\sum_{i,j} (x_i - x_j)^2/2N^2$ in at least three respects, ease of calculation, mathematical tractability and sampling simplicity. Professor Salvemini now points out that the case is not so strong as it was. He is right, but it still seems to me fairly strong in those respects, and if there were no other considerations present, the balance of advantage would still be with the variance. What Salvemini does not do, and might well have done, is to point out that the mean difference has now acquired a standing of its own, as instanced by Stuart's formula giving the correlation of variate-values and their rank numbers: $\Delta\sqrt{3}/2\sigma$ where Δ is the mean difference and σ is the standard deviation.

The text is very legible and the main argument can be followed with no more than a schoolroom knowledge of Italian.

M. G. KENDALL.

5.—*Petrographic Modal Analysis.* By Felix Chayes. New York: Wiley, 1956 (London: Chapman and Hall). xii, 113 pp. 9″. 44*s.*

Petrography, for the benefit of those statisticians as ignorant as myself, is the study of the mineral composition of rocks and the method of modal analysis is the process of estimation of this composition by the microscopic examination of a thin section of rock.

This book, by a professional petrographer, addressed in the main to professional petrographers, is an account of the statistical treatment of modal analysis data, and will not give a statistician any new knowledge about statistics.

Its value to a statistician is as a model example of how to present a statistical argument to non-statisticians, indeed, in this case, to a body of scientists who are notoriously prejudiced against the use of statistics.

It is probably one of the most lucid non-technical descriptions of the various kinds of experimental error yet written. It shows that, contrary to the popular opinion amongst geologists, the method of modal analysis is theoretically sound, since the method gives consistent estimates of mineral composition, and that under certain specified conditions these estimates are unbiased. It gives a clear exposition of the sources of random errors involved in the technique and it describes the methods of estimating the magnitude of these components. No practising statistician, however remote his interest in geology, would be wasting his time in reading this book.

The ultimate interest of the geologist is in the composition by volume of the rock in a locality. This is found from the composition by volume of certain selected hand specimens, and this in turn is found from the areal composition of randomly-chosen plane sections of these hard surfaces. is found from the areal composition of randomly-chosen plane sections of these specimens.

Modern statisticians will realize that this is an example of the Monte Carlo estimation of a volume by the area of random sections.

It is commonly said that the only new feature of Monte Carlo is the application of sampling methods to deterministic problems. Here is an example of the method applied in that way which has been known since 1848! Perhaps Monte Carloists will draw some satisfaction from the fact that geologists have been doubting the validity of the method right until the present day.

Even the determination of the areas of the various minerals in a section are obtained by Monte Carlo methods. Either the intercepts on randomly-chosen lines are measured or the incidences of the mineral under randomly-chosen points are counted. This is crude Monte Carlo. But the petrologist uses the more refined method of casting a mesh of points on the section at random. This method and certain refinements of it are now being hailed under the delightful title of antithetic variates. Progressive geologists had, by purely physical arguments, invented this method 50 years ago.

An exposition of the validity of these methods occupies the first chapter of the book. The second chapter considers the effect of a banded structure on the validity and by a geometrical argument shows that by restricting the orientation of the bands to the scanning axes to a narrow range the sampling variance can be considerably reduced.

The third chapter deals with the actual instruments used to make the analyses while the fourth and fifth chapters discuss the binomial character of the distribution of areal estimates of a mineral in a given section and illustrate from data that this theoretical distribution holds in practice.

The author then goes on to discuss the variability of areal estimates from *different* sections and emphasizes that, from this point on, the geometrical arguments that have sufficed till now, are no longer sufficient. The actual facts of grain distribution in the rock have to be taken into account.

It is shown how the grain size relative to the actual total area scanned is the important factor and describes the analysis of an experiment which indicates that for a fixed grain size the variance of estimated area has a logarithm linearly related to the logarithm of the area scanned.

Grain size is not an easily defined concept, and the author develops a quantity called the intercept count (IC) which can be used as a measure of grain size and traces, by analysis of experiments, the relation between this measure and the variance for a special rock (the two feldspar granites).

Once these two relations are accepted it is a simple matter to determine the amount of replication necessary to reduce the variance of the final estimate below any pre-assigned level. This is reduced to a set of three simple charts.

The last chapter of the book deals with the problem of very fine grained material in which the finite thickness of the section introduces a bias into the areal measurement. For each grain the maximum area in the section is measured and these do not all lie in one plane as assumed in the Monte Carlo theory.

This is a most instructive book for petrologists and statisticians alike.

K. D. TOCHER.

6.—*Wahrscheinlichkeitstheorie*. By Hans Richter. Berlin [etc.]: Springer, 1956. xii, 435 pp. 9½″. 66 Dm.

German technical literature has now almost recovered from the isolation imposed on it by past political authority and its more recent publications are in many ways more up to date than comparable treatises in the Anglo-Saxon countries. This book by H. Richter, and L. Schmetterer's *Mathematische Statistik*, published by the same firm of well deserved international repute, may serve as thorough introductions, in the German academic manner, to the twin disciplines of statistics and probability theory.

The author is well known to students of German periodicals from his contributions to the foundations of probability theory. In his Foreword he discusses the lack of German text-books written in a modern spirit. He therefore attempts to "introduce the reader sufficiently far into present-day theory of probability, so as to enable him to understand more difficult investigations without neglecting unduly the classical theory".

To make this attempt successful and rather than expect the reader to master other text-books first, the author includes a chapter on measure theory, and one on integration. He stresses the fact that his subject may properly be considered as a branch of measure theory, but for various reasons, not least that of easy applicability, the exposition starts with "intuitive" probability, proceeding through "physical" to "mathematical" probability, the latter being based on a system of axioms.

The headings of the chapters are: I. Foundations of Measure Theory. II. The Concept of Probability. III. The Elements of the Theory of Probability. IV. Elements of Integration Theory. V. Random Variables in General Probability Fields. VI. Special Probability Distributions. VIII. Convergence of Random Variables. In any book on probability, a selection must be made and Professor Richter excludes, to his expressed regret, stochastic processes, Markov chains, and the "problem of indirect theory," i.e. the relation between objective and subjective probability. Mathematical statistics is not treated, but the chi-squared, t, F and T^2-distributions are mentioned as "connected with the normal distribution" (pp. 359 ff.). The very last section deals with the central limit theorem.

The demonstrations are clear and rigorous, with the usual warnings about sections which may be skipped at a first reading. The reviewer was pleased to find (on p. 116) a detailed account of the question which the Chevalier de Méré asked, thereby sowing the seed of the theory of probability. However, he would have liked, here and elsewhere, more precise references to original sources. The bibliography contains 30 titles, a few of them heretofore unknown to the reviewer, but the author disclaims any intention of thereby rating the available literature.

At the end of sections there are exercises and their solutions are indicated rather than given at the end of the book.

The formulae in Springer books are generally more crowded than we like it in this country, but the production is, of course, quite excellent.

S. VAJDA.

7.—*Statistical Sampling for Auditors and Accountants.* By Lawrence L. Vance and John Neter. New York: Wiley, 1956 (London: Chapman and Hall). x, 310 pp. 9″. 72*s.*

This book consists largely of an attempt to present to auditors the modern statistical techniques for sampling which might be used in situations, such as test checks, where sampling of a rather more rough and ready kind is already in use; it does, however, also attempt to suggest that sampling methods might be used in other accounting situations where they have not so far been extensively used. Apart from various discussions of actual or possible applications of the methods in auditing practice, the book's main contents are expositions first of the techniques of acceptance sampling and then of statistical estimation.

The emphasis throughout is placed on giving practical instruction on how to use the statistical techniques rather than on explaining the theoretical basis for them. The meaning of operating-characteristic curves is explained, for example, in terms of the probabilities of accepting populations of different qualities; little is said on how these probabilities might be calculated. There is, instead, a long discussion of the methods of using the various tables and curves which are available for choosing sampling plans; many of the tables and curves themselves are reproduced. As a result, the section on acceptance sampling can be used conveniently as a reference book when actually carrying out such a sampling scheme. It might, however, have been desirable to have given accountants, if they are to use this as their only reference on sampling, a little more of the basic theory, which one would not have thought more difficult than the instructions on the use of the tables. One consequence of the omission of such theory may be that the person who uses the book is left uncertain of what he is in fact doing when he attempts the suggested modifications of the tables to suit his particular requirements. For instance, in discussing the use of Dodge-Romig graphs for doubling sample plans they suggest that if the consumer's risk of ·10 for accepting a population of 5 per cent. incorrect items is felt to be too high, the auditor may work with, say ·04 instead of ·05 and thus reduce this risk. However, no guidance is given on the amount of the reduction.

In the section of the book dealing with statistical estimation, the standard results on estimators, confidence limits, and optimum allocation are presented for many of the more familiar sampling designs; numerical examples are given throughout to illustrate how the results should be applied. The omission of the relevant theory is clearly more justified in this case in view of its greater length and difficulty. The exposition should be very helpful for anyone wishing to use the book as an aid in carrying out a sampling plan. Even here, however, the reader without statistical knowledge may be lost occasionally as, for example, in the discussion of cluster sampling where the way in which the advantages of the method depend on the value of the intra-class correlation coefficient is stressed without an adequate indication of what this is and how its value is to be estimated.

The authors leave an impression of meticulous care in their presentation of the statistical techniques. When the methods given in the text are not exact, attention is frequently drawn to the fact in a footnote—although the reader for whom the book is intended will often be unable to understand the footnote.

Perhaps slightly less care has been given to other points as a consequence. On p. 108 the probability of reaching a decision after the first sample of a double sampling plan is given as the sum of the probabilities read off a logarithmic graph as 0·91 and 0·0002, the sum being given as 0·9102. One might be forgiven for thinking that accountants, more than most people, need warning against this type of spurious precision. On p. 274, a working sheet is shown for arranging in order numbers obtained from a table of random numbers. This takes the form of a rectangle with rows for thousands and columns for hundreds, giving 100 cells for four digit numbers. It is suggested that 400 cells can be provided by giving a vertical interval of 500 and a horizontal one of 50.

Despite the book's lack of statistical theory, the authors have succeeded admirably in presenting the methods of statistical sampling and their book may well become a standard work on the application of such methods in auditing.

W. J. CORLETT.

8.—*Local Expenditure and Exchequer Grants.* By D. S. Lees, W. Appleyard, D. A. Hogg, H. Keeling, T. Watson and S. P. Roberts on behalf of the Institute of Municipal Treasurers and Accountants (Inc.). London, 1956. viii, 352 pp. 8½″. 42*s.*

This is a research study sponsored by the Institute of Municipal Treasurers and Accountants and the terms of reference of the study group were "To review the system of Exchequer Grants to Local Authorities in England and Wales".

The importance of local authority expenditure in the country's economy can be gauged from the fact that in the financial year 1952–53 it amounted to £743 million of which £381 million was contributed by the central government—£323 million by way of percentage grants for specific services, e.g., education, police, etc., and a further £58 million through the operation of the

Exchequer Equalization Grant (a grant to equalize the disparities of rate poundage between local authorities).

The book sets out to review the problem of the distribution of this central government subvention and the equity of its working in relation to the resources and needs of local authorities whose only other source of revenue is the local rates. Chapters I–III deal with the Exchequer Equalization Grant, chapters IV–VI discuss the needs of local authorities according to their varying characteristics and certain conclusions on weighting for needs are given on p. 154; chapters VII–XII examine the percentage and unit grants for specific services followed by a summary chapter and final conclusions.

Apart from the use of the coefficient of dispersion the authors have not availed themselves of statistical techniques and much of the data contained in the tables in the appendix is well suited to treatment by correlation methods; however, the information is there for any statistician so disposed. Considerable expenditure of effort has been devoted to showing that in the working of the Exchequer Equalization Grant " . . . children weighting does not have a very pronounced effect on the relative finances of local authorities. This is because most authorities have children/population ratios not far removed from the average". This would have been better demonstrated if the ratios had been calculated and included in the tables.

A deficiency of the book is that all the figures are based on the financial year 1952–53 and the revaluation of rateable values which took effect in 1956 has seriously altered the picture. The authors cannot be blamed for this.

It is interesting to read the conclusions reached in the light of the recent governmental statement on this subject. The conclusions of the authors that the percentage grants should be retained and the equalization grant remain, with minor variation, are at variance with the Ministerial statement (February 12th, 1957) of the government's intention to review the whole structure of local authority finance and to give local authorities more scope in the spending of the central subsidy, i.e. replacement of percentage grants by a general grant system.

As a text-book for the student of public administration there is insufficient treatment of the historical background of the subject (but this is not its purpose) and it is likely soon to be out of date. As a reasoned examination of the present system it will be an invaluable aid to politicians and administrators currently engaged upon the formulation of new proposals. It has a place in the reference library of statisticians for the factual data contained in the appendix which are presented in a convenient form. A. T. GORE.

9.—*The Registrar-General's Decennial Supplement, England and Wales*, 1951, *Life Tables.* London, H.M.S.O., 1957. 38 pp. 9½″. 3*s*.

The second world war interrupted the sequence of population censuses which had been taken in this country at decennial intervals since 1801. For twenty years the most recently available table of population mortality remained the English Life Table No. 10, based upon the mortality experienced by the population of England and Wales in the years 1930–32. The two decades which followed witnessed great strides in medical science with remarkable changes in mortality rates. Accordingly, E.L.T. No. 10 had become obsolete long before it was superseded.

A description of the new table—which will be known as English Life Table No. 11—forms the greater part of the volume under review. The table was constructed by the Government Actuary at the request of the Registrar-General and is based upon the population of England and Wales enumerated at the 1951 census and the deaths recorded in the three years 1950–52. An awkward problem was presented by the abnormally high mortality—particularly at older ages—experienced in 1951 owing to severe weather conditions during the early months of the year. Fluctuations from year to year are, of course, inevitable and any period of several years will normally contain some that are high" and some that are low". But the excess mortality of 1951 was altogether exceptional and its inclusion in the period on which E.L.T. No. 11 is based means that the general level of the mortality rates at older ages in the table is somewhat higher than the current norm. The possibility of further "diluting" the 1951 experience by basing the table on the deaths over five years (viz. 1949–53) was considered, but was rejected because the inclusion of 1949 led to overstatement at young ages. Another possibility which was examined was the construction of a table based upon the experience in 1950 and 1952, the year 1951 being ignored. This was also rejected as not being sufficiently representative of the mortality around the actual census date.

While accepting these arguments, however, one is still left with a feeling of uneasiness. English Life Tables Nos. 7 and 8 were both based on the 1911 census and one may wonder whether the precedent could not have been followed and two tables produced on the present occasion. The second table could have been based on 1950 and 1952, or, since population projection is now a regular feature of the Registrar-General's publications, it might have been constructed from the deaths in 1953 and the estimated population in the middle of that year. Mortality in 1953 was, on the whole, somewhat lighter than in 1952 and in many age-groups it was considerably lighter than in 1950. An E.L.T. No. 12 based on 1953 would, therefore, have given a closer representation of contemporary mortality, while E.L.T. No. 11 would still have served as the official life table associated with the 1951 census.

Unlike earlier national life tables, the rates of mortality in E.L.T. No. 11 were graduated by a mathematical formula. In the past, finite difference methods have been preferred as it has not been found possible to fit a simple Gompertz or Makeham curve. Nowadays it is usual to employ graduation formulae containing several more parameters than the Gompertz and Makeham formulae and, in consequence, to reproduce the main kinks in the mortality curve. Even so, the formulae used in constructing E.L.T. No. 11 were restricted to ages over 20 for men and over 26 for women. For younger ages, finite difference methods were retained.

A feature of the ungraduated mortality rates which called for careful attention was an increased steepness in the progression of the rates for men between 40 and 50. As the report remarks: ". . . in recent years the mortality rates of young men have declined much more rapidly than those of men in middle life". A similar feature was noted in a memorandum published by the Institute and Faculty of Actuaries on the mortality of assured lives during the period 1949–52. It is interesting to note how the problem was met in each case. In both tables the investigations began with the logistic

$$f(x) = a + b/\{1 + e^{-\alpha(x-x_1)}\}$$

which has been found useful in earlier graduations. For the assured lives' table, provision was made for the new feature by the addition of two extra parameters in the denominator of the second term, i.e.:

$$f(x) = a + b/\{1 + e^{-\alpha(x-x_1)} + e^{-\beta(x-x_2)}\}$$

For E.L.T. No. 11, the first and second terms were retained without modification, but a third term was introduced involving three extra parameters, i.e.:

$$f(x) = a + b/\{1 + e^{-\alpha(x-x_1)}\} + c\ .\ e^{-\beta(x-x_2)^2}$$

Thus the complete formula involves the addition of a Gaussian function to a logistic. It is interesting to note that identical values of α and β were used for both the male and female tables.

For both males and females the resultant graduation gave a close fit to the original data (as measured by a comparison of observed deaths with those expected by the graduated rates of mortality) and also corresponded extremely closely with the rates produced by the traditional finite difference method evolved by King. Whether the graduation is *too* close to the original data, or whether the employment of complex formulae containing numerous parameters is a wholly desirable development, raises issues on which there has been a great deal of inconclusive argument during the past fifty years.

As is to be expected, comparison with earlier life tables shows that the greatest reductions in mortality have been achieved at the youngest ages. Moreover, the change over the period 1931–51 was, on the whole, more spectacular than it was over the period 1911–31. Thus at age 20, the rate of mortality for males fell from ·00348 in 1911 to ·00316 in 1931 and then to ·00129 in 1951. Corresponding rates for females were ·00295, ·00268 and ·00083. At age 60, however, the male rate, after falling from ·03042 to ·02415 between 1911 and 1931, fell by only a negligible amount to ·02369 in 1951. This arrest in the reduction of mortality rates for middle-aged and elderly men is disquieting and is largely to be attributed to the increase in deaths from lung cancer and coronary disease. There is no corresponding feature in the women's rates. As a result, the gap between male and female mortality rates has been steadily broadening and at age 60, the male rate of mortality in E.L.T. No. 11 is 186 per cent. of the female rate. All these matters are lucidly illustrated in the *Decennial Supplement* by a series of tables and graphs.

In addition to the national rates of mortality, the *Decennial Supplement* gives rates for various regions and "conurbations". The heaviest mortality is to be found in the conurbations of Tyneside, Merseyside and South-East Lancashire, where it is approximately 135 per cent. of the mortality in southern, south-eastern (excluding London) and eastern England, i.e. the three regions composing the area which shows the lightest mortality. In certain age-groups the differences are even more remarkable, as may be seen from the following rates of mortality for men in Merseyside and in Eastern England:

Age	*Merseyside* (1)	*Eastern England* (2)	(1)/(2) (3)
32 . . .	·00223	·00134	1·66
42 . . .	·00443	·00285	1·55
52 . . .	·01369	·00833	1·64
62 . . .	·03496	·02292	1·53
72 . . .	·08230	·05816	1·42

Clearly there is some scope here for further developments in social and industrial medicine.

R. D. CLARKE.

10.—*Methods for Population Projection by Sex and Age* (Population Studies No. 25). United Nations Department of Economic and Social Affairs. New York, 1956. London: H.M.S.O. vii, 81 pp. 11″. 5*s*. 6*d*.

Until recently, projections of population have been attempted only for countries with highly-developed systems of vital statistics and censuses. Many have regarded these excercises as being of doubtful value, but in spite of such reservations the general demand for estimates of future numbers has increased. Indeed, the Population Branch of the United Nations Bureau of Social Affairs, in its series of manuals on various aspects of demographic studies, has now begun to issue forecasts for a period of thirty years ahead for regions of the world in which vital statistics are known not to be very accurate—for example Central and South America. The present manual, based on the supposition that projections for these areas have not hitherto been made only because they lack statisticians with sufficient training (and not because estimates of this kind were considered too unreliable to be worth making), is directed towards the provision of a method that can be learnt and applied by relatively inexperienced workers in the field of population records.

As a result of the particular aim of the manual the text, after a good general introduction, is largely concerned with the problems that are likely to arise in under-developed countries. Thus, chapters are devoted to the construction of abridged life tables and to the use of a standard model giving a broad idea of how mortality might decline in future years. Attention is devoted also to the appraisal and adjustment of data, especially of fertility, that are of doubtful accuracy or provide conflicting evidence. At each stage, a practical example is given by showing how the method could be applied in the case of one country—Costa Rica.

The manual, like other United Nations population productions, is clear and concise and should enable the suggested method to be applied by students in many countries for which projections have not hitherto been made. It is of particular interest to follow through the sections devoted to the example of Costa Rica. In that country, delay in birth registration may amount to a period of several years, and the number of registrations in recent years has been about one-third higher than the number of births said to have occurred in those years. Considerable uncertainty exists, therefore, as to the actual level of fertility at present, and an apparent increase in fertility since 1950 cannot be relied upon because the rise in the recorded numbers of births may be attributable to a reduction in under-registration or to a change in the extent of the delay in registration. Such uncertainty about the level and trend of a most important factor in population growth adds piquancy to the discussion, which took place at the World Population Conference in 1954 and is sure to recur, of the validity and usefulness of population projections in such countries, especially for a period of no less than thirty years ahead of the latest census.

Among the papers of the Rome Conference there was one by the United Nations Population Division giving projections for the world as a whole, in which the contributory figures for the United Kingdom, supposedly based on medium assumptions, were much more "optimistic" than even the upper extreme of the sixteen carefully considered assessments of the British Royal Commission. The method used on that occasion was a more rough and ready one than that now proposed; but the unreasonableness of the result obtained illustrates the fact that methods of procedure that are relevant to areas such as Central and South America are unlikely to be the most appropriate that could be devised for (say) the United States or Western Europe. Although warnings are sounded in the manual to the effect that adaptation of the method is desirable according to individual circumstances, these are not carried to the extent of showing how projections might be made in a country with a history of the collection of comprehensive and reliable data for several decades.

It would appear that a number of the figures in Tables 49 and 50 should have been prefaced by minus signs.

P. R. COX.

11.—*Immigrants and Their Children* 1850–1950. By E. P. Hutchinson. New York: John Wiley, 1956 (London: Chapman and Hall). xiv, 391 pp. 9″. 52*s.*

The foreign-born population of the United States was 2¼ million in 1850, rose to a peak of 14⅕ million in 1930 and fell to 10⅓ million in 1950. A detailed analysis of trends in the earlier decades was provided in the 1920 Census monograph by Niles Carpenter, *Immigrants and Their Children.* The year 1920 proved to be an important turning-point in the history of immigration to the United States, and this gives added interest to this new study by Professor Hutchinson which carries the story up to 1950.

The volume is a model of meticulous scholarship, lucidly arranged and far-reaching in scope. Drawing on the rich array of Census data, the author deals thoroughly with changes in the size, composition and geographical distribution of the foreign stock from 1920 to 1950 and gives far more information about occupational structure than Carpenter had been able to furnish. Separate chapters are devoted to the occupational characteristics of the foreign-born at each Census year. Students of population statistics will be particularly grateful to Professor Hutchinson for his appendices giving the instructions to enumerators relating to nativity and parentage, notes on the reporting of nativity, parentage, etc., in the censuses of 1850 to 1950, and the grouping of occupations in the 1950 condensed occupational classification, with corresponding grouping for 1920 and 1910.

One of the results of this study not generally recognized is that the changes in the composition of the flow of immigrants into the United States have had consequences quite as significant as the decline in the size of the inflow. In the words of the author, “ . . . the altered socioeconomic conditions of interwar and postwar Europe and especially the refugee movements changed the selectivity of migration and drew migrants from strata of the European population that had formerly contributed few migrants to the United States” (p. 269). Although the Quota Act was designed to weight the scales in favour of Northern European stocks, differences in mortality rates have caused the proportion of North-western Europeans among the foreign-born to decline steadily, while there has been a growing proportion of Southern Europeans and of natives of the Western Hemisphere. The median age of the foreign-born in 1920 was 40 years, but by 1950 it was as high as 56 years—more than double that of the native white stock. The same trend is visible in the age composition of the second generation.

The book contains fascinating material for studies of the economic aspects of social stratification in a dynamic society. Striking differences are to be observed between the various national stocks. Usually the occupations chosen by the second generation are quite different from those of the parents; but this is on the whole not true of the Polish, Hungarian, Czech, Yugoslav, Greek, French Canadian and Mexican stocks.

The author concludes by reminding us that the occupational data analysed in this book “ . . . can give only partial answers to the question of the role of immigrant workers and their children in the industrial and economic growth of the United States” (p. 278). He has, however, provided an invaluable store-house of material which will make the path of the investigator in this field much easier.

BRINLEY THOMAS.

12.—*European Refugees,* 1939–52 . . . By Malcolm J. Proudfoot. London: Faber and Faber, 1957. 542 pp. 8¾″. 52*s.* 6*d.*

Forced migrations of one kind or another have been common since biblical times. However, the scale and tempo of the movements in the last twenty-five years are certainly unique in the world's history, and on that account alone deserve special study. These movements have also a special significance in many facets of our lives. From the purely military standpoint, for instance, the refugee movements when France fell certainly hindered the Allied armies. It may well be that, if the Allied command had laid no plans to deal with the situation that arose towards the end of the war, the refugee would have rendered more difficult the final overthrow of the German armies. There are many other interesting aspects of these vast movements—organizational, medical, social, psychological and political.

In 1939 the Oxford University Press published *The Refugee Problem*, which covered forced movements in Europe up to October 1938. The present work deals with the subsequent period. The author was deeply involved in the very centre of this movement as it developed and thus brings to his study a vitality that no purely documentary study could hope to give. Details, of trivial importance in themselves, enable the reader to picture more vividly the nature of the events under discussion, for instance, the reference to a camp of 12,000 refugees run by a lieutenant and sergeant alone.

The various movements to and fro as the tide of battle turned, the numbers and types of people involved and other details are all adequately covered. But the core of the work derives from the author's own experiences as a senior officer at S.H.A.E.F. dealing with displaced persons, and concerns the plans that were laid to meet the problem, the part played by U.N.R.R.A., I.R.O. and other bodies, and how these plans worked out in practice.

Briefly, the pattern of the work is both logical and chronological, with subdivisions by nationality permitting ready reference to particular movements. The first four chapters cover the dispersion of the peoples of Europe by the circumstances of the war. Chapters 5 and 6 cover the plans to deal with the vast problems that the military and civil governments would face as the advancing armies overran more and more displaced persons. Chapter 7 deals with the static problem of immediate care for the refugees after liberation and the remaining six chapters deal with their disposal.

The mass repatriation of Western nationals who fell into the hands of Western armies is covered in a single chapter, but the story of the refugees who refused repatriation to Eastern Europe drags on as the problem changes from one of repatriation to one of resettlement. An interlude is necessary to discuss the political problems involved—the relative positions of the military and U.N.R.R.A., the hand-over from U.N.R.R.A. to I.R.O., and the growing tension between the Russian and Western governments. Because of their special importance, the problems of Jewish and German refugees receive a separate chapter of their own.

This work is likely to remain a unique study in its own particular field. It will prove invaluable to anyone again involved in the planning of refugee treatment, useful in statistical, sociological and administrative fields as a work of reference, and well worth reading by a wide range of students of human populations. N. H. CARRIER.

13.—*Social Characteristics of Urban and Rural Communities*, 1950. By Otis Dudley Duncan and Albert J. Reiss, Jr. New York: Wiley 1956. London: Chapman and Hall. xviii, 421 pp. 9″. 52*s*.

Every time a census is taken, new information becomes available about the spatial distribution of the population. This aspect of demographic statistics probably attracts the attention of a wider circle of workers in different fields of inquiry than does any other aspect, for studies of it have been made not only by economists and social scientists but also by town planners and geographers. Whereas official census publications are normally limited to a straightforward presentation of the data for each separate administrative area, it is open to the specialist worker to group the areas together in such a way as to reveal the characteristics he is seeking.

In Great Britain the Interdepartmental Committee on Social and Economic Research, in its 1950 Report, stated that "It has long been recognised that social statistics . . . cannot be adequately studied on the basis of single administrative units and that studies on . . . the daily social and economic life of the people are often made difficult by the absence of statistics for larger urban areas." The outcome of the deliberations of the Committee has been the interesting 1951 Census Conurbations Report. Procedure in the United States has been somewhat similar. Late in the 1940's the Social Science Research Council there took the lead in stimulating the formulation of plans for a series of post-censal monographs, officially sponsored but not official, one of which is the subject of the present review. This monograph is not, however, concerned only with large cities and their environs but treats of a whole range of communities including farming and non-farming areas. The analysis is "in terms of concepts and hypotheses drawn from the discipline of human ecology or social morphology", and is thus guided along a sociological line of inquiry.

Four aspects of communities were made the object of study in the American investigation, namely size, spatial organization, growth or decline and functional specialization. It was also recognized, though less prominently than in the Conurbations Report, that regional location has effects that should be taken into account. In Great Britain, each conurbation was treated to some extent as an individual entity, but the American authors have been more concerned to investigate differences between central cities as a whole and their adjacent areas as a whole; between those parts of the rural population that are within the satellite zones of metropolitan centres and the rural population situated further away from such centres; and between urban places enjoying a relatively independent status and urban places located within metropolitan areas. These are all nation-wide generalizations. A more extended—and particularly interesting—comparison, bringing within one analysis not only the metropolitan centre of a city and its suburbs but also the surrounding area of the countryside that is in a process of transition from a rural to an urban mode of settlement, is confined, however, to one city.

In the 1950 Census of the United States of America, besides the usual questions about sex, age, occupation and nationality, a sample of the population was asked for details of recent mobility, employment, earnings, other income, and educational standard attained. The tabulation of the answers to these questions for various types of area in relation to the four principal aspects of the inquiry, and the accompanying commentary on the figures, necessarily occupy considerable space, and the authors proceed solidly item by item through the whole mass. Inevitably the outcome is inspiring only in the sense of completeness and thorough application which it arouses. It is perhaps not surprising in the circumstances that insight into the obvious is occasionally provided—for instance, "Non-manufacturing centers . . . have a much higher proportion of white-collar workers than do the manufacturing centers". Nevertheless this is a job that needed doing and has been faithfully performed. It is a pity that almost the whole of the analysis is confined to the 1950 Census of the United States and that comparisons with earlier censuses in that country, or contrasts with the results of enumerations in other countries, have not been included. There seems to be room for a broader study on these lines, the outcome of which should be stimulating.

P. R. Cox.

14.—*Readings in Market Research.* Edited by Frederick Edwards. London: British Market Research Bureau Ltd., 1956. xxxvi, 235 pp. 10″. 35*s.*

Since the end of the war British manufacturers have shown a rapidly expanding willingness to use market research to help decide policy on advertising, distribution, packaging, product content, public relations, etc. The personnel required to carry out this work has been largely recruited from university graduates who have read economics, mathematics and various arts subjects and who have received their specialist education while working as trainees for their first employers. In recent years this task of training recruits has been facilitated by occasional week-end schools, evening lecture courses and discussion groups organized by the Market Research Society, the Institute of Practitioners in Advertising and similar associations. One or two elementary text-books have been published but any newcomer of university level has had to depend for published material on the many excellent American journals and text-books on this subject. No substantial text-book on market research has yet been written in this country and Mr. Edwards and his colleagues have set out to fill the gap by republishing in a single volume fifteen articles by British authors which bear on market research techniques and practices. The only new material is an excellent introductory essay in which Messrs. Downham, Shankleman and Treasure describe market research as now practised in Britain, indicate likely future developments and provide a sound bibliography. All the fifteen articles are good but their sum is perhaps ill-balanced. Over half the material is provided by academicians and civil servants concerned primarily with social surveys and not with market research (e.g. L. Moss—"Sample Surveys and the Administrative Process", and H. F. Lydall—"The Methods of the Oxford Savings Survey"). One consequence of this is that, relatively, far too much space is given to technique of sampling (e.g. the papers by Corlett and Edwards, Gray, Corlett and Frankland, and Gray, Corlett and Jones), too little to the construction of questionnaires and the problems of interviewing. Some of the gaps are substantial. There is little or nothing on product testing, communications research, copy testing, advertising media research other than the press, motivation research, and the only case history is taken not from commercial market research but from a university social survey. Usually these absences are due to the fact that no relevant material has been published by a British author and this makes one doubt the wisdom of nationalism in the compiling of a "Reader". Half-a-dozen essays drawn from American sources (e.g. Payne or Cantril on questionnaire construction, Hyman and Stember on interviewing, Smith or Politz on motivation research, Lazarsfeld on communications, Ferber or Katona on predictive consumer surveys) would have made possible a book of much greater value to those starting on a career in market research in this country. Even among the British contributors there are some regrettable omissions; one would have liked to have seen reprinted Mr. Cauter's paper describing in detail the organization of a large-scale markect research agency, and the code of practices adopted by the Market Research Society. However, even as it stands, the book is sufficiently valuable to make us look forward to new editions with additional material.

Mark Abrams.

15.—*Modern Market Research: A Guide for Business Executives.* By Max K. Adler. London: Crosby Lockwood, 1956. ix, 158 pp. 7¼″. 15*s.*

Aiming to provide (i) a general outline and basic perspective of practical market research work and (ii) a guide to the executive either wanting to see possibilities, or looking for a "check list" when business need dictates that he must buy research for the first time, this book should commend itself to students and teachers alike, for the first object seems admirably fulfilled; the newcomer to business can profitably base his further study on such a practical review.

Emphasis has had to be placed here and there on matters which may not seem quite so relevant to the second purpose. To "get the message over" to that school of executives dependent on courses of optical exercises to help their reading and understanding of material may still be difficult, but others who are sufficiently interested to turn its pages will almost certainly read to the end—and with profit. This itself is creditable when modern jargon and technicalities still await ecological discipline within the forest of words and systems that make up socio-economic research at present. It is much easier to write 50,000 words than 5,000 words readably yet cogently. Very few undefined terms are left to puzzle the layman on first reading. A few cross references might have been useful when a topic is taken up later in the text.

Figures and formulae are conspicuous by their absence. The most required of the reader is a knowledge of percentages, mainly when looking at the diagrams. It may seem paradoxical therefore to commend the book to statisticians; but it can prove illuminating just to see where and how "popularized" statements about the basic concepts of probability reveal differences between lay and professional imagery and how far the gap can be bridged. Thus where "the condition of randomness" is asserted "as an essential condition to ensure that the sample is representative of the whole", practical experience is obviously implicit. But it may well be felt that, although necessary, it is not necessarily sufficient. (Is the condition really necessary in certain types of work? Is not stratification the real essential? It seems to depend on the nature of the problem involved; indeed insistence on randomness may sometimes frustrate the securing of useful results.)

The biggest snag always lies in conveying an idea of the "margin of error". The treatment may not be quite adequate for either the young student or the intelligent executive. The latter often thinks in terms of the *range* within which the results *must* lie. To him there is no question of repeating samples indefinitely when he has paid for a single set of figures, but rather "What are the most realistic figures here to use when making my decision?".

The way the variance is generally found to be small in the component groups forming normal 500 or 2,000 samples might perhaps have been stressed when discussing the theoretical base and perhaps also more space given to the discussion of practical biases. Some researchers may differ from the author about the Politz—Simmons method of coping with non-response. The reviewer feels that the claims would be more sound "theoretically" if the actual interviewing operation could itself be "randomly" distributed throughout the 24-hour day (so that night or catering workers and those wanted by the police would have an equal chance of inclusion!).

A controversial question is whether, as the author believes, "motivation" and "depth interview" will replace conventional market research studies. Is an oblique reply to "Why?" usable without direct knowledge of "Who?", "What?" and "How?". An open mind is perhaps advisable until and unless data can be given statistical weight. It may well prove that with better analysis there are more possibilities still latent in existing market research methods, and that the most promising future lies more in the proper marriage of the two. A welcome biography on recent motivation research articles contrasts with the fact that no general bibliography has been given—but that perhaps is why this book is necessary. Very little comprehensive material has yet been published incorporating the considered views and general knowledge of people with practical experience and no axe to grind. Few pioneers in this great current experiment have as yet found time to write their memoirs but a considerable amount of collating is now being undertaken and the sincerity and integrity of researchers are no longer questioned.

Discussion as to what is done when questionnaires have to be discarded, and also the importance of stating the date and period covered by the survey, are two small omissions, while one or two charts (p. 56 and 65) are unsatisfying; it is not clear whether differences in average quantities purchased have been allowed for in the latter. So too, the phrase "notoriety measure" seems an unfortunate newcomer (p. 77).

But if this book succeeds in making more business men question their methods, products and markets and start to examine where research can possibly help, it will have served its purpose; for economic surveys sensibly carried out can provide most, if not indeed all, of the base required for many business decisions. As markets widen and competition increases, so too will the need for market research, and indeed for books of this kind. JOHN MASON.

STATISTICAL NOTES

(1) British Official Statistics

The index of retail prices compiled by the Ministry of Labour and National Service which was 103 (January 17th, 1956 = 100) in November and December rose to 104 in January and February. In January there were increases in the prices of milk, tea, meat, bacon, fish, vegetables and sugar, partly offset by reductions for eggs and butter. There were also increases in the prices of some articles of furniture and floor coverings and for laundering and shoe repairing. In February, increases for sugar, bread, mutton and lamb were more than offset by reductions for eggs, butter, fish and cheese. The detailed figures of the weights used in calculating the index and the indices for the different commodity groups were as follows:

Date	*Food*	*Alcoholic Drinks*	*Tobacco*	*Housing*	*Fuel and Light*	*Durable Household Goods*	*Clothing and Footwear*	*Transport and Vehicles*	*Miscellaneous*	*Services*	*All Items*
Weights:	350	71	80	87	55	66	106	68	59	58	1,000
Nov. 13th, 1956	102·5	102·6	105·3	104·7	104·5	101·4	101·1	102·9	103·4	105·9	103·1
Dec. 11th, 1956	102·3	102·6	105·3	104·8	105·9	101·4	101·2	106·3	104·9	106·1	103·4
Jan. 15th, 1957	104·0	102·6	105·3	105·0	106·2	101·7	101·2	109·5	105·8	106·5	104·4
Feb. 12th, 1957	103·5	102·6	105·3	105·2	106·3	101·9	101·6	109·8	105·9	105·9	104·3

The Ministry of Labour index of weekly wage rates which is now calculated on the basis of January 31st, 1956 = 100, showed that in November, 1956, the level was 106 and remained at that figure till February, 1957, when it rose to 107. For manufacturing industries alone the indices were the same. The indices for all industries and services were also 106 from November to January, and 107 in February for men and women separately and 107 in November and December and 108 in January and February for juveniles.

The six-monthly enquiry into average earnings showed that in October, 1956, the average earnings per week in the industries covered by the enquiries, i.e. in the manufacturing industries and in some of the principal non-manufacturing industries, were 237*s*. 11*d*. for men, 123*s*. 2*d*. for women, 102*s*. 4*d*. for youths and boys and 81*s*. 4*d*. for girls. These figures represent percentage increases since April, 1947, of 99, 83, 116 and 102 per cent. For all workers combined the average was 200*s*. 8*d*., an increase of 94 per cent. This compares with an estimated rise in weekly full-time *rates* of wages in the same industries of 66 per cent. In manufacturing industries alone the average weekly earnings in October, 1956, were 245*s*. 7*d*. for men and 123*s*. 8*d*. for women. The corresponding hourly earnings were 61·1*d*. and 35·7*d*. The average weekly hours worked in all the industries and services fell slightly from 46·7 to 46·6 between April and October, 1956.

The total working population and the numbers in civil employment in the four months ended January, 1957, were as follows:

(*Thousands*)

	Total Working Population			*Numbers in Civil Employment*		
	Males	*Females*	*Total*	*Males*	*Females*	*Total*
Oct., 1956 .	16,245	7,978	24,223	15,319	7,872	23,191
Nov., 1956 .	16,236	7,989	24,225	15,296	7,882	23,178
Dec., 1956 .	16,207	7,880	24,087	15,227	7,762	22,989
Jan., 1957 .	16,204	7,873	24,077	15,223	7,748	22,971

The number of persons on the unemployment registers of the Employment Exchanges rose by 32,500 in December and by a further 85,700 in January. In February it fell by 2,500. The total for February represented 1·8 per cent of the estimated total number of employees in Great Britain. The percentages in the different Regions ranged from 1·0 in the East and West Ridings and 1·1 in the North Midlands to 3·0 in Wales and 3·1 in Scotland.

The following is a sex analysis of the figures:

Number of Unemployed Persons on the Registers of Employment Exchanges

	Men and Boys	*Women and Girls*	*Total*
Nov. 12th, 1956 . . .	167,517	97,061	264,578
Dec. 10th, 1956 . . .	199,663	97,284	296,947
Jan. 14th, 1957 . . .	267,735	114,870	382,605
Feb. 11th, 1957 . . .	262,351	117,709	380,060

Of the total of 380,060 in February 90,793 had been unemployed for not more than two weeks, 111,256 for two to eight weeks and 145,326 for over eight weeks, while 22,685 were temporarily stopped. In the four weeks ended February 6th, 153,024 vacancies were filled by the Employment Exchanges. The number of vacancies unfilled at this date was 239,028, 16,514 less than a month earlier.

The numbers of insured persons absent from work owing to sickness, including self-employed as well as employed, were 910,400 in November, 1956, 840,000 in December, 971,200 in January and 948,900 in February. The numbers of employed persons absent owing to industrial injuries were 60,900, 54,100, 57,600 and 61,700.

The *Ministry of Labour Gazette* for February contains estimates of the total number of employees (employed and unemployed) at May, 1956, based on the insurance cards exchanged in the subsequent three months supplemented by returns from employers. The totals show, for the United Kingdom, 22,173,000 (14,397,000 males and 7,776,000 females) and for Great Britain 21,700,000 (14,100,000 males and 7,600,000 females). A full analysis of the figures by industries is given.

(2) OTHER STATISTICS

The *Annual Bulletin of Transport Statistics for Europe* 1955 (Geneva, E.C.E. 9*s*.) continues to develop. Information additional to that in previous issues (see this *Journal*, Vol. **118**, p. 261) is the distribution of locomotives and railmotor vehicles by horsepower (under 350 h.p.; 350 h.p. and over) and of lorries by load capacity. More information than in past issues is available for eastern European countries and an appendix is devoted to statistics of transport in the U.S.S.R. supplemented by an up-to-date map of the railways of the U.S.S.R.

In the years 1951 to 1954, eight European countries made sample surveys of the magnitude and structure of goods transported by road. The results of these surveys are given in an appendix. Statistics of transport accidents are given in a separate publication and those of air transport are compiled by another international organization.

The *Year Book of Labour Statistics* 1956 (Geneva I.L.O. 30*s*.) follows the same lines as previous issues (a full account of the 1954 issue is given in this *Journal*, Vol. **118**, p. 260). The chief change is a very large increase in the number of "countries." No less than 114 are listed in the index, almost double the number of some years ago. Some of these countries appear in only a few of the tables, but for the table of consumer price indexes only nine are missing (Ethiopia, Nigeria, Alaska, Aden, Ryukyu, Singapore, Czechoslovakia, Liechtenstein and U.S.S.R.). The table of total and economically active population is almost as complete. No statistics for the U.S.S.R. have been given since the issue for 1947–48; this year an appendix gives the available data on population and its percentage distribution by productive sectors and principal industries, and the numbers of wage earners and salaried employees in recent years. The number of "specialists" is given as 5,133,000 of whom 2,184,000 are with higher education.

The years covered vary: for the general tables annual data are usually given for 1937, 1938, and 1948 to 1955, and monthly data from 1953 to 1956, while for many of the detailed tables only the annual figures for 1948 to 1955 are given. All index numbers are given whenever possible on the year 1953 as basis. All the data are based on communications to the office or on official publications, a complete list of which is given on pp. 491–498.

The European Productivity Agency, a branch of the Organization for European Economic Co-operation, decided, a few years ago, to devote to the technique of statistical quality control (i) a series of seminars, led by American experts, and (ii) two conferences to compare the opinions and experience of various O.E.E.C. countries. The first of these projects was carried out from June to August 1954 and the second in July 1955.

Following these projects a booklet, consisting of four sections, has now been published by the E.P.A. entitled *Statistical Quality Control.*

The first section gives an account of the work done in the various European countries in connection with the visits of the American experts, Professor Clifford, Mr. Littauer and Mr. Benson, who made two tours of Europe. An appropriate body in each country arranged meetings for them to address, the programmes varying from one country to another. The United Kingdom did not participate in this project.

The second section is entitled "Modern Quality Control; Objectives, Necessary Conditions and Procedures" by Paul C. Clifford. There is, deliberately, no intention of writing a text-book in this section; it outlines the considerations that make Quality Control necessary and sketches briefly the types of technique used. A complete novice would have difficulty in understanding in one or two places; for example the text states "figure 13 shows a double sampling plan" whereas in fact what figure 13 shows is the operating characteristic curve of a double sampling plan, together with the curve for acceptance on the first sample, with no explanation of what these curves represent—the scales are not even identified as percentage defective and probability of acceptance (the vertical scale is marked $P_a i\%$ but these mystic symbols are not explained). However, in spite of a few obscurities such as this, this section is, in general, to be recommended as a useful exposition for someone who knows nothing of the subject but wishes to see what it has to offer.

The third section is a report of the first of the two conferences. This conference was restricted to two experts from each of the countries which had participated in the first project. In addition the United Kingdom was invited to appoint two experts as observers: Mr. E. D. van Rest and Mr. Mark Taylor were appointed. At this conference the delegates submitted national reports, which are summarized in the booklet.

The fourth section is a report of the open conference which followed the restricted one. It was attended by 127 representatives from 11 member countries of the O.E.E.C. and observers from Spain, India and the U.S.A. The booklet summarizes the 13 papers presented to this conference (4 by British speakers), the discussion which followed each one, and the opening and closing speeches by Mr. C. Igonet, the Director of E.P.A.

CURRENT NOTE

At the invitation of the City and University of Edinburgh and the Royal Society of London, the International Congress of Mathematicians will meet in Edinburgh from August 14th to 21st. 1958. The Executive Committee is inviting a number of mathematicians to deliver one-hour and half-hour addresses. There will also be daily sessions devoted to fifteen-minute communications. Those who wish to present such communications will be given an opportunity of offering to do so.

There will be eight sections: 1, Logic and Foundations; 2, Algebra and Theory of Numbers; 3, Analysis; 4, Topology; 5, Geometry; 6, Probability and Statistics; 7, Applied Mathematics, Mathematical Physics and Numerical Analysis; 8, History and Education. A programme of entertainments and excursions is being planned. There will be two categories of membership of the Congress: *Ordinary members*, who will be entitled to participate in the scientific and social activities of the Congress and to its Proceedings, and *Associate members* accompanying ordinary members, who will be entitled to many of the privileges of ordinary membership, but will not participate in the scientific programme and will not receive the Proceedings.

Those who wish to receive further information are requested to communicate their name and full address to Frank Smithies, Secretary of the Congress, Mathematical Institute, 16 Chambers Street, Edinburgh 1, Scotland, as soon as possible. Towards the end of 1957 they will receive more detailed information (including the fees to be paid by members).

OBITUARY

Professor Sir Arthur Lyon Bowley

November 6th, 1869–January 21st, 1957

The death of Sir Arthur Bowley on January 21st, 1957, in his 88th year, removed from the Society's roll one of its most distinguished Fellows. In the length of his Fellowship and in the extent of his contributions to the Society's Proceedings, he was probably the most notable figure in the Fellowship of the Society since it was established. His activities in other statistical and economic Societies at home and overseas gave him an outstanding international reputation.

The son of the Vicar of SS. Philip and Jacob in Bristol, Bowley was born on November 6th, 1869. He was a pupil at Christ's Hospital from 1879 to 1888 and during this time gained the first of many distinctions which came his way during his long life. In 1886 he won the Tyson Gold Medal for mathematics and two years later, in his last year at school, he gained the Montefiore Prize for mathematics and classics, and also the Thompson Gold Medal for mathematics. In this year he won a major mathematics scholarship to Trinity College, Cambridge. The years he spent at Christ's Hospital made a lasting impression on him and he not infrequently spoke of them in his later life. It was with satisfaction that he renewed his association with the school as a representative of the University of London on the Council of Almoners from 1936 to 1947 and was thus a Governor *ipso facto* during that time.

At Cambridge, Bowley read mathematics, being bracketed as 10th wrangler in 1891. He gained the Cobden Prize in the following year with *A Short Account of England's Foreign Trade in the 19th Century* (third edition, 1922), and the Adam Smith Prize in 1894. In 1913 he became Sc.D. It was as an undergraduate that he began to develop the interest in social conditions and questions of social reform which he maintained for over 60 years. In about 1890 he began to turn his thoughts to the subject of poverty and of income distribution and this was given early expression in an unpublished paper now in the Society's Library (written in his own handwriting in an elementary exercise book) on the subject of socialism. This was an address given in 1896 at St. John's School, Leatherhead, when he was on the staff. It is interesting to notice how very little his handwriting changed during the subsequent 60 years.

On leaving Cambridge, Bowley spent a year as Assistant Master at Brighton College and then, from 1893 to 1899, he taught mathematics at St John's School, Leatherhead. In 1900 he became Lecturer in Mathematics at University College, Reading, and was appointed to the Chair in Mathematics and Economics in 1907, a post which he held until 1913. It was while at Leatherhead that he entered upon the main path of his distinguished career. In 1895 Sidney Webb and others founded the London School of Economics and Political Science in Adelphi Terrace. On Marshall's recommendation, Bowley was invited to become one of the initial and very small staff, as part-time lecturer in statistics. Thus in October 1895, the month when the London School of Economics opened, Bowley gave the first lecture of his 40 years' unbroken service. By 1908 he was appointed Reader at the School and seven years later he was given the title of Professor. Meanwhile, he had ceased to occupy the Chair of Mathematics and Economics at Reading, but retained his association there as Lecturer in Economics from 1913–1919. In 1919 Bowley was elected to the newly-created chair in Statistics in the University of London, an appointment which he held until his retirement in 1936, when he was made Emeritus Professor. He was elected a member of the Senate of the University of London in 1930 and during the last War, from 1940–1944, was acting Director of the Oxford University Institute of Statistics.

Bowley's professional activities covered an unusually wide range both at home and abroad. He was elected a Fellow of the Royal Statistical Society in 1894 and became a member of the Council in 1899. At that time one of the Council members was Sir Rawson Rawson, who had been elected a Fellow in 1835, so that Bowley at the time of his death must have been the sole surviving Fellow who had a direct link with an almost original Fellow. He read his first paper to the Society in March 1895 (on "Changes in Average Wages (Nominal & Real) in the United Kingdom between 1860 and 1891") and thus began a series in the Society's *Journal* of almost 20 papers of his sole authorship and 15 papers jointly with others (mainly G. H. Wood). He was a Vice-President of the Society for the two Sessions 1907–1909 and 1912–1914, becoming President in 1938–1940. There will be a small number of Fellows who still recall with considerable pleasure that afternoon in the early summer of 1939 when as President he gave a garden party to the Council of the Society and some others at his home in Haslemere; refreshments were served in the shade of his trees and members of the Dolmetsch family played Elizabethan music as his guests sat or reclined on the lawns. In 1895, Bowley was awarded the Guy Medal in Silver (only two had previously been given) and, 40 years later, the Medal in Gold, thus becoming one of the only three Fellows who have received that double honour.

His other professional interests were similarly prominent. He became a member of the International Statistical Institute in 1903 and attended most of the Sessions during his membership. In 1929 he was elected Treasurer of the Institute, being succeeded by Sir Josiah (later Lord) Stamp in 1936. He served again as Treasurer from 1947 to 1949, when he was honoured by election as Honorary President. It was on his return from the 1929 Session in Warsaw, when asked how he had fared, that he gave a very characteristic reply, "Scenery monotonous, currency varied". He was as diligent in the interests of the Institute as for other Societies to which he belonged and over the course of years wrote many reports on a range of subjects, including international comparisons of wages and economic and social conditions in large cities.

He was also active in the British Association, being Secretary of Section F in 1899–1901, Recorder in 1902–1905, President 1906 and for the following five years a member of the Council. His reports for the Association covered international trade statistics, women's labour, and the distribution of small incomes.

From its foundation in 1923, Bowley was Editor and member of the Executive Committee of the London and Cambridge Economic Service. He continued as Editor until 1945 and as a regular contributor until 1953. During this period, he published, either alone or jointly with others, ten special memoranda in addition to his regular contributions, mainly on wages, earnings and prices.

He was closely associated with the Royal Economic Society, having been elected a Fellow in 1893, and was on the Council by 1901. He published 15 papers in the *Economic Journal* on a wide range of subjects between 1895 and 1928.

Bowley was a Foundation member of the Econometric Society in 1933 and was a member of the Council for the first 14 years, being President in 1938–39.

He was the Newmarch Lecturer at University College for two consecutive years on two occasions. In 1897 he gave a series of lectures on The Accuracy of Averages and in 1898 on Wages in the United Kingdom in the 19th Century. Some 30 years later he was again the Newmarch Lecturer, giving six lectures in 1927 on Tests of Trustworthiness of Public Statistics and in the following Session a further six lectures with the all-embracing title "Statistics".

In his field, Bowley was held in high esteem in official circles. He served on a number of official Committees, particularly the Advisory Committee on the Cost of Living. At the end of 1933, he was invited by the Secretary of State for India to undertake, in association with D. H. Robertson (as he then was), "a review of Indian economic conditions and of the present statistical organisation of India." He accordingly spent the early months of 1934 in India, and later that year published with Robertson *A Scheme for an Economic Census of India*.

He received many other honours: the honorary degrees of D. Litt., Oxford, and D.Sc., Manchester; he became a Fellow of the British Academy in 1922; in 1937 he was appointed C.B.E. and in 1950, Knight Bachelor.

The immense quantity of his publshed work is indicated in the bibliography given at the end of this notice. All this was done while he was lecturing regularly to generations of students and giving detailed care to the work of post-graduate research workers. As a teacher it must be recognized that he was at his best with post-graduate students and with others undertaking advanced studies. With the undergraduate, groping his way in an endeavour to achieve a statistical frame of mind, Bowley assumed an ability to follow an argument that was not always justified. Whether the student should have taken this as a compliment is an open question, but certain it is that the pre-degree student often stumbled and even sometimes fell. There was perhaps a tendency for Bowley's lectures to develop on occasion into a confidential monologue addressed to the blackboard, as he chalked up line after line of formulae. The fact that intermediate steps were often omitted as being unnecessary did not help. Thus it was that those students who took statistics as a subordinate or voluntary subject were apt to become impatient; but those who were taking statistics as their main subject came sooner or later to realize that the necessity of helping themselves was itself a very real element in the education that Bowley provided and by them he soon became acknowledged as the very great man he was. The research worker found himself in a different position. In order to do research under Bowley, one needed already to be mentally prepared to reap the advantage of Bowley's mind. To such he was an invaluable guide and many men distinguished in public life, such as the late Lord Stamp and Lord Beveridge, have acknowledged their indebtedness to him.

His investigations into the incidence of poverty brought him into close contact with the leading contemporary social investigators. In this work he relied largely on sampling techniques and was a pioneer in this particular branch of statistical method. It may indeed be claimed that Bowley laid the foundation of the substantial development in later years of the application of sampling techniques which to-day is the basis of market research and social enquiry. His last full-scale research into the measurement of changes in social conditions was the work he did on the monumental *New Survey of London Life and Labour*, 1930–1935, on Volumes III and VI of which he left his unmistakable impression. Indeed, he himself was inclined to regard the part he played in this investigation as the culmination of his long interest in social enquiry.

In personality, Bowley was somewhat shy and retiring. He did not readily make close friends and his intimate friendship with Edwin Cannan over many years was an almost unique experience. Few of his students achieved the very rewarding privilege of his intimacy but those who did realized their exceptional good fortune. Bowley was the very soul of discretion and only in his last few years would he occasionally reveal to close friends the inner history of this or that event which at the time was a matter of wide speculation.

It was with Cannan in particular that he indulged his addiction to the bicycle. "Cannan and I used our bicycles as more modern people use cars". As early as 1895 when living at Leatherhead and lecturing at the London School of Economics "I used to bicycle from and to Leatherhead on the Wednesday half holiday to lecture at 5.0 (or was it 6.0?) p.m." The following extract from a letter dated September 12th, 1906 to Cannan shows Bowley in a typical frame of mind, " . . . That's all I can say. I don't think the theory of any importance; all that counts is already in Marshall. The following problem is however of finite importance. I wish to go to Wirksworth (a few miles north of Derby) on September 30th and return on October 2nd. In both cases, I want from 70 to 90 miles cycling on moderately secluded roads and a complementary express railway journey, e.g., Reading to Leicester and Derby to Wirksworth by road and L. to D. by train. What is my route?". F. Y. Edgeworth, who not infrequently joined Bowley and Cannan on their cycling trips, was prone to discuss mathematical questions as the trio rode along, and Cannan is reported to have said on one such occasion "Bowley, let us go a little faster, Edgeworth cannot talk mathematics at more than eight miles an hour".

To know Bowley in his home was to know how much his family life meant to him. With his wife and daughters he shared great happiness and the gracious atmosphere of his home left a deep impression on all who knew it. It was a rare delight to see Bowley seated at his piano as he played Bach or Haydn with a real personal pleasure. In the last year or two of his life he took up weaving (at which Lady Bowley had long been expert) and for this purpose a skein of yarn had to be cut into a number of pieces of similar length. Even then he brought his professional interests to bear as he discussed with a visitor the significance of the standard deviation of a large number of lengths which ought to have been but were not identical.

There are several of Bowley's pre-1914 students now retired who continue to acknowledge their debt to him, and in this they are joined by his later students and colleagues. He was held in high esteem by all who knew him—and indeed by those who knew him best with a deep affection. It was in recognition of his authority as a statistician and social investigator together with his greatness as a man that a large congregation, including many distinguished figures, attended on February 11th, 1957 at St. Martin-in-the-Fields the Memorial Service arranged jointly by the London School of Economics and the Royal Statistical Society.

R. G. D. ALLEN.
R. F. GEORGE.

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The Measurement of Productive Efficiency[*]

By M. J. Farrell

[Read before the Royal Statistical Society, March 20th, 1957, the President, Professor E. S. Pearson, C.B.E., in the Chair]

1. Introduction

The problem of measuring the productive efficiency of an industry is important to both the economic theorist and the economic policy maker. If the theoretical arguments as to the relative efficiency of different economic systems are to be subjected to empirical testing, it is essential to be able to make some actual measurements of efficiency. Equally, if economic planning is to concern itself with particular industries, it is important to know how far a given industry can be expected to increase its output by simply increasing its efficiency, without absorbing further resources.

A number of attempts have been made to solve this problem, but, although they usually produced careful measurements of some or all of the inputs and outputs of the industry, they failed to combine these measurements into any satisfactory measure of efficiency. This failure was partly due to a pure neglect of the theoretical side of the problem. Indeed, for a long time it was considered adequate to measure the average productivity of labour, and to use this as a measure of efficiency. This is a patently unsatisfactory measure, as it ignores all inputs save labour, but it was so widely used by economic statisticians that it is now enjoying an extensive popular vogue, which may indeed have unfortunate effects on economic policy. More recently, attempts have been made to construct "indices of efficiency", in which a weighted average of inputs is compared with output. These attempts have naturally run into all the usual index number problems.

It is the purpose of this paper to provide a satisfactory measure of productive efficiency—one which takes account of all inputs and yet avoids index number problems—and to show how it can be computed in practice. In doing so, an estimate of the relevant production function is obtained. The method is illustrated by an application to agricultural production in the United States.

It is hoped that the paper will be of interest to a wide range of economic statisticians, business men and civil servants, many of whom have little knowledge of economic theory or mathematics. For their benefit, the main exposition is in the sort of terms used by elementary economic textbooks, and such elementary mathematics as is necessary for discussion of the general case or of computing problems is confined to sections 2.3 and 5. Similarly, although the treatment of the efficient production function is largely inspired by activity analysis,[†] no reference is made to this in the exposition. The professional economist can easily draw the necessary parallels for himself, as indeed, he can note the similarity of the measure of "technical efficiency" and Debreu's "coefficient of resource utilization" (Debreu, 1951).

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[*] I should like to express my gratitude to Sir Dennis Robertson, Dr. T. E. Easterfield and Dr. M. R. Fisher for their helpful comments on this paper, and to Mr. P. Fisk and Mr. E. Osborn for their assistance with the calculations. I am also grateful to the Director of the Cambridge University Mathematical Laboratory for permitting the larger-scale calculations to be performed on EDSAC, and to Dr. L. J. Slater for her help in this. I am, however, most deeply indebted to Mr. J. A. C. Brown, who not only offered many helpful comments on the paper, but devoted an immense amount of time and energy to acting as interpreter between EDSAC and myself.

[†] See, for example, Koopmans (1951).

The measures developed are intended to be quite general, applicable to any productive organization from a workshop to a whole economy. For ease of exposition, they are developed primarily in terms of the efficiency of a firm, but, as will be seen later, the application to industries is in many ways more interesting and, indeed, the illustrative example is of this kind.

2. STATEMENT OF THE METHOD

2.1. *Efficiency Measures in a Simple Case*

When one talks about the efficiency of a firm one usually means its success in producing as large as possible an output from a given set of inputs. Provided that all inputs and outputs were correctly measured, this usage would probably be generally accepted. At any rate, the measure of *technical efficiency* defined below conforms to this usage.

Consider, for the sake of simplicity, a firm employing two factors of production to produce a single product, under conditions of constant returns to scale (These restrictions will all be relaxed later.) Suppose the *efficient production function* is known; that is, the output that a perfectly efficient firm could obtain from any given combination of inputs. This concept, too, will be discussed later.

The assumption of constant returns permits all the relevant information to be presented in a simple "isoquant" diagram, of the kind familiar in elementary text-books. In Diagram 1, the point P represents the inputs of the two factors, per unit of output, that the firm is observed to use. The isoquant SS' represents the various combinations of the two factors that a perfectly efficient firm might use to produce unit output.

Now the point Q represents an efficient firm using the two factors in the same ratio as P. It can be seen that it produces the same output as P using only a fraction OQ/OP as much of each factor. It could also be thought of as producing OP/OQ times as much output from the same inputs. It thus seems natural to define OQ/OP as the *technical efficiency* of the firm P.

This ratio has the properties that a measure of efficiency obviously needs. It takes the value unity (or 100 per cent.) for a perfectly efficient firm, and will become indefinitely small if the amounts of input per unit output become indefinitely large. Moreover, so long as SS' has a negative slope, an increase in the input per unit output of one factor will, *ceteris paribus*, imply lower technical efficiency.

However, one also needs a measure of the extent to which a firm uses the various factors of production in the best proportions, in view of their prices. Thus, in Diagram 1, if AA' has a slope equal to the ratio of the prices of the two factors, Q' and not Q is the optimal method of production; for although both points represent 100 per cent. technical efficiency, the costs of

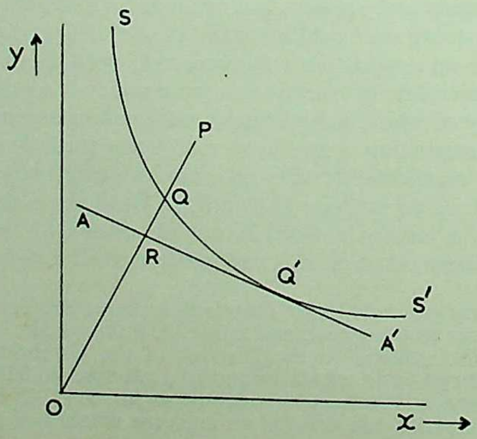

DIAGRAM 1.

production at Q' will only be a fraction OR/OQ of those at Q. It is natural to define this ratio as the *price efficiency* of Q.

Further, if the observed firm were to change the proportions of its inputs until they were the same as those represented by Q', while keeping its technical efficiency constant, its costs would be reduced by a factor OR/OQ, so long as factor prices did not change. It is therefore reasonable to let this ratio measure the price efficiency of the observed firm P, too. This argument is not entirely conclusive, as it is impossible to say what will happen to the technical efficiency of a firm as it changes the proportions of its inputs, but, with this qualification, it seems the best measure available. It also has the desirable property of giving the same price efficiency to firms using the factors in the same proportions.

If the observed firm were perfectly efficient, both technically and in respect of prices, its costs would be a fraction OR/OP of what they in fact are. It is convenient to call this ratio the *overall efficiency* of the firm, and one may note that it is equal to the product of the technical and price efficiencies.

2.2. *The Efficient Production Function in the Simple Case*

These measures of efficiency have been defined on the assumption that the efficient production function is known. In other words, they are methods of comparing the observed performance of a firm with some postulated standard of perfect efficiency, so that each of the measures has, in general, corresponding to each postulated standard, a different value and a different significance. It is therefore necessary to consider the definition of the efficient production function before discussing the significance of the efficiency measures.

Although there are many possibilities, two at once suggest themselves—a theoretical function specified by engineers and an empirical function based on the best results observed in practice. The former would be a very natural concept to choose—after all, should not a postulated standard of perfect efficiency represent the best that is theoretically attainable? Certainly it is the concept used by engineers themselves when they discuss the efficiency of a machine or a process. However, although it is a reasonable and perhaps the best concept for the efficiency of a single production process, there are considerable objections to its application to anything so complex as a typical manufacturing firm, let alone an industry.

In the first place, it is very difficult to specify a theoretical efficient function for a very complex process. Even the best engineer is likely to overlook some problems, and it must be very difficult indeed to estimate, *a priori*, a plant's need of, say, indirect labour. Thus, the more complex the process, the less accurate is the theoretical function likely to be. Also, partly because of this, and partly because the more complex a process, the more scope it allows to human frailty, the theoretical function is likely to be wildly optimistic. If the measures are to be used as some sort of yardstick for judging the success of individual plants, firms, or industries, this is likely to have unfortunate psychological effects; it is far better to compare performances with the best actually achieved than with some unattainable ideal.

Thus, although the theoretical standard is perfectly valid and has its own uses, this paper will be concerned with the observed standard. The next problem is thus to estimate an efficient production function from observations of the inputs and outputs of a number of firms. On the same assumptions as before, each firm can be represented by a point on an isoquant diagram, so that a number of firms will yield a scatter of points like that on Diagram 2. The efficient production function will be represented by an isoquant, and the problem is to estimate such an efficient isoquant from the scatter diagram.

If it can be assumed that the isoquant is convex to the origin and has nowhere a positive slope, then the curve SS' is the most conservative (or pessimistic) estimate of it. That is to say, SS' is the least exacting standard of efficiency that is consistent with the observed points and satisfies these two assumptions.

Of these two assumptions, that of convexity is almost always made in economic theory. It amounts to assuming that if two points are attainable in practice, then so is any point representing a weighted average of them. Since constant returns to scale have already been assumed, this merely requires that the processes represented by the two points can be carried on without interfering with each other. The assumption that the slope of the isoquant is nowhere positive is

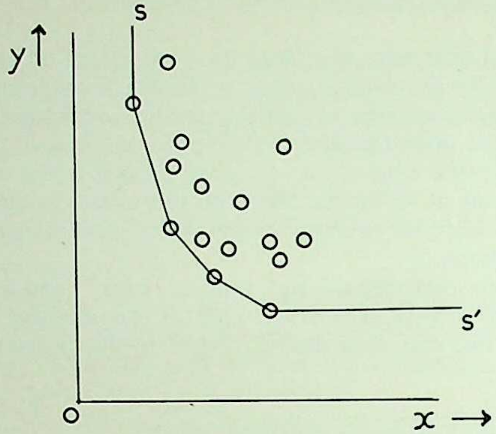

DIAGRAM 2.

not usually made in the text-books, but it ought to be. If it did not hold, increased applications of both factors would result in reduced output!

The curve SS', then, will be taken as the estimate of the efficient isoquant. It will be seen that this method of measuring the technical efficiency of a firm consists in comparing it with a hypothetical firm which uses the factors in the same proportions. This hypothetical firm is constructed as a weighted average of two observed firms, in the sense that each of its inputs and outputs is the same weighted average of those of the observed firms, the weights being chosen so as to give the desired factor proportions.

It is worth emphasizing that it is this that is the essence of the method, and not the representation in an isoquant diagram, which is purely an expository device. In the generalization in the next section to allow many inputs and outputs the isoquant diagram is perforce abandoned, but the basic principle of forming a hypothetical firm as a weighted average of the appropriate number of observed firms remains unchanged.

2.3. *Generalization to the Case of Many Inputs and Outputs*

The curve SS' may be defined geometrically as follows. It is composed of the line-segments joining certain pairs of points, chosen from a set A of points consisting of the observed points plus the two points $(0,\infty)$ and $(\infty, 0)$. (The two points at infinity are added to give the parts of SS' parallel to the axes.) The pairs of points chosen are those for which the line joining them satisfies the two conditions:

(i) that its slope is not positive;
(ii) that no observed point lies between it and the origin.

These two conditions can conveniently be expressed as the single condition that no point of A lies on the same side of the line as the origin.

This is equivalent to the following algebraic definition. Write any point in the form $P_i = (x_{i1}, x_{i2})$, and let λ_{ijk}, μ_{ijk} be the solution of the equations

$$\left.\begin{aligned}\lambda x_{i1} + \mu x_{j1} &= x_{k1}\\ \lambda x_{i2} + \mu x_{j2} &= x_{k2}\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where P_i, P_j and P_k are points in A. Then the line-segment joining P_i and P_j is part of SS' if and only if

$$\lambda_{ijk} + \mu_{ijk} \geqslant 1 \text{ for all } P_k \text{ in } A \quad . \quad . \quad . \quad . \quad (2)$$

It is perhaps worth while to elaborate a little. Any point on the line P_iP_j can be written as $\lambda x_{i1} + \mu x_{j1}, \lambda x_{i2} + \mu x_{j2})$ where $\lambda + \mu = 1$; and for points between P_i and P_j, $\lambda, \mu \geqslant 0$. Hence, if P_iP_j lies between P_k and the origin, $\lambda_{ijk} + \mu_{ijk} > 1$; and if OP_k cuts P_iP_j internally, $\lambda_{ijk}, \mu_{ijk} \geqslant 0$.

Thus the equations (1) may be used to determine the technical efficiency of any point P_k. It is first necessary to find which segment of SS' is intersected by OP_k—that is, to find the segment P_iP_j of SS' for which λ_{ijk}, $\mu_{ijk} \geqslant 0$. Then the technical efficiency of

$$P_k = \frac{1}{\lambda_{ijk} + \mu_{ijk}}.$$

An equivalent but more elegant definition (and one which is useful in actual computations) is that the technical efficiency of P_k is the maximum of

$$\frac{1}{\lambda_{ijk} + \mu_{ijk}}$$

for all segments P_iP_j of SS'. The convexity of SS' ensures that this expression reaches its maximum where λ, $\mu \geqslant 0$.

The generalization to permit n inputs, while retaining the assumptions of a single product and constant returns, is straightforward. Each observed firm is now represented by a point in n-dimensional space, written typically as a column vector x_i. The set A is constructed by adding to the observed points the n points

$$(\infty, 0, \quad . \; . \; . \quad, 0)\; (0, \infty, \quad . \; . \; . \quad, 0) \quad . \; . \; . \quad, (0, 0, \quad . \; . \; . \quad, \infty).$$

Just as in two dimensions pairs of points in A defined lines and line-segments, so now sets of n points in A define hyperplanes and "facets". Here "facet" is used to describe that part of a hyperplane whose points can be expressed as weighted averages, with non-negative weights, of the n defining points. The efficient isoquant is now a surface S in n dimensions, composed of such facets.

To the equations (1) there corresponds the matrix equation

$$[x_i, x_{i+1}, \quad . \; . \; . \quad, x_{i+n-1}]\lambda = x_k \qquad . \quad . \quad . \quad . \qquad (3)$$

whose solution is the column vector λ, and the facet defined by the n points P_i, P_{i+1}, . . . , P_{i+n-1} is part of S if and only if

$$\lambda' u \geqslant 1 \text{ for all } P_k \text{ in } A \qquad . \quad . \quad . \quad . \quad . \quad . \qquad (4)$$

(Here u is a column vector, all whose elements are unity.) As before, the technical efficiency of P_k may be defined either as $1/(\lambda' u)$ for that facet intersected by OP_k, or as the maximum of $1/(\lambda' u)$ for all facets of S.

The relaxation of the single-product assumption is slightly more complex. Since output is no longer a scalar quantity, it is no longer possible to reduce the observations to points on our isoquant diagram by dividing inputs by output. Instead, each observed firm has a vector X_i of outputs and a vector x_i of inputs (note the change in notation) and must be represented by a point in $n + m$-dimensional space. The efficient surface S is now composed of facets determined by sets of $n + m$ points, chosen from a set A including, besides the observed points and the points at infinity, the origin. As constant returns are still assumed, the definition of a facet is changed to permit the origin a negative weight. It follows that the origin appears in every efficient facet.

The counterpart of equations (1) and (3) is now the matrix equations

$$\left.\begin{aligned} [X_i, X_{i+1}, \quad . \; . \; . \quad, X_{i+m+n-2}, 0]\lambda &= (\lambda' u)\; X_k \\ [x_i, x_{i+1}, \quad . \; . \; . \quad, x_{i+m+n-2}, 0]\lambda &= x_k \end{aligned}\right\} \qquad . \quad . \quad . \quad . \qquad (5)$$

equivalent to $n + m$ linear equations. The counterpart of conditions (2) and (4) is that

$$\lambda' u \geqslant 1 \text{ for all } P_k \text{ in } A \qquad . \quad . \quad . \quad . \quad . \quad . \qquad (6)$$

and the efficiency of P_k is defined in terms of $\lambda' u$ precisely as before.

It may not be immediately obvious that this criterion is simply a generalization of the previous ones. It can, however, easily be shown that in the case where $m = 1$ (so that X_i is a scalar) the two procedures are equivalent. Let $\lambda = (\lambda_1, \lambda_2, \ldots, \lambda_{n+1})$ be the solution of (5) and define $\mu = (\mu_1, \mu_2, \ldots, \mu_n)$ by

$$X_{i+j-1}\,\lambda_j = X_k\,\mu_j \qquad j = 1, 2, \ldots, n.$$

Then the equations (5) may be written

$$\left.\begin{aligned} \lambda' u &= \mu' u \\ \left[\frac{1}{X_i}x_i, \frac{1}{X_{i+1}}x_{i+1}, \ldots, \frac{1}{X_{i+n-1}}x_{i+n-1}\right]\mu &= \frac{1}{X_k}x_k \end{aligned}\right\} \qquad (7)$$

It will be seen that (7) consists of (3) re-written in the present notation, plus the statement that $\lambda' u = \mu' u$, i.e. that the two criteria are equivalent.

The interpretation of the technical efficiency of P_k defined in this way is precisely the same as before. If an efficient firm were to produce outputs X_k, it could do so from inputs $1/(\lambda' u)\, x_k$; or, using inputs x_k, it could produce outputs $\lambda' u\, X_k$. Thus, except for the need to change the geometrical picture, this generalization, too, is quite straightforward.

Where there is more than one output, it would be natural to extend the measure of price efficiency to cover the extent to which the firm's choice of outputs is adapted to the prices of these outputs. This has not been done, largely because, as will be argued later, price efficiency is in any case a measure with rather limited usefulness, so that further elaboration would be of rather academic interest. A measure of price efficiency defined as before remains a valid measure of the firm's adaptation to factor prices.

2.4. *Increasing and Diminishing Returns to Scale*

It is, unfortunately, more difficult to relax the assumption of constant returns to scale. It is quite simple to allow for diseconomies of scale. All that is necessary is to alter the definition of a facet in the last section so as not to permit a negative weight to the origin, and equations (3) so as to permit any set of $n + m$ points from A (not necessarily including the origin) to determine a facet. But there is no entirely satisfactory way of allowing for economies of scale.

The difference between the two cases is illustrated in Diagram 3 for the simple case of one input and one output. Whereas under diseconomies of scale, the efficient production function S is convex, so that the average of two points on S is attainable (indeed, probably inefficient), in the case of economies of scale this is not so. This is a serious matter, as the whole method is based on the assumption of convexity. It means that the method will give an optimistic instead of a conservative estimate of S—some straight line like OR—and, of course a pessimistic estimate of the efficiency of any point. Indeed, none of the justifications of the method advanced in previous

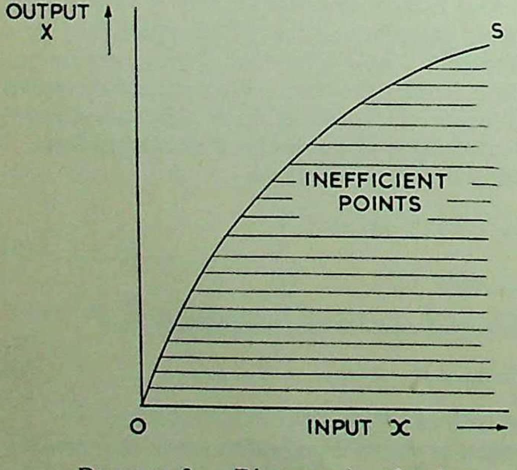

DIAGRAM 3*a*.—Diseconomies of scale.

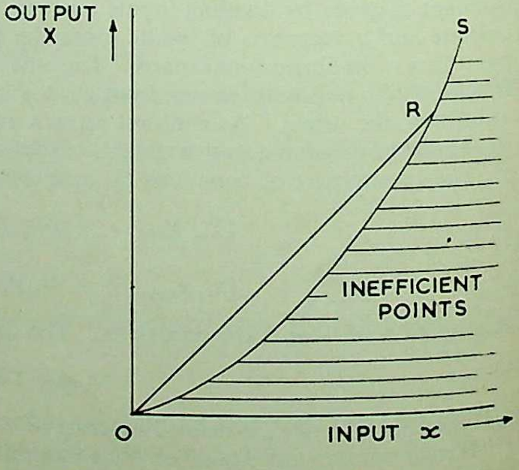

DIAGRAM 3*b*.—Economies of scale.

sections will hold, and furthermore, the estimated S will always show apparently constant returns.

The only practical method of dealing with this problem seems to be that of dividing the observations into groups of roughly equal output, and applying the method to each of these groups separately, the assumption being that returns are constant within a group to a sufficient degree of approximation. This will yield a different efficient isoquant for each level of output, and comparison of these isoquants will show the extent and nature of the economies of scale.

This device should work satisfactorily if there are a sufficient number of observations, but it clearly needs many more observations than the original method. Fortunately, where economies of scale are likely to be important, as in measuring the efficiency of firms, or plants, there are likely to be relatively many observations; while, in those cases where there are likely to be only a few observations, as where the efficiency of an industry is measured, economies of scale are likely to be unimportant. It is worth noting that the relevant economies of scale are those where a larger industry permits greater specialization by firms; for those "economies of large-scale industry" that take the form of cheaper inputs are irrelevant, while it is unlikely that the method will be applied to industries that are too small to permit firms of optimum size.

Of course, where economies or diseconomies of scale are present there are two measures of technical efficiency. The given output would have been efficiently produced with only a fraction e_1 of the given inputs, and $1/e_2$ times as great an output could have been produced from the given inputs, where e_1 and e_2 are not in general equal except under constant returns to scale. The ambiguity is, of course, systematic, and the choice of measure must depend on whether it is desired to produce the given output from minimal inputs, or maximum output from the given inputs.

If in practice there are several inputs but only a single output, it will be sensible to choose e_1 as the measure of efficiency, as this involves measuring the scale of production in terms of a single variable, output. Otherwise, the scale of production is a vector and not a scalar quantity. The practice of using a single input as a measure of scale—as labour in manufacturing, or land in agriculture—is clearly indefensible, and leads to obvious anomalies.

2.5. *Quasi-factors*

It is well known that production depends on certain factors besides those generally regarded as factors of production—air and water, for instance, and climate and location. The treatment of such *quasi-factors* must, of course, be determined *ad hoc*, but certain general observations may be made. Air and water, for example, are really perfectly ordinary factors of production, save that their price is (or may be) zero. They should therefore be treated accordingly, and the method has the advantage that any deviation of the price from zero can readily be dealt with.

Other quasi-factors, like the thickness of seam in coal-mining, are more difficult to deal with in that, although they undoubtedly influence the output obtained from a given set of inputs, diminishing returns to the other factors will not set in if the input of the quasi-factor is held constant. The simplest and most obvious solution to this problem is analogous to that of economies of scale, that is, to divide the observations into groups homogeneous (to a desired degree of approximation) in the quasi-factor. Alternatively, if the quasi-factor lends itself to representation by a continuous variable a method analogous to that for diseconomies of scale can be used.

3. INTERPRETATION

3.1. *The Technical Efficiency of a Firm*

One of the most important features of the method outlined above is the distinction between price and technical efficiency. The former measures a firm's success in choosing an optimal set of inputs, the latter its success in producing maximum output* from a given set of inputs. This distinction is quite a natural one, but it has also the merit that most of the difficulties are associated with price efficiency, leaving technical efficiency as a relatively uncomplicated measure. For example, even a firm's operating above or below its optimum output will affect only its price efficiency. Even so, there are a number of qualifications that must be made in interpreting technical efficiency.

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* It will be convenient in this discussion to talk as though there were a single product. It is easy to see how the arguments should be extended to cover several.

In the first place, the definition chosen of the efficient production function means that a firm's technical efficiency is relative to the set of firms from which the function is estimated. If additional firms are introduced into the analysis, they may reduce, but cannot increase, the technical efficiency of a given firm. This, of course, is perfectly natural—a firm may be highly efficient by British standards, but not by world standards.

A more important qualification arises in respect of the measurement of inputs. It is not proposed to discuss here the practical difficulties of measuring particular inputs, or the old riddles about the measurement of capital and the length of the working week. These problems remain, and are often formidable, but there is also a problem of more general interest. No one would dispute that the output produced from *given inputs* is a genuine measure of efficiency, but there is room for doubt whether, in a particular application, the inputs of a given firm are really "the same" as those represented by the corresponding point on the efficient isoquant. Of course, if, as has been implicitly assumed, there are a small number of homogeneous factors of production, each of which can be measured in physical units, there is no problem. It is perhaps trivial to point out that the omission of one of these factors would give a firm that used relatively much of this factor a relatively high technical efficiency. But so would quality differences in a factor favour a firm using a relatively high grade, and, in practice, factors of production are notoriously heterogeneous.

It is important to note that mere heterogeneity of factors will not matter, so long as it is spread evenly over firms. It is when there are differences between firms in the average quality (or, more strictly, in the distribution of qualities) of a factor, that a firm's technical efficiency will reflect the quality of its inputs as well as the efficiency of its management. If these differences in quality are physically measurable, it may be possible to reduce this effect by defining a larger number of relatively homogeneous factors of production, but in practice it is never likely to be possible completely to eliminate it.*

Thus, the technical efficiency of a firm must always, to some extent, reflect the quality of its inputs; it is impossible to measure the efficiency of its management entirely separately from this factor. This is, indeed, as it should be, for it is never possible to decide precisely how far the fertility of a particular farmer's land is due to nature and how far to good husbandry, how far the laziness and intractability of a particular firm's labour force is ingrained and how far the product of bad management.

Technical efficiency, then, is defined in relation to a given set of firms, in respect of a given set of factors measured in a specific way, and any change in these specifications will affect the measure. This is inevitable in any such measure. But with these qualifications it functions in a natural and satisfactory way as a measure of efficiency.

3.2. *The Price Efficiency of a Firm*

The price efficiency of a firm also depends on the measurement of inputs, but in a rather complex way, so that such problems are best discussed *ad hoc*. It depends, too, on the set of firms in the analysis, and is indeed much more sensitive to the introduction of new firms than is technical efficiency. It can be seen from Diagram 2 that OR/OQ, the price efficiency of P, depends on the slope of AA', the slope of SS' at Q, and its curvature between Q and Q'. The introduction of fresh observations is likely, if it affects this part of SS' at all, to affect both the slope and the curvature, in either direction and perhaps quite substantially. This would have a large effect on the price efficiency of P, as indeed would a change in the slope of AA'. Thus price efficiency is very sensitive to the introduction of new observations and to errors in estimating factor prices, so that it is likely to be rather unstable. Of course, in a particular case where many observations and accurate price information were available, it might be quite reliable.

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* Capital presents a particularly difficult problem. The relative productiveness of two different sets of equipment may vary considerably according to the amounts of other inputs. That is to say, each must be represented by a production function of a different shape. In this case, it would be impossible to measure capital by any scalar quantity, such as a money value. It is only when the functions are of roughly the same shape, so that relative productiveness does not vary much with changes in other inputs, that measurement in money terms will be satisfactory.

The treatment of capital must, then, be to define a number of different sorts of capital, each homogeneous in this sense, and to measure each in either physical units or money terms (say, replacement cost). It is to be hoped that in practice a small number of sorts of capital will suffice!

The use of the straight line AA' (see Diagram 2) in defining price efficiency, implicitly assumes a perfectly elastic supply of each factor. In fact, the elasticities of supply will usually be positive, so that changing the proportions of the inputs will change their price ratios, those of which relatively more is used becoming relatively dearer. This means that OR/OQ will tend to underestimate the true price efficiency of the firm.

However, over and above these problems there remains the question of whether a high price efficiency is necessarily desirable. It may be that a firm's best policy is to operate for a time well above optimum output (in a period of expansion, say, or one of temporarily high demand) even though this gives it a low price efficiency.* Similarly, a firm whose inputs are adjusted to past or expected future prices may not be inefficient. In short, its price efficiency measures the extent of a firm's adaptation to a particular set of prices, and will therefore provide a good measure of its efficiency in adapting to factor prices only in a completely static situation.

Thus, price efficiency is a measure that is both unstable and dubious of interpretation; its virtue lies in leaving technical efficiency free of these faults, rather than in any intrinsic usefulness. From the point of view of the planner, the technical efficiency of a firm or plant indicates the undisputed gain that can be achieved by simply "gingering up" the management, while its price efficiency indicates the gain that, on certain assumptions about the future price structure, can be obtained by varying the input ratios.

3.3. *Price and Technical Efficiency of an Industry*

For the purpose of this exposition, an industry may be regarded as any group of firms making the same product. It might be thought, then, that the technical efficiency of an industry with respect to a given efficient isoquant would be simply a weighted average† of the technical efficiencies with respect to the same isoquant of its constituent firms. This is basically true, but it needs to be qualified, for in so far as its constituent firms use their inputs in different proportions, this dispersion will reduce the technical efficiency of the industry. This can easily be seen from the fact that the average of two points on SS' will in general lie beyond SS'—that is, that two firms which, taken individually, are perfectly efficient (technically, but not, of course, price-wise) are not perfectly technically efficient when taken together. By the same token, this dispersion will mean that the price efficiency of the industry will be greater than the weighted average of the price efficiencies of the constituent firms. (The over-all efficiencies must, of course, be the same.) Thus horizontal aggregation leads to the transfer of some elements of inefficiency from price to technical efficiency. It can easily be seen that the same is true of vertical aggregation.

Thus, with respect to a given efficient isoquant, the technical efficiency of an industry will tend to be somewhat less than the weighted average of the technical efficiencies of its constituent firms. But in an application of the method to industries, it is to be expected that the efficient isoquant will itself be estimated from observations of industries. What will be the effect of aggregation on the efficient isoquant? The answer is simply that it will lead to a less optimistic estimate. Except in very special circumstances, the combination of two or more efficient points will shift SS' away from the origin; so will, *a fortiori*, the combination of efficient and inefficient points. Conversely, of course, disaggregation will push the efficient isoquant closer to the origin.

With these qualifications, the method can be applied to industries in the same way as to firms. The set of observations may be either international, say the steel industries of a number of different countries, or inter-regional, say the agricultural industries of each of the 48 American states (the illustrative example given below). The latter is easier, in that many of the problems of comparability of data are avoided, but the former is in many ways more interesting.

It may be worth noting that whereas in the case of firms, if economies or diseconomies of scale are present, it will usually be desirable to allow for them in the ways mentioned in section 2.4, where industries are concerned this will not be so. The extent to which its firms are of optimum size may legitimately be considered part of an industry's efficiency. Of course, if an industry were too small to permit its firms to achieve optimum size or the optimum degree of specialization, it might be desirable to allow for this.

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* A period of temporarily *low* demand might lead to short-time working. If labour input were measured in man-hours, as is conceptually correct, this too would affect price efficiency, but if in number of men employed, it would affect the firm's technical efficiency.

† Weighted by output.

3.4. *The Structural Efficiency of an Industry*

In principle, an industry analysis can be carried out quite satisfactorily as outlined above, but in practice the scarcity of data is a serious limitation. It is notoriously difficult to obtain comparable input measurements in an international analysis, while the number of cases in which an inter-regional analysis will provide as many observations as United States agriculture is small. (There are few countries with so many regions as the United States, and few industries so ubiquitous as agriculture.) In any case, international comparisons are likely, where they are possible, to be the more interesting, and here it is likely to be difficult to obtain sufficient observations (on a comparable basis) to provide a satisfactory estimate of the efficient isoquant.

There is, however, a very satisfactory way of getting round this problem: that is, by comparing an industry's performance with the efficient production function derived from its own constituent firms. The "technical efficiency" of an industry, measured in this way, will be called its *structural efficiency*, and is a very interesting concept. It measures the extent to which an industry keeps up with the performance of its own best firms. It is a measure of what it is natural to call the structural efficiency of an industry—of the extent to which its firms are of optimum size, to which its high-cost firms are squeezed out or reformed, to which production is optimally allocated between firms in the short run.

It is open to the criticism that it does not reflect the extent to which the best practice in an industry compares with the best practice elsewhere. Thus an industry whose firms were uniformly inefficient would have a higher structural efficiency than one where there were both efficient and inefficient firms. This means that, in an international analysis, a comparison of structural efficiencies should be supplemented by a comparison of efficient isoquants, as described in section 3.5 below. But the introduction of the concept of structural efficiency confers two advantages. First, it permits a comparison of some of the most important aspects of efficiency to be made, even where the incomparability of input measurements precludes a comparison of efficient isoquants. Secondly, where some sort of comparison can be made of efficient isoquants, it separates this (probably rather shaky) comparison from the more reliable one of structural efficiencies.

However, perhaps the greatest importance of the concept is due to the fact that it is possible to compare the structural efficiencies of industries producing different commodities—where, of course, any comparison of efficient isoquants is out of the question. It is thus a measure that could well be used in empirical work on the comparative efficiency of economic systems. It may not measure *precisely* what the economic theorist wishes to measure, but it does measure a surprisingly large proportion of the relevant factors, and it may well be the best measure available in practice. It is hoped to develop this argument further elsewhere.

3.5. *The Efficient Production Function*

The method provides, as a sort of by-product, an estimate of the efficient production function which can be put to various uses, some of which have already been indicated. This section will be devoted to seeing what those uses are and how well the method of estimation is suited to them. It will not be surprising if the method of estimation is not the best for any particular use, for it was chosen simply as providing the best measure of technical efficiency.

One virtue of the estimate is that it involves no assumptions about the shape of the isoquant, other than negative slope and convexity. This is a considerable advantage for, as in so many econometric problems, the data are likely to be such that quite a variety of specific functions might yield a "good fit". (Indeed, in the absence of *a priori* reasoning about the shape of the function and the empirical testing of a wide variety of possible functions, little significance can usually be attached to a "good fit" in an econometric study.) Thus the present estimates can, by virtue of their freedom from special assumptions, cast useful light of the shape of efficient isoquants in practice.

An obvious use of an efficient production function is to provide estimates of the marginal rates of substitution of pairs of factors at various points. These estimates, like the related measure of price efficiency, are likely to be somewhat unstable, and it might be argued that some estimate based (by ordinary regression methods) on all the observations might be preferable. It would certainly be more stable, but as it would be a marginal rate of substitution in average rather than in best practice, its preferability would depend on which of these concepts it was desired to measure.

There remain a group of uses that are formally similar—they all involve comparing two or more efficient isoquants derived in different circumstances. As has been said, in an international comparison it is desirable, if possible, to compare the best practice of the various countries as well as their structural efficiencies. This is done by comparing their efficient isoquants. Again, investigating economies of scale is done by deriving an efficient isoquant for each size-group of arms and comparing them. Finally, to investigate technological progress, efficient isoquants are derived for an industry at different points in time and then compared.

In any of these cases the isoquants may cross, possibly at more than one point. In the first two cases this is obvious, but in the third case it requires some comment, as it would seem to imply forgetfulness on the part of our technologists! In fact, it would reflect a decline in the quality of some input—for example, land in the American Dust Bowl, or labour in Britain. Whether they cross or not the isoquants may well be of very different shapes, and thus contain a great deal of information that would be lost in any point-comparison; indeed, the latter might be wildly misleading.

3.6. *The Method of Estimation from a Statistical Viewpoint*

As the efficient production function is defined in section 2.2, there can be no doubt that the method chosen is the appropriate way of estimating it. It does give the most conservative function consistent with the observations and with the assumptions of convexity and non-positive slope. However, if one looks at it from a statistical point of view, one may wish to reformulate the problem as follows. There exists some efficient function, from which all the observed points deviate randomly but in the same direction. What, then, is the best estimate of the function?

It might seem at first sight as though the method chosen involved a great waste of information—only a handful of "efficient" points contribute directly to the estimate, the rest being "ignored". However, in the problem of estimating the extremes of the rectangular distribution, the relevant information is all contained in the extreme observations, and this seems to be an analogous case. On the other hand, the estimate chosen will almost certainly have a pessimistic bias (that is, a bias away from the origin) but in the absence of *a priori* knowledge of the distribution of the deviations, it is impossible to remove this bias.

Similarly, errors of observation will introduce an optimistic bias, which can only be eliminated if the distributions of both errors and efficiencies are known. This is an interesting problem for any theoretical statistician but for practical purposes the important fact is that if the errors are small compared with the variation in efficiencies, this bias will be negligible. In fact, the problem is more important in the illustrative example than it is ever likely to be in an industrial application. The variation in harvests in agriculture is much larger than genuine errors of observation are likely to be, although it is not strictly variation from year to year in harvests that matters, but differential variation. That is to say, it would not matter that all harvests were bad, but it would matter if some States suffered more than others.

4. PREVIOUS METHODS

4.1. *The Average Productivity of Labour*

Of the efficiency measures previously used, perhaps the most popular, and certainly the least satisfactory, was the average productivity of labour.* Ignoring as it does all inputs save labour, this measure is so obviously unsatisfactory that one would not waste space discussing it, were it not for the danger that its popularity with the general public may do the economy serious harm through over-capitalization. Very often, the easiest way to increase the average productivity of labour in an industry is to use more machinery, and this, pushed beyond a certain point, will lower the price efficiency of the industry. Indeed, it is quite possible for a country to increase "productivity" in every industry and yet to lower its standard of living: the reduction in the

* See, for example, Rostas (1948).

numbers employed in the consumer goods industries outweighs the increase in their average productivity.

Economists are well aware of this possibility, and in using the concept must have at least implicit reservations on this point. However, the concept has been widely adopted by the public, and one finds it in frequent use amongst politicians, trades unionists, journalists and the like. It would be surprising if all these people made the appropriate qualifications when using the concept, and indeed it seems that there is a large body of public opinion that believes any increase in "productivity" to be desirable. It may be argued that this does not matter, as the price mechanism would prevent any over-capitalization; but public opinion has nowadays little respect for the price mechanism, and might easily urge that it be over-ridden in the interests of "increased productivity". It is perhaps not too much to say that the eradication of "productivity" as a criterion of efficiency could make a considerable difference to our future standard of living.

4.2. *Indices of Efficiency*

The unsatisfactory nature of the "productivity" criterion led naturally to attempts to produce measures of efficiency that took account of all factors of production by adding up (in some sense) a firm's inputs of the different factors. They sought to represent the vector of inputs by a scalar—to add up land, labour, materials, capital and so on into a single quantity. They were naturally doomed to failure—one simply cannot add men and acres, pounds and horsepower.

To be more precise, one cannot add them without (explicitly or implicitly) pricing them, and the choice of a set of prices introduces an arbitrary element into the measure. The indices all included a weighted average of inputs, and whatever it is intended to represent, such a weighted average is always equivalent to a valuation of the inputs at prices proportional to the weights. Thus, any attempt to compare efficiency by such an index may be regarded as making a cost comparison, with a set of factor prices proportional to the weights in the index. The difficulty is simply that of choosing a suitable set of prices as a basis for the comparison. Of course, if all the firms in the analysis face the same set of prices, it is natural to choose that set; but then the "index" boils down to a simple cost comparison. Otherwise, the choice of a set of prices must be arbitrary, and leave at least one of the firms facing prices different from those chosen. In this case, the index will reflect not only the technical efficiency of the firm but also the extent of its adaptation to a set of factor prices different from those facing it.

4.3. *Cost Comparisons*

The most obvious measure of a firm's efficiency is its costs. Comparisons of costs must clearly be limited to situations where all the firms compared face the same factor prices, but in such cases they constitute a much better criterion than "productivity" and are equivalent to the best "efficiency index". That they have drawbacks even in such cases can be seen from the fact that a firm's costs are proportional to its over-all efficiency, so that cost comparisons are open to the same objections as price efficiency—principally, objections arising from the possible divergence of past or future factor price ratios from their present values. It is the virtue of the present method that it separates price from technical efficiency, and leaves the latter free from these criticisms. It is perhaps worth noting that, in analysing any particular group of firms, the lowest cost firm must be technically perfectly efficient, but so, in general, will a number of other firms.

5. THE COMPUTING PROBLEM

5.1. *The Methods Used*

This section will discuss the computing problem actually met in the illustrative example, that is, where there are n inputs, one output and constant returns to scale.* The problem is essentially very simple, as a glance at section 2.3 will show. (4) is a necessary and sufficient condition for the n vectors on the left-hand side of (3) to define an efficient facet. An obvious solution is thus to solve (3) for all possible combinations of n points in A, and to determine in each case whether (4) is satisfied. If there are m observed points, this involves $N = {}_{n+m}C_n$ matrix inversions and an equal number of matrix multiplications. Thus number would usually be prohibitively large for desk computers, but not for EDSAC for the moderate values of n and m met in the illustrative example. Here there were 48 observed points, but a large proportion of these could usually be eliminated from the calculation by inspection as clearly inefficient. Thus a typical case might give $n = 4$, $m = 12$ so that $N = 1{,}820$—a calculation that would take EDSAC about two hours. However, quite small increases in n and m could make the task large even for EDSAC. If $n = 5$, $m = 20$, $N = 55{,}130$ and the computation would take about 60 hours.* Thus the obvious method is practicable only for a very limited range of values of n and m.

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* The methods discussed below can easily be adapted to cases of many outputs or decreasing returns to scale, so that computing problems introduce no restrictions additional to those discussed in section 2.4.

There are also difficulties associated with the points at infinity in A. The most attractive method of dealing with them is simply to treat them on a par with the other vectors, substituting a suitably large number for the infinite element. Unfortunately, the capacity of the existing matrix inversion sub-routine for EDSAC is not large enough to permit an adequate approximation to infinity, so that this method could not be used. The alternative is to obtain inverses for matrices with infinite elements by inverting lower order matrices and bordering them appropriately. This may well be the best method in the long run, but it was felt that the complexity of the EDSAC programme involved would be greater than was justified by the present exercise.

Accordingly, the following method of avoiding the difficulty was adopted. Those facets defined by n observed points were christened "interesting" and those involving points at infinity "uninteresting", and the two sets of facets were computed separately. The interesting facets were computed by a simple adaptation of the above method. The exclusion of the points at infinity has two advantages. It avoids the problem mentioned above, and somewhat reduces N. Thus ${}_{12}C_4 = 495$ representing only half-an-hour's work, and ${}_{20}C_5 = 18{,}504$, representing about 20 hours' work. It also has a disadvantage—a separate criterion for non-positive slope must be introduced. Write B for the $n \times n$ matrix corresponding to a facet and C for the $n \times m$ matrix whose columns are the observed vectors. Then B corresponds to an efficient facet if and only if the column sums of B^{-1} are $\geqslant 0$ and those of $B^{-1}C \geqslant 1$. This procedure, which can easily be programmed for EDSAC, was used to find the interesting facets.

If n is small enough, the uninteresting facets can be filled in by elementary geometry, but as n increases the intuition sooner or later fails (in the author's case, at $n = 4$). However, it is possible to "build up" the uninteresting part of the efficient surface in n dimensions from a knowledge of the efficient surfaces in $n - 1$ dimensions. An efficient surface in $n - 1$ dimensions can be regarded mathematically as the efficient surface obtained when the observed points are projected orthogonally on to one of the co-ordinate hyper-planes, or from an economic point of view as that obtained when one of the inputs is ignored. It is clear that there is a one-to-one correspondence between the efficient facets in n dimensions that are parallel to a particular co-ordinate axis and the efficient facets in the hyper-plane orthogonal to that axis. Thus, if the n efficient surfaces in $n - 1$ dimensions are known, it is a simple matter to fill in the uninteresting facets in n dimensions.

It might seem that the calculation of efficient surfaces in lower-dimensional space is a considerable and unnecessary addition to the computing burden, but in fact it is not large enough to offset the reduction produced by omitting the points at infinity, and, in addition, is helpful in interpreting the analyses (see section 6.3).

5.2. *More Sophisticated Methods*

Although these methods proved adequate to this particular problem, it is clear that problems could easily arise for which the simple testing of all possible hyper-planes would be prohibitive even on EDSAC. Some more selective procedure is clearly necessary. One method is to start with an efficient facet (there are always some that can be written down from inspection) and then to determine all the neighbouring efficient facets, and so on, systematically.

It can be shown that, given an efficient facet passing through $x_1, x_2 \;\ldots\; x_n$, its neighbouring efficient facet on the side away from x_n passes through $x_1, x_2 \;\ldots\; x_{n-1}, x_k$ where x_k is that one of the remaining observed points for which $\lambda_n < 0$ and for which

$$\frac{\sum_i \lambda_i - 1}{|\lambda_n|}$$

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* The new machine EDSAC II (at present under construction) could complete this calculation in about an hour; but the point, of course, remains that the number of inversions rises very rapidly with n and m.

is least. (This condition must be modified in an obvious way of x_n is a dummy vector.) In this way, the number of inversions can be reduced to the number of efficient facets, with a very considerable saving in computing.

However, Mr. P. Swinnerton-Dyer has suggested the following very elegant method, which has the great advantage of side-stepping the matrix inversions. His method consists in starting with the efficient surface corresponding to a very small number of observed points (that for one observed point may be written down from inspection) and successively introducing the remaining points, amending the surface as necessary.

Suppose the equations of the facets of the efficient surface corresponding to a given set of observed points are written in the form $L_i(x) = 0$ (with $L_i(0) < 0$) and the further point x_k is introduced. Then if $L_i(x_k) > 0$ for all i, x_k is not an efficient point and the surface is unchanged. If however $L_i(x_k) < 0$ for some facets, these facets must be replaced by facets passing through x_k and the edges of the neighbouring facets.

That is to say, suppose that $L_i(x_k) < 0$ and for a neighbouring facet $L_j(x_k) > 0$. Then the amended surface will contain a facet passing through the intersection of these facets and through x_k. The equation of this facet may be written

$$L_j(x_k)\, L_i(x) - L_i(x_k)\, L_j(x) = 0$$

a form which is simple to calculate given $L_i(x)$ and $L_j(x)$. The method has to be modified in an obvious way where a facet parallel to one of the axes is discarded.

6. THE ILLUSTRATIVE EXAMPLE

6.1. *Choice of Illustrative Example*

The example chosen to illustrate the method is that of agriculture in the United States. The reasons for this choice are two. The first, and perhaps the more important, is the availability of information. American agricultural statistics give considerable information as to the agricultural inputs and outputs of each state. One is thus provided with measures of inputs and outputs, on a comparable basis, for 48 productive units. It is difficult to think of any other application where such adequate information is so readily available. Secondly, in so far as land plays a more important role in agriculture than in manufacturing, agricultural production has a more genuinely multi-variate nature than most industries. It might also be argued that agricultural outputs are more homogeneous than those of other industries, but against this must be set the heterogeneity of land inputs.

Thus, American agriculture provides an interesting application of the method, and one which is possible with relatively little preliminary work on the measurement of inputs and outputs. It should be emphasized, however, that it is intended purely as an *illustrative* application. The handy measures of inputs are not always the best, and a full-scale study of American agriculture by the method would require vastly more work on the measurement of several of the inputs than was worth while for the present purpose. At the least, one would need to make a detailed attempt to allow for the heterogeneity of land inputs, before one could draw more than the roughest inferences about American agricultural efficiency. This heterogeneity reflects differences in location and climate, as well as in fertility.

DIAGRAM 4 (key).

1. Australia.
2. Austria.
3. Belgium.
4. Canada.
5. Chile.

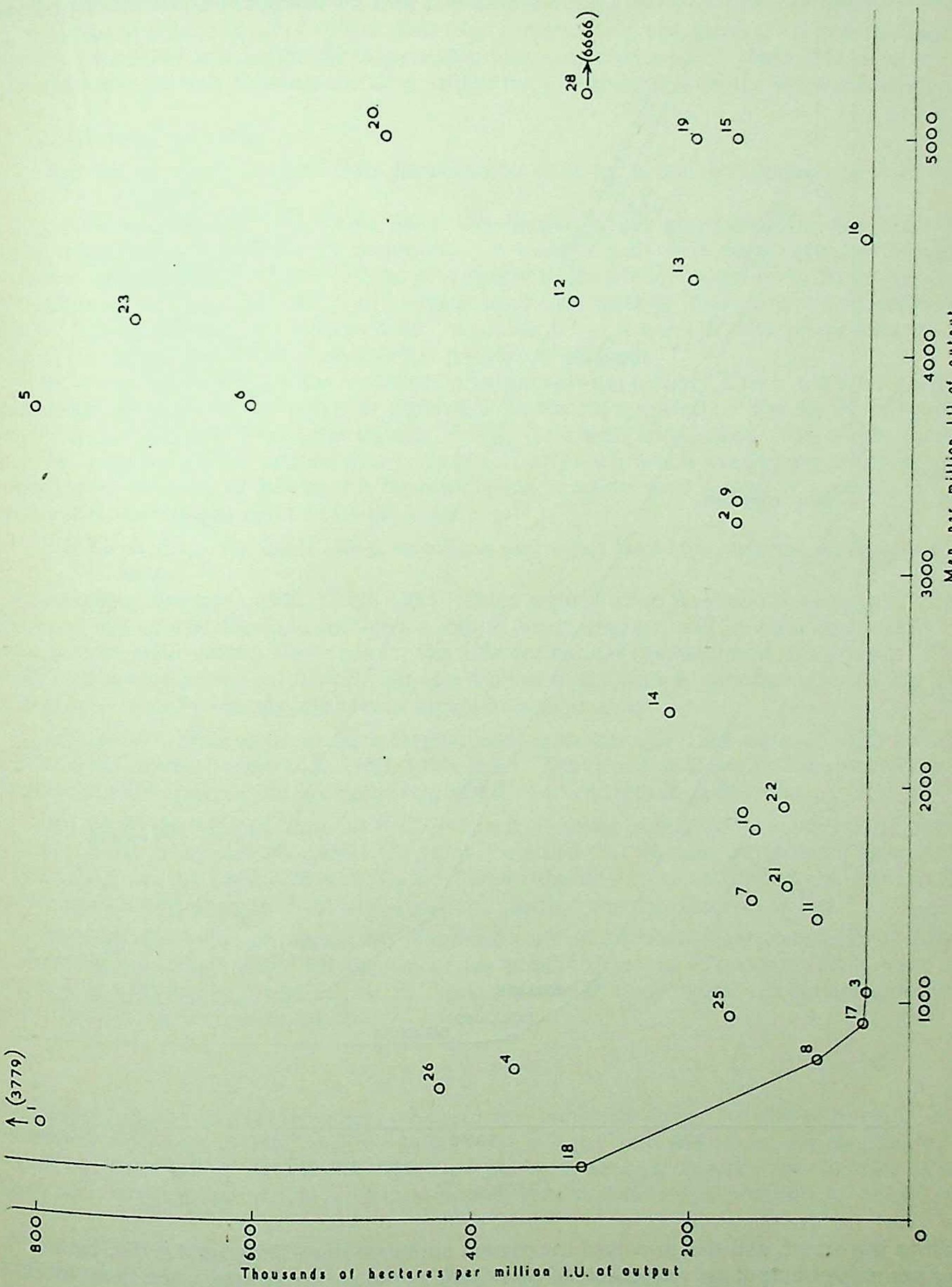
6. Columbia.
7. Cuba.
8. Denmark.
9. Finland.
10. France.

11. Germany.
12. Greece.
13. Hungary.
14. Ireland.
15. Italy.

16. Japan.
17. Netherlands.
18. New Zealand.
19. Poland.
20. Spain.

21. Sweden.
22. Switzerland.
23. South Africa.
25. United Kingdom.
26. United States.

28. Jugoslavia.


Thousands of hectares per million I.U. of output
Men per million I.U. of output
0
200
400
600
800
1000
2000
3000
4000
5000
1 (3779)
28 (6666)
1
2
3
4
5
6
7
8
9
10
11
12
13
14
15
16
17
18
19
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21
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25
26
28

DIAGRAM 4.

These considerations represent a justification of the illustrative example, but the actual history of the choice may also be of interest to society. In 1954 Mr. Colin Clark read a paper to the Society in which, *inter alia*, he compared the average productivity of agricultural labour in a number of countries. In the discussion it was suggested that the average productivity of land was at least as important, and this provoked the present author to draw Diagram 4. The figures for labour per unit of output are reciprocals of Mr. Clark's productivity figures, while those for land per unit of output were obtained from them by applying estimates of the number of workers per hectare, derived in most cases from the *F.A.O. Year Book*, 1952. Diagram 5 gives an enlargement of part of Diagram 4, with constant-efficiency contours drawn in. The positions of various

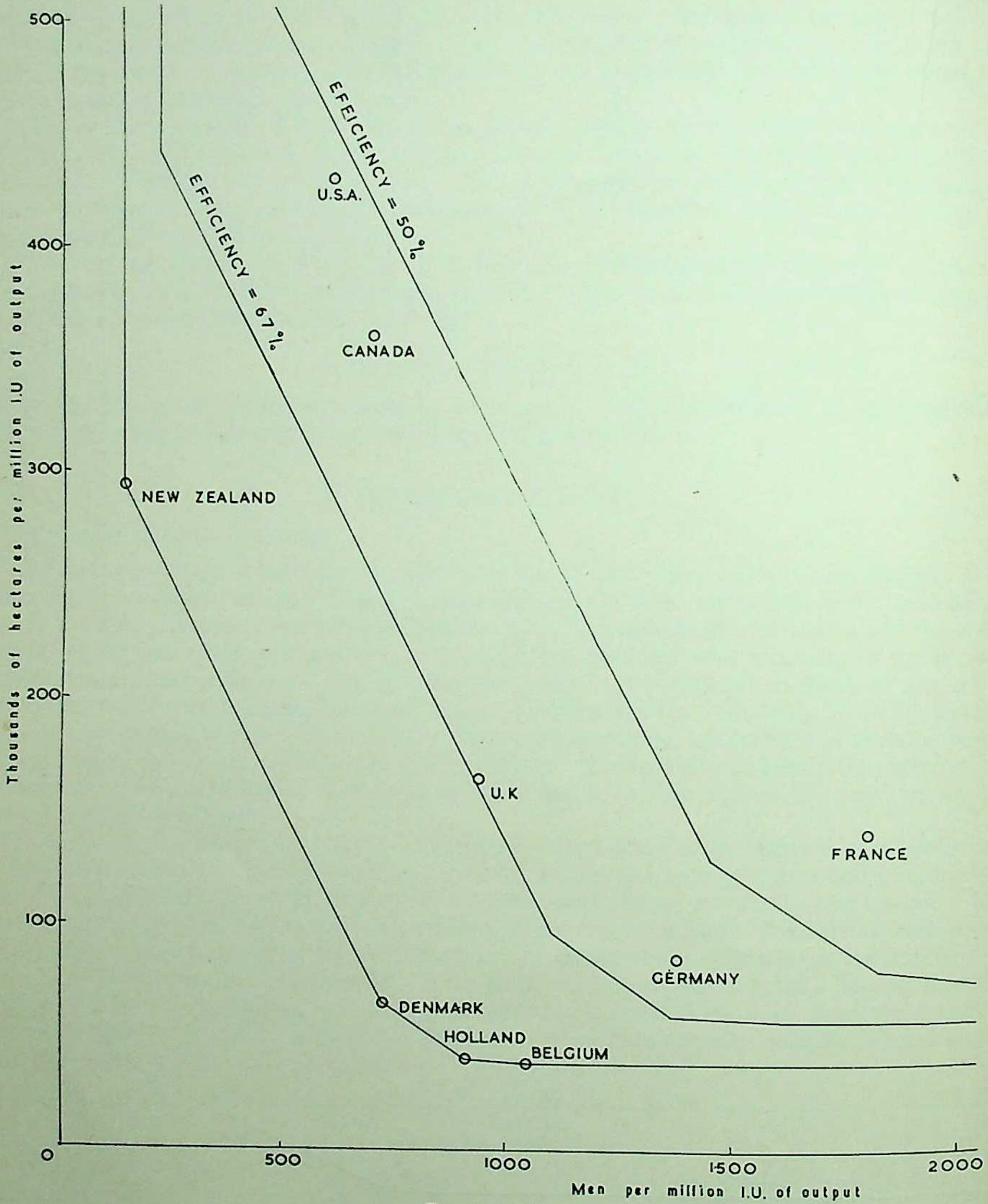

DIAGRAM 5.

countries on this diagram may be suggestive, but it must be emphasized that no firm conclusions may be drawn; the two diagrams are included only to indicate how Mr. Clark's international comparisons change when a second factor is introduced, and it is certainly not suggested that they themselves form an adequate basis for such comparisons.

The greatest objection is, of course, that they consider only two factors. It was felt that the difficulty of getting internationally comparable measurements made it preferable to apply the more elaborate analysis to American data, rather than to attempt it on the international figures.

6.2. *Definition of Variables*

(a) *Output*—Cash receipts from farming *plus* value of home consumption in millions of dollars.

(*Agricultural Statistics*, 1952, Table 700.) The figures include government payments, a mixed bag of subsidies and payments for restriction. It would ideally have been desirable to include the former and exclude the latter, but as government payments are never more than 3 per cent., and seldom more than 1 per cent., of a state's total cash receipts, this particular inadequacy is trivial. The figures were not corrected for "duplication" as it was felt that this problem is best handled by treating purchases of agricultural produce as an input.

A more serious problem is the variation in prices between states. This is a matter partly of heterogeneity of product and partly of distance from the main markets. The former is properly reflected in output, while the latter should, ideally, have been eliminated. This would be done either by revaluing all the outputs at a constant set of prices, which would have involved a disproportionate amount of labour, or by introducing location as a factor of production. An unsuccessful attempt was made to do the latter.

(b) *Land*—Land in farms *minus* woodland and other land not pastured, in thousands of acres.

(*Agricultural Statistics*, 1952, Table 637.) There seemed to be no obvious alternative to this definition. Of course, the land included is highly heterogeneous, and in a full-scale study one would have to make careful allowance for the different natural characteristics of the land. These include not merely geological factors, but also climate and location, although perhaps the latter are best dealt with by introducing special inputs or quasi-factors.

(c) *Labour*—Men on farms, including farmers, farm managers, and unpaid family workers.

(*Statistical Abstract of the U.S.*, 1953, Table 225.) The choice of definition here was somewhat arbitrary, but the analysis did not seem very sensitive to changes in definition.

(d) *Materials*—Expenditure on feed, livestock and seed purchased in thousands of dollars.

(*U.S. Census of Agriculture*, 1950.) It should be noted that whereas the previous figures refer to 1950, these are for 1949. This choice was largely dictated by the availability of information, but the implicit assumptions about time-lags in production seem as plausible as any.

(e) *Capital*—Value of implements and machinery on farms, 1950, in thousands of dollars.

These figures, which are a breakdown of his published results (Tostlebee, 1954) were very kindly made available to me by Dr. A. S. Tostlebee. From these figures the variables shown in Table 1 (p. 279) were constructed, thus:—

$$x_1 = \frac{b}{a}, \; x_2 = \frac{c}{a}, \; x_3 = \frac{d}{a}, \; x_4 = \frac{e}{a}$$

Thus, each of these variables represents the input of a particular factor per unit of output. Each observation represents a state, and the agricultural industry of a state must be regarded as an industry in the sense of section 3.4. Perhaps the most serious objection to the data is that agricultural output is notoriously subject to year-to-year variations (see section 3.6, above).

6.3. *The Estimates of Technical Efficiencies*

The estimates of the technical efficiencies of the states are given in Table 2 (p. 280). For each state there is a different estimate of efficiency, according to the factors of production included in the analysis. It will be seen that, in accordance with the theory developed in section 3, the introduction of a new factor of production into the analysis cannot lower, and in general, raises the technical efficiency of any particular state. Thus, the more factors that are considered, the more are apparent differences in efficiency explained as being due to differing inputs of these factors.

Of course, there is no doubt that the process could (and in principle should) be pushed beyond the four factors considered in this paper. That is to say, the apparent differences in efficiency in the final column of Table 2 reflect factors like climate, location and fertility that have not been included in the analysis, as well as "genuine" differences in efficiency. However, from drawing scatter diagrams, it was found that there was little correlation between the efficiencies of the final column and variables representing location, temperature and rainfall. It may well be objected that such a test depends too much on the selection of variables, and indeed on the judgment of whether or not the scatter diagram shows "appreciable" correlation. An analysis of variance was therefore applied to these efficiencies, the groups used being those shown in Table 2, which are the regional groupings normally used by the United States Department of Agriculture. It was found that the variance between groups was less than that within groups, so that it was unlikely that much of the difference in efficiencies could be explained in terms of variables whose variation was mainly between regions—as is the case in general with climatic and locational factors.

This does not mean, of course, that one must conclude that climate and location have no influence on agricultural production. A more likely explanation is that some of the factors already included are correlated with climate and location, and have already "explained" a good deal of the relevant variation in efficiency. The situation is thus analogous to the familiar one of a partial regression analysis where the pre-determined variables are highly inter-correlated, and the solution, too, must be analogous. That is, the analysis must, regretfully, be stopped at this point. It might be thought that some climatic and locational variables should be introduced while omitting some of the other variables, but, on the one hand, it is not clear how these factors should be represented, and on the other hand, it would be hard to justify an analysis from which any one of land, labour, capital and materials was missing.

It is perhaps also instructive to look at the frequency distribution of efficiencies, and Diagram 6 shows the distribution corresponding to each column of Table 2 in histogram form. It is to such frequency distributions that one must look for a measure of the success of the analysis, corresponding to the multiple correlation coefficient in regression analysis. It might seem at first sight that the obvious counterpart of the correlation coefficient was simply the average level of efficiency, being a measure of the extent to which differences have been explained in causal terms. But whereas in regression analysis the object is to explain as much of the variation as possible, here one does not wish to explain away all differences. In other words, one regards as ideal not an analysis that makes all states perfectly efficient, but one that leaves only "genuine" differences in efficiency, as discussed in section 3.1.

As any direct test of whether the differences in efficiency are genuine or not is impossible (except, of course, the sort of test described above) one is reduced to a consideration of the plausibility of the distribution of efficiencies. This is necessarily a highly subjective matter, and one hesitates to attempt to lay down any objective criteria of plausibility. Indeed, if one were to approach the matter in general terms it is doubtful whether one could secure agreement. Fortunately, in particular cases agreement seems much easier to obtain. Thus, in Diagram 6 people seem to agree that the distributions corresponding to $x_1x_2x_3x_4$ and $x_1x_2x_3$ are quite plausible while most of the others are not; and that the distribution for x_1x_4 is a good example of an implausible one.

In the latter case, there is a substantial group of states with very high efficiencies, and then a virtual gap until the 60 per cent. point; three-quarters of the states are less than 60 per cent. efficient. The reason why this is regarded as implausible is perhaps that one feels that there must be some neglected input accounting for the apparently very low efficiency of such a large group of states. The same sort of argument would apply to the other bimodal distributions. It is difficult to see any rational basis for regarding rectangular, J-shaped, or unimodal distributions as more plausible. Thus, it might be felt that the distribution for x_2x_3 was as plausible as those for $x_1x_2x_3$ and $x_1\ x_2\ x_3\ x_4$.

However, in comparing these three distributions (which seem to be the most plausible of those in Diagram 6) other criteria than the mere shape of the distribution may be introduced. Thus, in going from x_2x_3 to $x_1x_2x_3$, by introducing land (obviously an important factor of production in agriculture) one has clearly explained a good deal of the apparent variation in efficiency, so

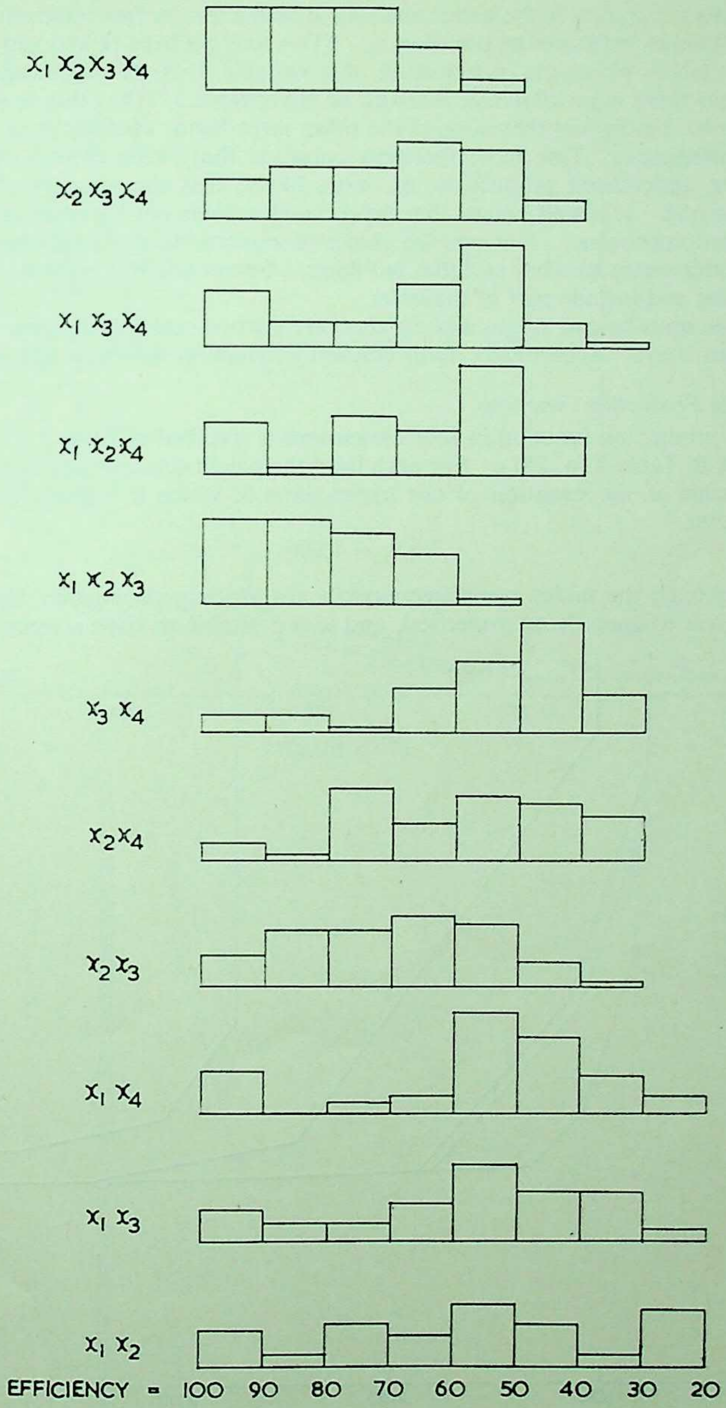
$x_1 x_2 x_3 x_4$
$x_2 x_3 x_4$
$x_1 x_3 x_4$
$x_1 x_2 x_4$
$x_1 x_2 x_3$
$x_3 x_4$
$x_2 x_4$
$x_2 x_3$
$x_1 x_4$
$x_1 x_3$
$x_1 x_2$
EFFICIENCY = 100 90 80 70 60 50 40 30 20

DIAGRAM 6.

that $x_1x_2x_3$ is clearly a better analysis. In going from $x_1x_2x_3$ to $x_1x_2x_3x_4$, by introducing capital (another relevant factor) only a little of the apparent variation in efficiency is explained. Thus although in principle $x_1x_2x_3x_4$ is the better analysis, it seems that in fact relatively little difference is made to the efficiency estimates by omitting x_4. (This may perhaps be thought of as analogous to a regression analysis where the introduction of a variable does not significantly improve the fit; although here there is no objective measure of significance.) That this is not due to inter-correlation is shown by the fact that none of the other three-factor analyses gives a very plausible distribution of efficiencies. One must therefore conclude that either capital does not play an important role in agricultural production, or, more likely, that the measure of capital used is not a satisfactory one. It is well known that depreciated value is not the ideal measure of capital input in a production process. Nor was the choice of implements and machinery entirely appropriate, as it includes motor cars but excludes buildings, whereas one would have wished to exclude most of the former and include part of the latter.

The conclusion must be that $x_1x_2x_3$ and $x_1x_2x_3x_4$ are the two best analyses, but that it is difficult to decide between them. Accordingly, both efficient production functions are given below.

6.4. *The Efficient Production Functions*

The efficient production function in four dimensions is specified in Table 3 (p. 281) and that in three dimensions in Table 4 (p. 281). For each facet the points determining it are given* together with the parameters of the equation of the hyper-plane of which it is part, the equation being written in the form

$$\Sigma a_i x_i = 1{,}000 \qquad . \quad . \quad . \quad . \quad . \quad . \qquad (8)$$

However, although the tables completely specify the efficient production functions, they do not lend themselves to analysis by inspection, and it is desirable to have a more graphic method

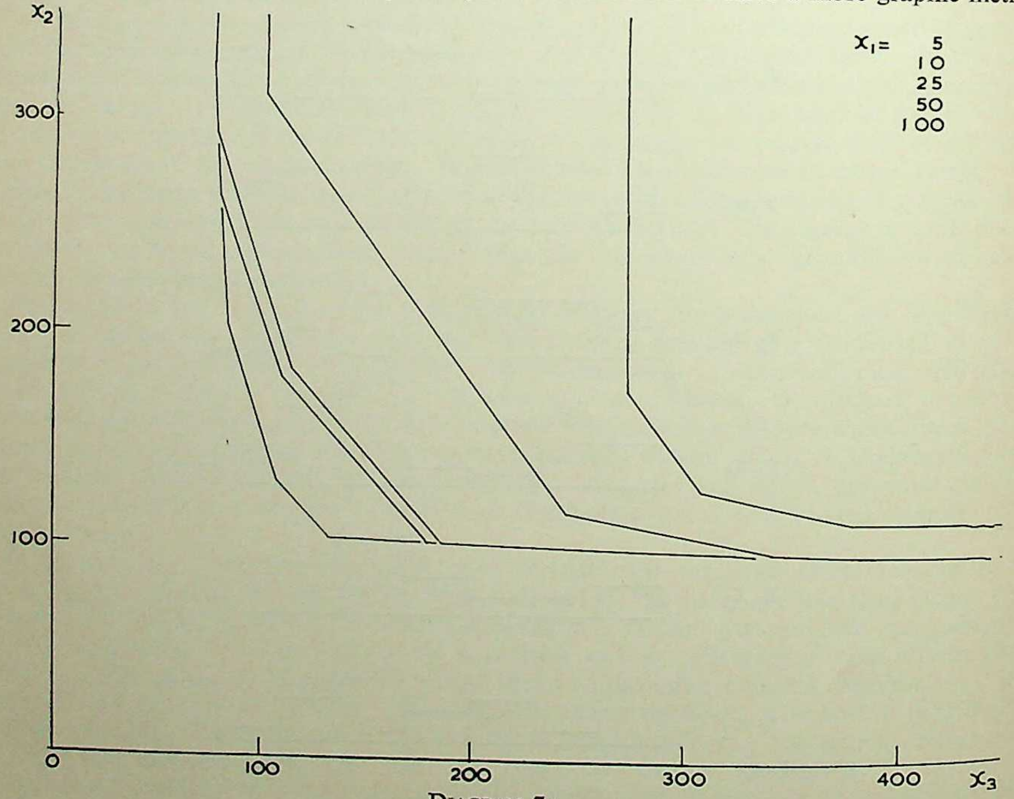

DIAGRAM 7*a*.

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* The observed points are numbered as in Table 1, while $D_1D_2D_3$ and D_4 are the dummy vectors described in section 2.3. Thus, in four dimensions, D_1 =(‘ 80, 0, 0), and so on.

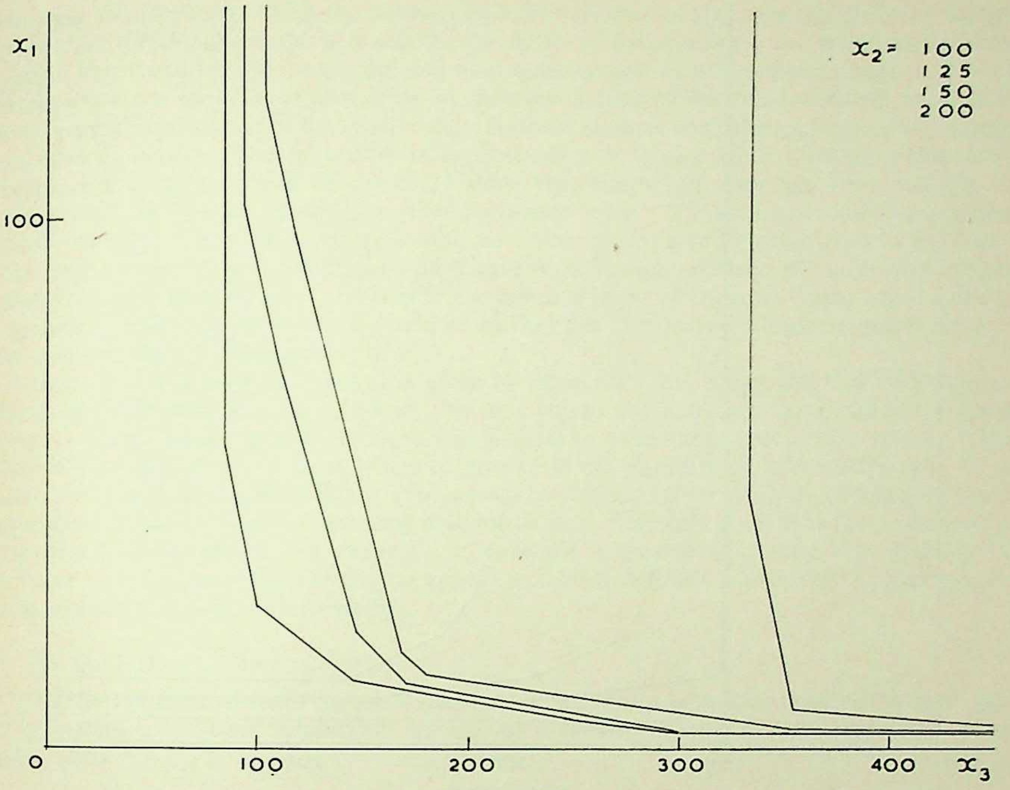

DIAGRAM 7*b*.

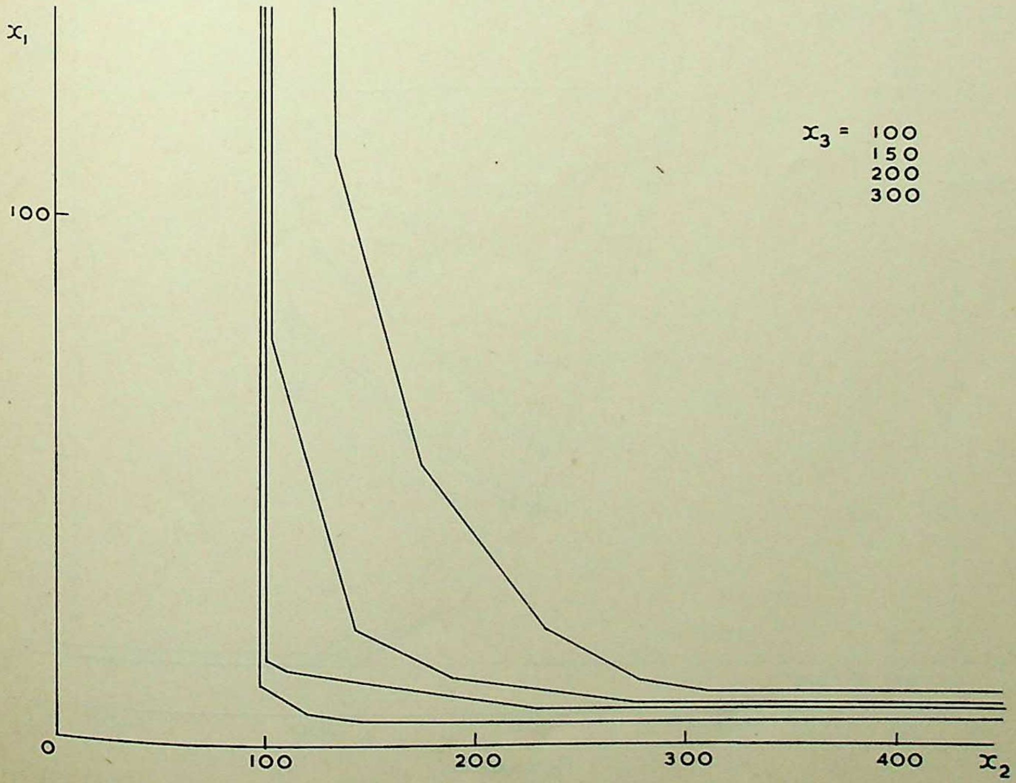

DIAGRAM 7*c*.

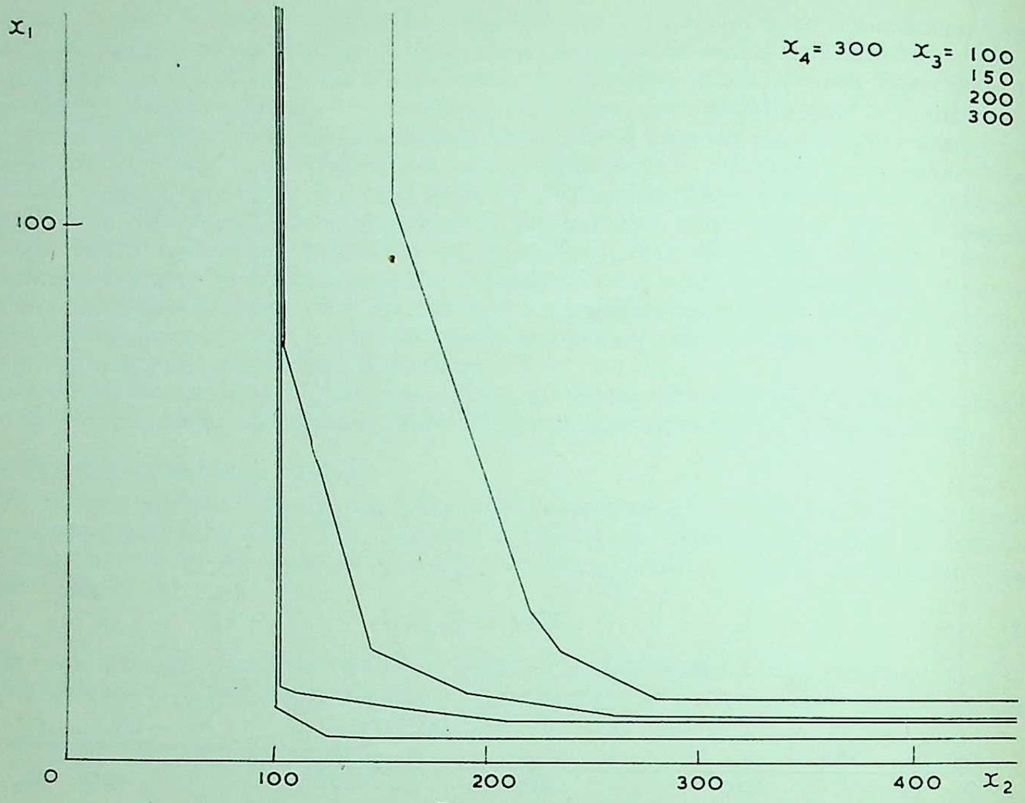

DIAGRAM 8*a*.

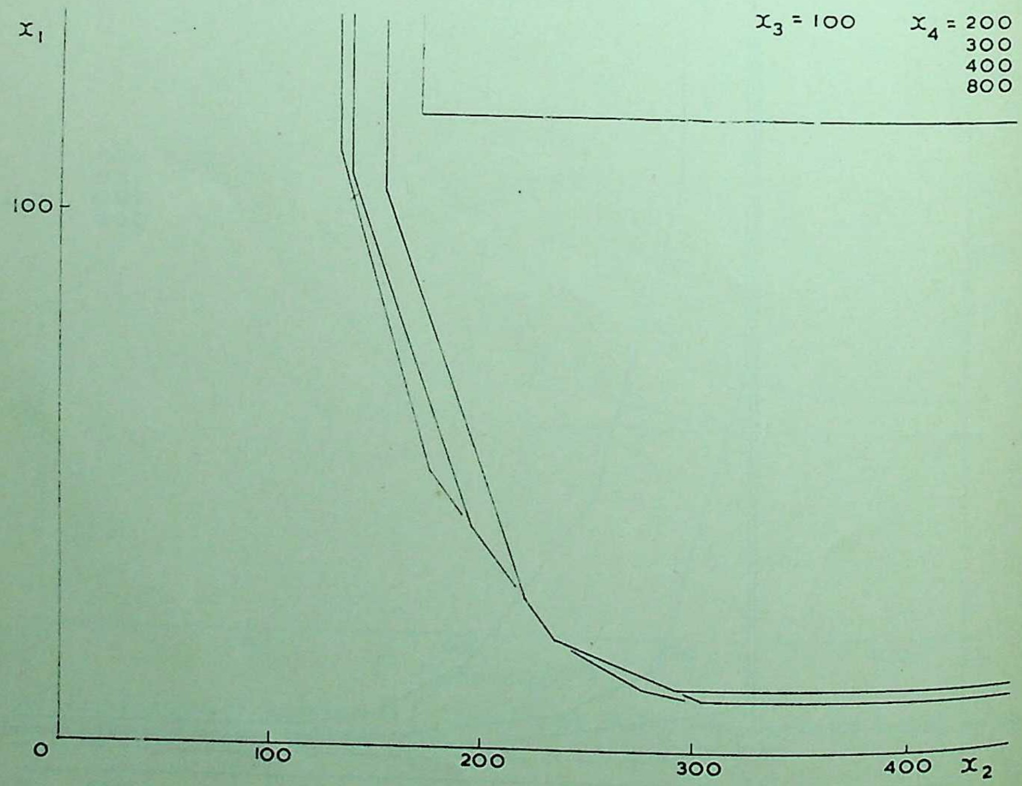

DIAGRAM 8B.

of showing the functions. This can be done by drawing contour diagrams like those of Diagrams 7 and 8. In three dimensions, if one of the variables is held constant, the relationship between the other two variables can be represented as a contour line in a two-dimensional diagram. If such contours are plotted for a number of different values of the third variable, the resultant contour map gives a picture of the relationships between all three variables, as is done in Diagram 7.

In four dimensions, the procedure must be pushed a stage further. Holding one variable constant, a contour map can be drawn to show the relationship between the remaining three variables when the fourth variable has that particular value. Thus Diagram 8*a* shows the relationship between x_1, x_2 and x_3 for $x_4 = 300$. In order to give a complete picture of the function a very large number of such diagrams would have to be drawn—reasons of space preclude giving here more than a small sample. Of course, the contour maps of Diagram 7 also help to complete the picture for the four-dimensional function, as they are identical with the contour maps for the latter corresponding to large values of x_4.

Perhaps the most striking impression given by these diagrams is that the Law of Diminishing Returns operates very sharply. Indeed, this has led to the contours being drawn at unequal intervals—using equally-spaced contours would give an even more spectacular result. This is at least in part due to the method, which assumes that the elasticity of substitution falls to zero outside the range of observed factor proportions. Various rather obvious conclusions may be drawn, but perhaps the most interesting diagram is 8*b*. Here one notes that for a process to be economical in both capital and materials, it must use a great deal of land—i.e. grazing. One also notes how sharply returns to capital appear to fall off, but this is probably a reflection of the factors discussed in the previous section.

6.5. *The Cobb-Douglas Approximation*

It will be appreciated from the previous section that it is a laborious business both to specify and to analyse the efficient production function as it stands. This does not constitute a criticism

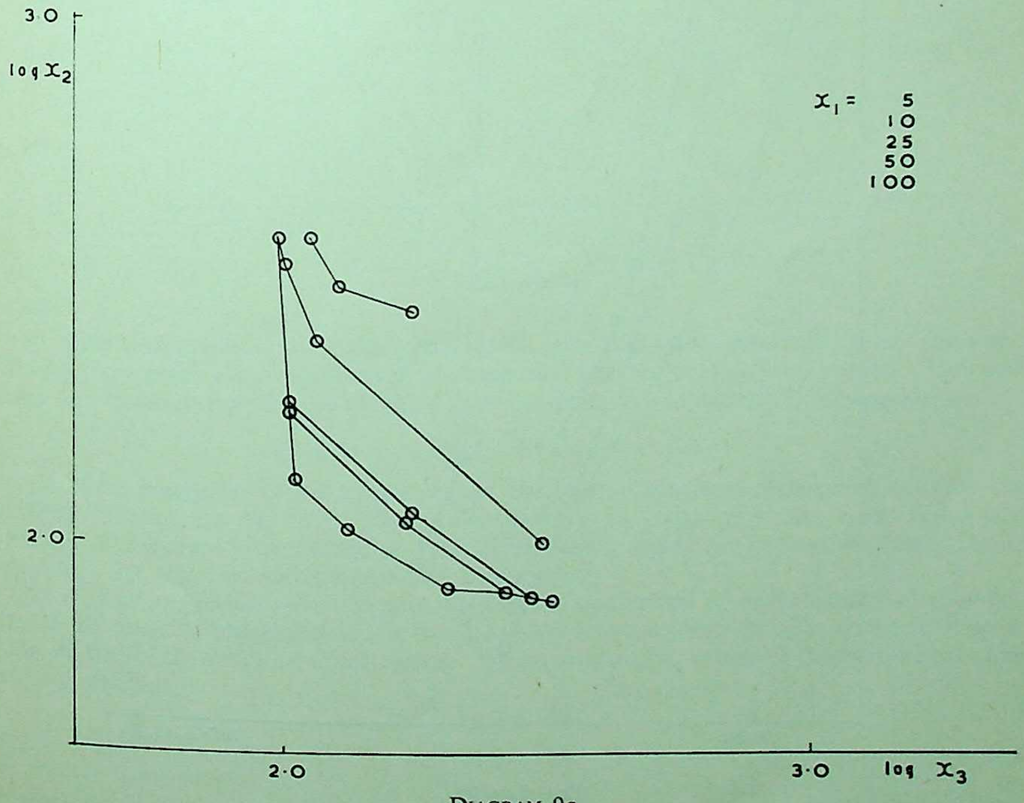

DIAGRAM 9*a*.

of the method—if the world is complex, its analysis must needs be laborious. However, it would be a great convenience to approximate the efficient production function by a simple mathematical function. It would then be specified by a simple formula, together with the values of a few parameters, and its properties could be analysed algebraically.

The best known and perhaps the most plausible of such approximations is the Cobb-Douglas function (Cobb and Douglas, 1928) which may be written in our notation

$$x_1^{\alpha_1} x_2^{\alpha_2} x_3^{\alpha_3} = k \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

where

$$\alpha_1 + \alpha_2 + \alpha_3 = 1.$$

This defines a surface, convex to the origin, very similar in principle to the efficient production function. If it were to turn out to be a good approximation to the observed function, it would have the great advantage that it could be specified in terms of four or five parameters. Its disadvantage is that it makes quite strong assumptions about the shape of the efficient function, and for this reason it would not have been desirable to fit such a function at the beginning of the analysis.

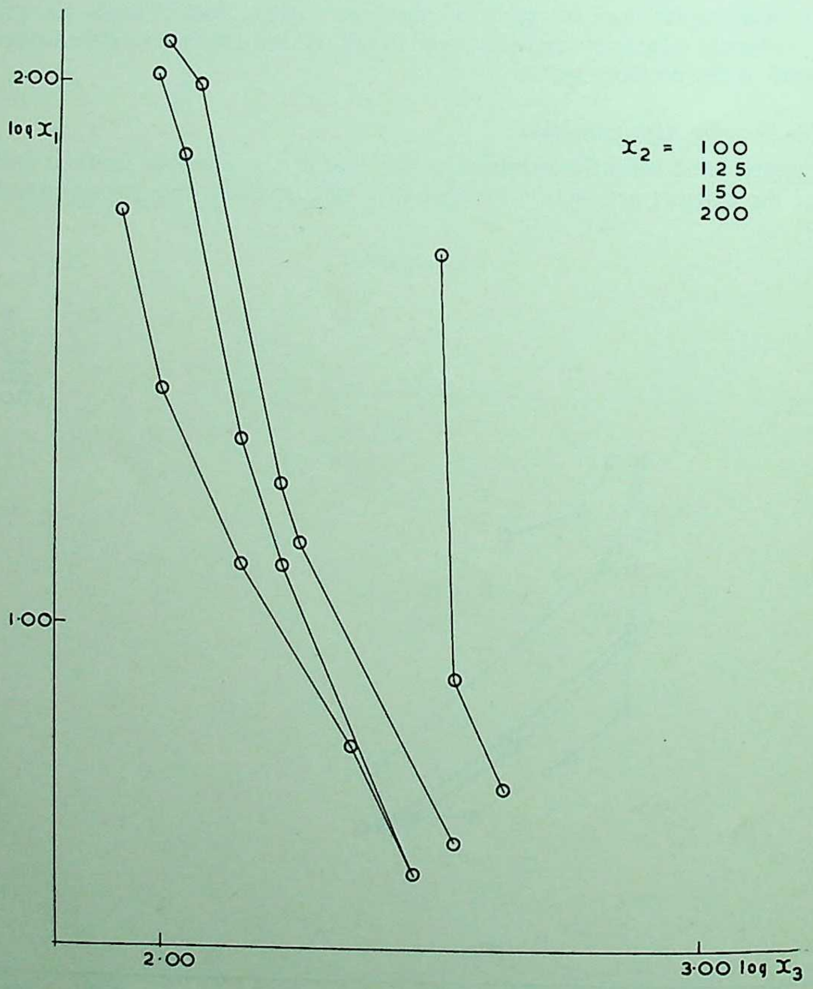

DIAGRAM 9*b*.

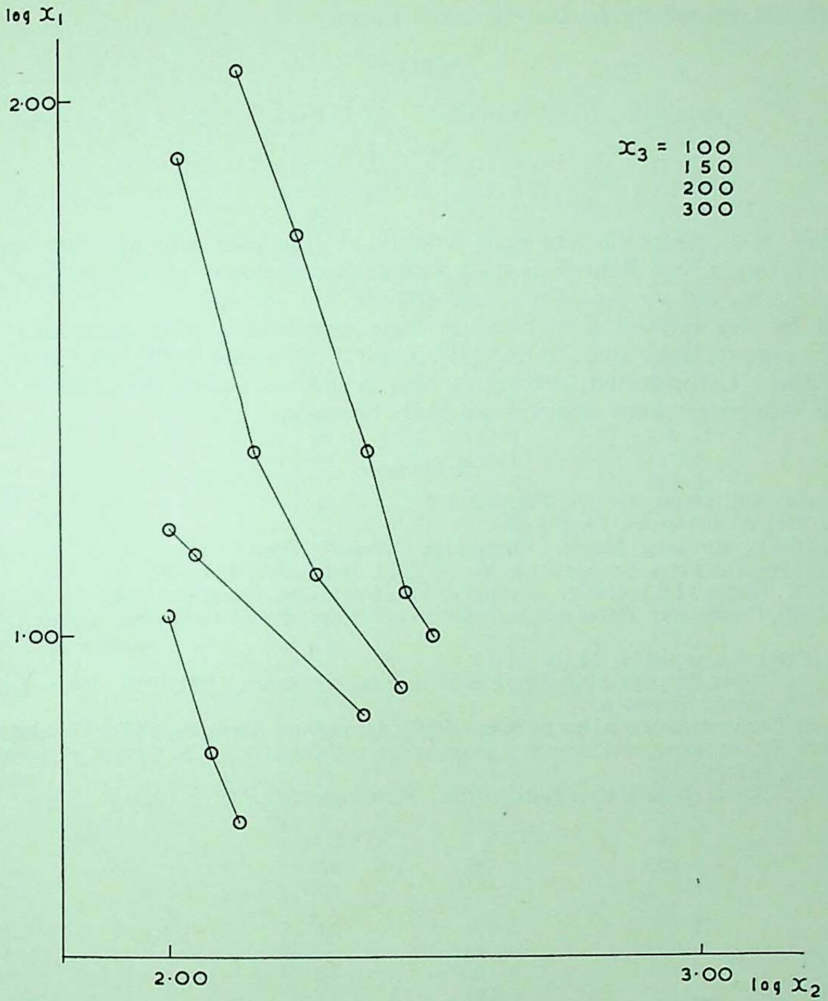

DIAGRAM 9*c*.

As three (independent) parameters have to be estimated from (effectively) seven observations (or four from nine) the paraphernalia of regression analysis is clearly unjustified. However, a rough test by inspection can be obtained from Diagram 9. Equation (9) may be written

$$\alpha_1 \log x_1 + \alpha_2 \log x_2 + \alpha_3 \log x_3 = \log k \quad . \quad . \quad . \quad (10)$$

so that, if the logarithms of the x's are plotted, the contour diagrams corresponding to the Cobb-Douglas function are sets of parallel straight lines. In Diagram 9 the corner points of the contours of Diagram 7 are plotted on logarithmic scales, and it can be seen that, apart from the extreme points, they lie roughly on parallel straight lines.

As the extreme points reflect in part the implicit assumption of zero elasticity of substitution outside the range of observations, one may conclude that the Cobb-Douglas approximation gives a fair fit within the range of observations. By inspection, the values of the parameters seem to be (very roughly)

$$\alpha_1 = \cdot 15$$

$$\alpha_2 = \cdot 45$$

$$\alpha_3 = \cdot 40$$

If x_4 is introduced, the parameters become (again roughly)

$$\alpha_1 = \cdot 12$$

$$\alpha_2 = \cdot 36$$

$$\alpha_3 = \cdot 32$$

$$\alpha_4 = \cdot 20$$

The low values of α_1 reflect the very great variation in x_1 as compared with the other variables. (The efficient points in four dimensions show a range of variation of about 3,000 per cent. for x_1, 350 per cent. for x_2, 600 per cent. for x_3 and 480 per cent. for x_4.)

It would be very interesting to compare these results with other agricultural production functions, for example those that Tintner (1944) and J. O. Jones (1956) derived from data for individual farms. Unfortunately, differences both in method and in definition of variables are so large that such comparisons cannot at all easily be made.

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TABLE 1

State	*Land* x_1	*Labour* x_2	*Materials* x_3	*Capital* x_4
1. Maine	11·79	153·9	222·7	341·8
2. New Hampshire	13·58	175·4	411·2	346·0
3. Vermont	23·46	207·1	328·0	454·9
4. Massachusetts	5·06	138·2	309·6	286·9
5. Rhode Island	4·57	151·5	331·0	274·5
6. Connecticut	5·29	125·7	317·3	258·3
7. New York	15·46	179·7	277·5	535·7
8. New Jersey	4·17	130·9	317·4	307·6
9. Pennsylvania	13·96	189·2	303·6	581·4
10. Ohio	18·97	199·8	209·0	525·0
11. Indiana	16·56	157·4	199·4	428·3
12. Illinois	15·66	130·1	197·4	434·8
13. Michigan	20·73	211·4	155·2	655·5
14. Wisconsin	19·96	217·6	189·7	625·3
15. Minnesota	22·65	185·4	187·7	528·7
16. Iowa	14·37	118·8	251·5	378·0
17. Missouri	27·87	220·7	226·7	398·8
18. North Dakota	74·18	176·4	87·6	761·8
19. South Dakota	81·58	172·2	207·3	574·0
20. Nebraska	44·21	134·0	246·2	407·3
21. Kansas	43·01	137·3	213·2	428·6
22. Delaware	5·62	97·2	452·0	252·0
23. Maryland	11·42	198·8	296·4	425·4
24. Virginia	18·19	299·9	189·5	342·3
25. West Virginia	37·13	367·6	243·9	342·6
26. North Carolina	10·66	333·3	76·8	324·6
27. South Carolina	19·56	441·4	88·3	386·3
28. Georgia	26·74	383·1	144·3	420·1
29. Florida	27·02	176·8	113·9	182·9
30. Kentucky	24·58	391·1	173·6	397·1
31. Tennessee	25·34	440·0	153·1	494·2
32. Alabama	29·80	462·8	119·1	413·2
33. Mississippi	26·87	442·3	91·5	406·1
34. Arkansas	25·09	336·8	132·7	449·6
35. Louisiana	22·52	344·4	97·2	473·9
36. Oklahoma	54·84	229·9	193·5	475·5
37. Texas	63·04	177·4	161·8	341·0
38. Montana	155·69	135·1	140·4	534·0
39. Idaho	38·14	161·3	200·7	584·6
40. Wyoming	233·93	140·8	282·3	428·3
41. Colorado	69·00	122·3	254·5	407·8
42. New Mexico	207·94	158·1	186·0	288·3
43. Arizona	137·89	105·3	109·4	158·9
44. Utah	64·87	167·6	309·3	421·5
45. Nevada	144·65	122·4	186·4	338·1
46. Washington	30·08	127·7	161·2	398·4
47. Oregon	45·53	143·2	180·6	441·2
48. California	14·53	104·8	191·1	244·4

TABLE 2

	Efficiency when Variables Considered are:—										
	x_1x_2	x_1x_3	x_1x_4	x_2x_3	x_2x_4	x_3x_4	$x_1x_2x_3$	$x_1x_2x_4$	$x_1x_3x_4$	$x_2x_3x_4$	$x_1x_2x_3x_4$
1. Maine . .	63	72	71	68	65	49	94	71	83	68	94
2. New Hampshire	55	52	69	58	58	46	69	69	70	58	71
3. Vermont . .	47	39	51	50	48	35	58	53	56	50	58
4. Massachusetts .	89	95	94	74	73	55	98	95	99	74	99
5. Rhode Island .	91	94	100	68	68	58	94	100	100	68	100
6. Connecticut .	92	92	100	81	80	62	98	100	100	81	100
7. New York .	54	55	46	58	54	37	75	54	57	58	75
8. New Jersey .	100	100	100	78	76	52	100	100	100	78	100
9. Pennsylvania .	51	57	43	55	51	34	74	51	57	55	74
10. Ohio . .	49	52	46	53	49	45	78	49	58	53	78
11. Indiana . .	62	58	56	67	62	50	87	62	68	67	87
12. Illinois . .	75	61	56	80	75	50	93	75	69	80	93
13. Michigan . .	46	51	37	64	46	49	88	46	51	64	88
14. Wisconsin .	45	51	39	55	45	45	79	45	51	55	79
15. Minnesota .	52	46	45	58	52	48	82	52	56	58	82
16. Iowa . .	82	60	64	86	82	43	88	82	73	86	89
17. Missouri . .	44	37	55	48	47	46	68	57	62	48	68
18. N. Dakota .	55	88	27	100	55	88	100	55	88	100	100
19. S. Dakota .	56	37	32	61	56	44	71	56	45	61	71
20. Nebraska . .	73	31	49	77	73	43	78	73	51	77	78
21. Kansas . .	71	36	48	76	71	47	81	71	52	76	81
22. Delaware . .	100	74	100	100	100	63	100	100	100	100	100
23. Maryland .	49	66	58	52	51	37	78	58	68	52	78
24. Virginia . .	32	54	67	50	46	55	71	67	78	55	78
25. West Virginia .	26	31	58	40	46	46	52	58	60	46	60
26. North Carolina	39	100	75	100	49	100	100	75	100	100	100
27. South Carolina .	25	87	60	87	41	87	87	60	87	87	87
28. Georgia . .	25	53	53	58	38	62	67	53	64	62	67
29. Florida . .	55	67	100	84	87	94	100	100	100	94	100
30. Kentucky . .	25	44	57	50	40	56	63	57	66	56	66
31. Tennessee .	22	50	47	54	32	56	62	47	58	57	62
32. Alabama . .	21	64	53	66	38	70	66	53	71	70	71
33. Mississippi .	22	84	55	84	39	84	84	55	84	84	84
34. Arkansas . .	29	58	51	64	35	64	74	51	65	68	74
35. Louisiana .	28	79	49	83	34	79	86	49	79	84	86
36. Oklahoma .	42	40	41	54	44	49	67	48	51	54	67
37. Texas . .	55	47	53	66	58	61	81	64	64	66	81
38. Montana . .	72	55	33	78	72	57	81	72	57	78	81
39. Idaho . .	60	38	38	65	60	45	79	60	46	65	79
40. Wyoming . .	69	27	39	73	69	38	73	69	39	73	73
41. Colorado . .	79	30	45	84	79	42	85	79	45	84	85
42. New Mexico .	61	41	57	66	65	58	66	65	58	66	66
43. Arizona . .	92	70	100	100	100	100	100	100	100	100	100
44. Utah . .	58	25	43	62	58	38	62	58	43	62	62
45. Nevada . .	79	41	51	85	79	56	85	79	57	85	85
46. Washington .	76	48	55	82	76	59	99	76	63	82	99
47. Oregon . .	68	43	46	73	68	53	87	68	54	73	87
48. California .	93	65	93	99	95	65	100	99	100	99	100

TABLE 3

Facet				a_1	a_2	a_3	a_4
5	6	8	22	77·056	1·062	·113	1·638
6	8	22	48	22·991	4·597	·918	·036
6	8	26	48	28·024	1·196	1·798	·507
6	22	29	48	10·491	·926	·303	2·834
6	26	29	48	17·352	·012	1·020	2·258
18	26	29	43	1·184	1·395	6·046	·179
22	29	43	48	2·084	2·231	·050	2·972
D_1	18	26	43	0	1·011	7·924	·167
D_2	5	6	8	51·771	0	1·333	1·173
D_2	6	8	26	49·480	0	1·414	1·121
D_2	6	22	29	13·155	0	·129	3·444
D_2	6	26	29	17·367	0	1·014	2·271
D_2	26	29	43	·599	0	6·003	1·641
D_3	5	6	22	62·514	·169	0	2·509
D_3	5	8	22	86·511	·986	0	1·659
D_3	22	29	43	2·045	2·138	0	3·097
D_4	8	22	48	23·006	4·671	·922	0
D_4	8	26	48	29·730	1·562	2·116	0
D_4	22	43	48	·141	9·034	·268	0
D_4	29	43	48	2·187	3·182	3·322	0
D_4	26	29	48	10·270	1·884	3·418	0
D_4	18	26	29	3·090	1·631	5·516	0
D_4	18	29	43	2·069	2·978	3·666	0
D_1	D_2	26	43	0	0	7·109	1·399
D_1	D_3	22	43	0	8·395	0	·730
D_1	D_4	18	26	0	·690	10·025	0
D_1	D_4	18	43	0	2·164	7·057	0
D_1	D_4	22	43	0	9·269	·219	0
D_2	D_3	5	6	59·630	0	0	2·650
D_2	D_3	5	8	126·787	0	0	1·532
D_2	D_3	6	22	53·134	0	0	2·783
D_2	D_3	22	29	11·952	0	0	3·701
D_2	D_3	29	43	1·146	0	0	5·298
D_2	D_4	8	26	78·545	0	2·119	0
D_3	D_4	8	22	102·018	4·390	0	0
D_1	D_2	D_3	43	0	0	0	6·293
D_1	D_2	D_4	26	0	0	13·021	0
D_1	D_3	D_4	22	0	10·288	0	0
D_2	D_3	D_4	8	239·808	0	0	0

TABLE 4

Facet			a_1	a_2	a_3
8	22	48	23·006	4·671	·922
8	26	48	29·730	1·562	2·116
22	43	48	·141	9·034	·268
29	43	48	2·187	3·182	3·322
26	29	48	10·270	1·884	3·418
18	26	29	3·090	1·631	5·516
18	29	43	2·069	2·978	3·666
D_1	18	26	0	·690	10·025
D_1	18	43	0	2·164	7·057
D_1	22	43	0	9·269	·219
D_2	8	26	78·545	0	2·119
D_3	8	22	102·018	4·390	0
D_1	D_2	26	0	0	13·021
D_2	D_3	8	239·808	0	0
D_3	D_1	22	0	10·288	0

DISCUSSION ON MR. FARRELL'S PAPER

MR. COLIN CLARK: It is a great pleasure to propose the Society's thanks to Mr. Farrell. Later this evening I am leaving for Japan, where I really do hope to learn something about agricultural productivity.

It would be wrong to imply that I am *au fait* with every aspect of Mr. Farrell's method. It does appear to me however that Mr. Farrell has designed a most ingenious procedure which is a very great advance on anything we have had before, and which still remains within the limits of practicability. Some of us first became acquainted with Mr. Farrell's work through his study of the demand for motor cars, in which he showed an ingenuity which put his work well ahead of that of previous workers in this field.

I think that the first point on which comment is required is on the choice of data. One generally thinks that intricate methods of this sort can be better applied to industrial than to agricultural data. Mr. Farrell, however, showed an inspired intuition, which has succeeded, in taking agricultural data as matter for his experiment.

The data are more precise than might appear at first sight. No agricultural data are perfect, but the United States has spent a remarkable amount of money on improving its agricultural statistics.

In other countries besides the United States, agriculture is of great political importance, and therefore may receive more than its share of the funds available for statistical work, and many industrial statistics for that reason still leave much to be desired.

I think we will agree, having heard this paper, that Mr. Farrell has already reached some interesting and successful results and has come nearer than any previous investigator to a true measure of agricultural efficiency, which figures in their turn will send the economists further down the road hunting for the social and other factors which lie behind them.

We all share Mr. Farrell's concern about his inability to introduce climatic factors into his analysis. I would suggest that in the present state of our knowledge we would probably do best by introducing climatic factors *a priori*. Some plant physiologists have reached the stage at which they can quantify their knowledge of plant requirements of moisture and temperature. I hope in later discussions with Mr. Farrell to be able to put before him some evidence about some approximate methods of quantifying the climatic element for purposes of international comparison. I certainly found his treatment of my international comparative data most valuable and I look forward to many interesting results in future. I have been puzzled for years about getting some three-cornered relationship between land, labour and agricultural product, and Mr. Farrell has opened up the hope of securing a valuable relationship.

The Cobb-Douglas approximation, which is still being ferociously debated by American statisticians, still gives a useful result. Many will be relieved to hear this, because it is certainly one of the simplest functions which we can hope to handle.

Mr. Farrell has warned us, on purely mathematical grounds, that we cannot use any functions which imply general decreasing or increasing returns to scale (as apart from diminishing returns to individual factors). It was that condition, I think, which prevented the fruitful application of the Cobb-Douglas approximation to United States manufacture, except for a very limited period. It will continue to limit the scope of this work.

As to the very crude methods of using weighted averages of costs, pioneered by G. T. Jones in the 'twenties, to which Mr. Farrell refers, I think every one of Mr. Farrell's criticisms is justified; but, nevertheless, Jones was searching for, and managed to find to some extent, a measure of the extent of increasing returns, in a field in which it is still impossible to use more refined functions. I think that we all regret that industrial statisticians have done so little over the last 30 years to improve on Jones's crude results, and in industrial as well as agricultural statistics we should be able to look forward to a lot of fruitful work in the future, started by Mr. Farrell's paper.

MR. C. B. WINSTEN: Two aspects of Mr. Farrell's paper I found especially useful. The first is his emphasis on the importance of measuring more than one input when facing a problem of efficiency. Diagrams like diagram 2 of his paper would be very helpful in many such situations. The second aspect is the introduction of what he calls the "efficient production function" as a function to be fitted from empirical data. Most production functions in the past have either been fitted by regression methods, or by extensions of these methods by a simultaneous equation approach (for example, Marschak's and Andrew's work, "Random simultaneous equations and the theory of production" in *Econometrica*, 1944). All such regression methods estimate what are, in a sense, average production functions, or perhaps the production function of the average firm. It seems an interesting further question to ask what the production function of an efficient firm might be, and Mr. Farrell's method of estimating this function is ingenious. It would also be interesting to know whether in practice this efficient production function turned out to be parallel to the average production function, and whether it might not be possible to fit a line to the averages, and then shift it parallel to itself to estimate the efficient production function.

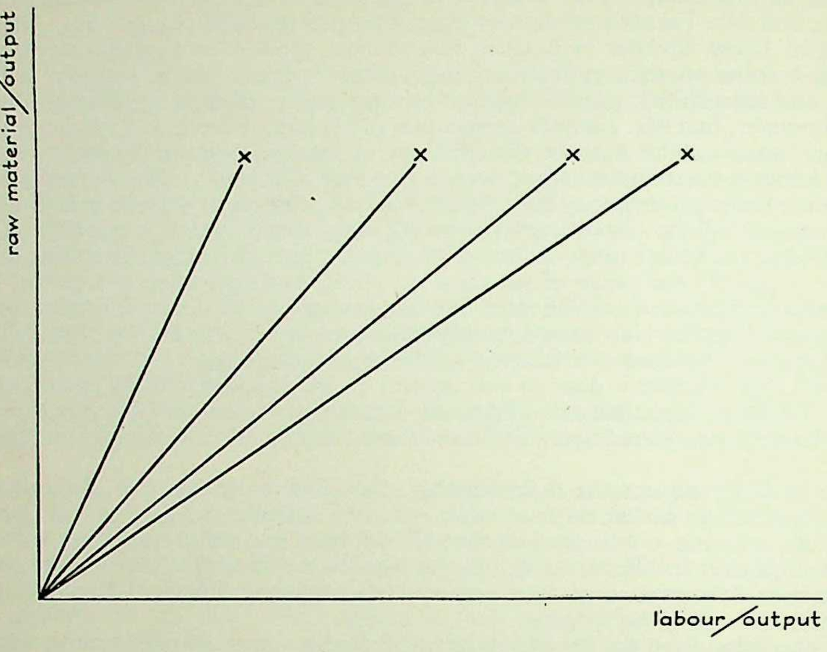
I do not, however, find Mr. Farrell's explanation of his index of technical efficiency convincing. He states that "when one talks about the efficiency of the firm one usually means its success in producing as large as possible an output from a given set of inputs". Mr. Farrell has suggested that he is dealing with an immensely wide field "from a workshop to a whole economy", and it is difficult to summarize all the sorts of questions of efficiency which may arise and difficult, therefore, to judge whether a particular index is "usually" useful. But all of them have this in common: it is necessary to specify the range of variation (of choice perhaps) open to the firm before one can set a standard of efficiency. The most familiar example is the distinction between short and long run problems. In the short run a factory management may have to work with the plant it has, and a short run standard of efficiency would be set accordingly. In the longer run it can change its plant, but whether it does so will depend on the conditions of its present plant. The standard of efficiency will be changed. Thus, any standard, or index, of efficiency depends on the possibilities which are considered open, and these should enter in an obvious way into the definition of indices.

Whether one can interpret the definition Mr. Farrell gives within this framework depends partly on how he defines his inputs, and what are to be included. All of us, but especially the journalists, trade unionists, politicians and the like, will have learnt that they must not take labour as the only input, but it is difficult to find in the paper what should be taken. Yet the fixing of such inputs help to define the constraints under which the factory manager is to operate, and the standards by which he is to be judged.

From the examples given the list of inputs to be used is wide. It includes raw materials and various forms of capital. It also includes quasi inputs such as position and seam thickness and weather. Even air and water are mentioned. But any plant manager would be astonished if his technical efficiency (his "need for gingering up") were judged by the amount of air he used. A usable definition of the inputs corresponding to actual standards to be set is an essential of any index, technical or otherwise.

Some of the inputs used are composite commodities; for example, capital might consist of different sorts of equipment. A method of determining how capital should be aggregated is given in the footnote on 3.1. The method suggested depends on the production characteristics of the equipment. But the way one aggregates equipment in this way affects the type of question one is asking. If, in diagram 2, two sorts of capital are aggregated to give one input, this means that while this aggregate input remains constant, the types of capital used may be varied. Thus the combining of two forms of capital in this way relaxes the restrictions under which the firm is supposed to produce its output. But such restrictions should depend on possibilities of change, not on the production characteristics of the equipment. Another difficulty is that what seem to be used as inputs in the paper can occur in actual problems in two roles. Sometimes they are in the control of the manager, and so might justifiably be called inputs, and sometimes they are, in the terms of the problem, fixed environmental factors which the manager cannot change. When they occur in the latter role they may affect the production function, and may be useful in helping to determine the production function empirically. But we should expect an index of efficiency to distinguish these two roles.

The following is an example, extreme but not far fetched, which suggests that Mr. Farrell's index does not answer one kind of question concerning efficiency in which we may be interested. Suppose we are studying a set of plants, each producting one output and, for the sake of simplification, using just two inputs to do this. We might call these inputs labour and raw material. Now suppose it happens that, whatever the plant, it requires a fixed amount of the raw material to give a given level of output. For example, the plant might be cutting up strip, and different plants have little variation in scrap or waste material. But suppose that these plants use very different amounts of labour. Then on a diagram like diagram 2, points might look like those shown on page 284; and the "technical efficiencies" of the firms in Mr. Farrell's sense will all be 100 per cent. even though some are far more wasteful in the use of their workers than others. Notice that the actual technique which they use may be the same. But in this case one would be interested in the waste of labour (labour productivity), a different comparison to the one implied by Mr. Farrell's index. The point will still arise if there are many inputs one or some of them being closely related to output. Then these will dominate the index. So Mr. Farrell's index does not answer the questions we are likely to ask in this case. But that is not surprising. To attempt to answer such a wide range of questions by an index is a very ambitious thing.



Mr. Farrell has contributed a lively paper which I think will stimulate further work on his subject. I have much pleasure in seconding the vote of thanks.

The vote of thanks was put to the meeting and carried unanimously.

Dr. A. J. HOFFMAN: I shall make a few comments about the proposed methods of computation described in 5.2. The principal problem to be solved is: given points $P_1, \ldots, P_n$ (including the infinite points) in m-space, to find the equations of the facets of the convex hull of the P's. This is demanding a great deal of information, and m and n do not have to be very large for the problem to become hopeless.

The second method discussed in 5.2 which, under the name of the "double description method", was proposed by Motzkin, Raiffa, Thompson and Thrall in 1950, is probably the best, but computational experience indicates that the number of potential facets that have to be considered, although they are eventually discarded, may become terribly large.

If one's ambition is more modest, and it is only desired to compute the efficiencies of some of the points $\{P_n\}$, then the following method seems best. It is essentially the first idea proposed in 5.2, combined with an intelligent way of proceeding to neighbouring facets. Indeed, there is no difficulty in principle of continuing until the efficiencies of all points and all efficient facets have been found.

Let $P_1, P_2, \ldots$ stand for the technologies of each firm and for the infinite points as well; then the problem is, for each P_k, to find the smallest value of λ such that λP_k is in the convex hull of all the P's. This number is the reciprocal of the maximum value of $x_1 + \ldots + x_n$, where $x_j \geqq 0$ and $P_k = x_1P_1 + \ldots + x_nP_n$. The latter is a problem of linear programming, and the dual simplex method of C. E. Lemke (*Naval Research Logistics Quarterly*, 1, 48–54) is particularly adapted to solve this problem. Without going into details of the method let me nevertheless illustrate some of its virtues.

It first solves the problem for P_1, and then permits an easy determination of all points P which have the same efficient facet as P_1 and computes their efficiency as well. Turning to the next point P whose status is still undetermined it proceeds "efficiently" through efficient facets to determine that facet which is efficient for this P, and so on. At each stage, all the points for whom the associated efficient facet is the one currently under examination are readily found and (if they are not on the efficiency boundary) can be easily discarded in subsequent computations.

Mr. STURROCK: I am a practical economist, not a statistician. One of the duties of my department is to provide measures of efficiency that can be used by the advisory services in agriculture. These measures may not be very elegant from the theoretical point of view but they must work in practice. It is, therefore, of particular interest to listen to Mr. Farrell's paper which deals with the statistical basis of such measures.

There are, first of all, three points that Mr. Farrell might like to take into account in improving his technique:

Firstly, I am a little sceptical of a system which depends on joining up the *best* results. If a large sample is used, individual results may be freakish. Not only are there errors but there are chance variations that have nothing to do with efficiency. Every business has good years and bad years depending on the state of the market, chance variation in prices and other factors over which the manager has no possible control. To call only these freakishly good results "100 per cent. efficient" would result in hanging the carrot too high and the donkey would be discouraged. It would be better to have either "average results" or 'premium results" (say the average of the upper 10 or 20 per cent.).

Secondly, the factors quoted by Mr. Farrell are not always homogeneous, and if care is not taken this may give rise to spurious rates of substitution. To give an example: Suppose we were quoting two farms in South Dakota and Iowa respectively. Suppose the first of these had 600 acres and a yield of 15 bushels an acre and the second had 150 acres and a yield of 60 bushels an acre, both, therefore, having the same total output. Suppose also that the first farmer uses 150 dollars worth of fertilizer and the second 600 dollars worth. If one takes acres of land as an input, one might conclude that 450 dollars worth of fertilizer was a substitute for 450 acres of land. In practice, the South Dakota farmer would be wasting his time if he put on more fertilizer because there is not enough moisture to give a bigger yield. It would be more accurate in a case like this to take the real estate value of land instead of acres. In this case, the output of these two farms is about the same and the real "input" of land in estate value might be about the same—not four times as great on one as the other as a consideration of acreage alone might lead one to suppose.

Thirdly, in moving from one State to another along Mr. Farrell's curves, the nature of the function is changing completely. For example, at the top of the curve, one may have a negro farmer in the deep South who is thinking of substituting a tractor for a mule in the production of cotton. In the middle of the curve, one may have a mid-West farmer who is wondering if he should buy a cornpicker instead of hiring one from a neighbour. This is part of hog production. At the bottom, we may be considering a Wisconsin dairy farmer who is wondering if he should buy a mechanical cleaner for his cowhouse. This is milk production. These three points, however, prepresent three different industries with no product in common. Does a curve joining these three points have any particular meaning? The same fault is apt to arise in the use of Cobb-Douglas functions. The input-output ratios are apt to be very mixed, and for that reason I am not sure that there is not more hope in the use of linear programming.

There is another more theoretical point. As every economist knows, the average cost curve, as output increases in a business, is normally U-shaped. Average cost per unit first falls owing to economies of scale, then rises as one reaches the full capacity of the existing equipment. The optimum level of production, however, is not the point of lowest average cost. In practice, it pays to increase production until marginal cost has risen to marginal revenue. In Mr. Farrell's first diagram, a firm that was expanding output might migrate from point P to point Q, then back again to point P as output reached the full economic level. Point Q might, therefore, represent a firm operating below optimum capacity. A business that appears highly efficient in diagram 1 may, therefore, be operating below the optimum output.

I must confess that this last criticism also applies to many of the efficiency factors that we use ourselves.

Finally, I should like to make a plea to Mr. Farrell to use his methods on data from British farms. There is ample information available. I mention these points not as a fundamental criticism of Mr. Farrell's paper but merely in the hope that he may use them in improving the techniques described. We are always very interested in research of this kind and would be among the first to adopt any technique that would give practical results.

Dr. EASTERFIELD: I have been quite fascinated by Mr. Farrell's line of approach. I was privileged to see some of this work while under development, and it struck me as an enormously useful addition to the equipment of tools for comparing the efficiency of firms and for seeking out the causes of efficiency variations in order to find ways of raising efficiency.

There is, however, one point of his approach which I should like to criticize to some extent. It seems to me to suggest that one can get a measure of efficiency which will answer all the questions and show which firms ought to get the medals and which need gingering up. But I do not believe it. It seems to me that some of the measures which Mr. Farrell has dismissed—for example measures of the productivity of labour—may be very useful for particular purposes. It may be well worth while for firms to know whether they are using much the same labour per unit of output as others. That they are using much more labour per unit of output is not necessarily a criticism, provided they know they are doing it and are doing it deliberately.

Again, the engineering type of efficiency, the comparison with what would theoretically be possible if everything went well, was dismissed as being psychologically depressing. But it may be useful to be given an idea of what is possible in an imperfect world. There is obviously a difference between knowing that you are running an engine at something like 50 per cent. of its capacity and knowing that in the cutting of a bar of steel you are using something like 10 to quite a high power times the work theoretically needed to make two surfaces. In the latter case the efficiency is quite irrelevant, but in the former it may be useful to know you are getting only 50 per cent. of the efficiency you might get, even if you know that no one has yet managed to get to 60 per cent.

I should like to take up a point implicit in Mr. Winsten's comments, which is that an important way of looking at efficiency is to assess it in terms of what one is trying to do. One may, for example, reasonably be required to consider what sort of fluctuations one may expect to be up against. If one plans a firm to be able to cope with changing factors of demand, and so on, it is extremely likely that one will pay insurance policies in some form, in labour, stocks or reserves, and to that extent incur costs and in a sense lower efficiency. But if in so doing the firm is enabled to survive then it seems to me that in another sense one is more efficient. I would, therefore, come back to the point that one must not use this method of efficiency, or any other method, as the sole means of knowing which is the best firm. In fact, the idea of looking for the best firm ought to be cut out. It has bedevilled all the ideas of productivity.

I should like finally to make a suggestion. Is it possible to get the collaboration, if not of the Census of Production then maybe of a trade association, to try to apply Mr. Farrell's methods in an industry where one could work with something a little more homogeneous than the land which the farmer uses? And, if so, might we not consider a factor which one or two writers recently have placed great stress on, namely, indirect costs of production? There have been suggestions that a great deal of indirect costs in the way of research and so on relate to ways of increasing efficiency or productivity (or something that ought to be increased, anyway). There have also been suggestions that this is quite wrong and that by having people on such work you are carrying a lot of passengers who are no doubt very busy but on quite irrelevant activities. An analysis on Mr. Farrell's lines that covered raw materials, labour, indirect costs of the type just referred to, and possibly other input factors, might give very interesting results.

Professor M. G. **Kendall** (read by Mr. Quenouille): The method employed by Mr. Farrell is admittedly a rather elaborate one, and, as he points out, would strain the resources even of an electronic computer if many variables were involved. I have therefore considered whether a simpler approach might not be adequate. The following is based on one of the methods for measuring agricultural productivity which I discussed in a paper read to the Society in 1939.

If we take the figures in the first column "Land" of Table 1, and rank the 48 States according to them, we obtain a ranking of the numbers 1 to 48. The same procedure can be followed for the other three columns, and the rank numbers summed for the four variables, giving 48 rank sums. These numbers may then be used to arrange the 48 States in a descending order of "productivity" and the results compared with the ranking obtained from the final column of Table 2.

In the rankings obtained from Mr. Farrell's figures there are a number of ties but this does not seriously affect the comparison. I have carried it out, and find that with a few exceptions the ordering given by the ranking method is very similar to the one given by Mr. Farrell's. The Spearman correlation co-efficient between my ranking and Mr. Farrell's is about 0·76, and, if we exclude six anomalous values, is 0·92.

The anomalous values in question concern New Mexico, New Hampshire, Kentucky, Michigan, North Dakota, and Idaho, and I have looked at these individually. It seems to me that the ranking method gives results which on the face of it are at least as acceptable as those derived by Mr. Farrell. For example, in the case of North Dakota, my ranking makes it the 37th, whereas Mr. Farrell gives it an efficiency of 100 per cent.

On the individual variables, North Dakota is 42nd for land, 25th for labour, second for materials, and 48th for capital. The discrepancy seems to arise from an extraordinarily low figure on materials, and on the whole it seems to me that my ranking is a fairer reflection of the position than Mr. Farrell's, since he gives it an efficiency of 100 per cent., notwithstanding that it is the least efficient in the use of capital and nearly the least efficient in the use of land.

The ranking method, of course, purports to arrange the States of the Union in order and not to quantify the measurement of efficiency. If it is no more, however, it is an easy check on the more elaborate method, and from the examples I have given I think it may well prove to be more than that.

All this is without prejudice to the question whether productivity in the agricultural sense can be measured in this way. In my own work on the English crop counties—which, incidentally, also were 48 in number—I worked on the crop yields without reference to the factors of production. The same method has more recently been used by Professor Dudley Stamp in his book on *Land Today and Tomorrow* in arranging the countries of the world according to agricultural productivity.

Mr. Quenouille: The point that is in part the basis of Professor Kendall's remarks involves the choice of scale for the input factors. Obviously, the measurement of technical efficiency for the more extreme points may be greatly affected by choice of scale.

For instance, suppose in the analysis of diagram 1 the logarithms of x and y are used. The lines through the origin are then parallel diagonal lines (with the slope of one) and the logarithm of the efficiency is the diagonal distance of the point P from the curve SS'. The whole approach of diagram 1 transforms simply.

The estimation of diagram 2 gives rise to SS' as a series of exponential curves joining the extreme points, and, at the ends, to exponential curves approaching the axes. This approach, probably, will not greatly affect the estimated efficiencies for the central point, but points near the axes may have completely different estimates of efficiency.

The following contributions were received in writing after the meeting:

Mr. J. A. C. Brown: I should like to comment briefly on Mr. Farrell's remarks on estimation in section 3.6. Mr. Farrell's emphasis is placed mainly on assuring us of the efficiency of his procedure for estimating the efficient production function and he rather minimizes the importance of errors of observation. But in many applications interest will centre on the estimates of the relative efficiency of individual firms, and most economic statisticians would wish, I think, to attach some measure of error to these and to correct any possible bias. It is important at the outset to distinguish between the approach based on a hypothetical distribution of efficiencies from that based on errors of observation, particularly in relation to the size of sample considered. For on the first approach the *pessimistic* bias in the estimate of the efficient function will diminish as the sample size increases, whereas on the second approach the *optimistic* bias will increase with the sample size. I do not myself think that the assumption of a hypothetical parent distribution of efficiencies is appropriate here, since it involves a departure from Mr. Farrell's principle of comparison with an observed rather than a theoretical standard (section 2.2). This particularly applies if the sample consists of all the firms in an industry, or all the farms in a region. It is therefore probably better to consider the true efficiencies as given variables, but their measurement is subject to error and random disturbance because of accidental variations in the inputs of factors. It is true, as Mr. Farrell says, that these random disturbances are likely to be most important in agriculture, but it will be very difficult even in industrial applications for the statistician to impute inputs correctly to a particular product during a particular period.

I hope, therefore, that Mr. Farrell can be encouraged to develop this aspect of his method further, for it is not only of theoretical interest.

Lady Hall: In the course of making a comparative analysis of the structure of United States and British retail trade, based on the 48 American States, and Washington D.C. and also on the regional data for Britain, I and my colleagues* have sought for a measure of technical efficiency for use in appraising actual performance. Mr. Farrell's most provocative paper is an invitation to raise some of the conceptual difficulties involved which his paper sets aside.

Mr. Farrell's diagram 2 is based on the assumption that a unique measure of each output can be found. In the analysis of retail trade, the conceptual difficulties involved in measuring output soon become apparent. A choice can be made between including the goods sold as part of the output or regarding the output simply as a series of services to the consumer which will include "transformations" like breaking bulk, wrapping etc.; these transformations are analogous to the transformations of goods from place to place, the service output of the transport industry. The choice of a particular output measure commits us to a choice of inputs appropriate to the supposed production function. If one includes the merchandise as an output then one must include the merchandise as an input but if one restricts oneself to the concept of output as services, this would be inappropriate. As far as I can see, there is, in the case of retail trade, no conclusive reason for preferring one of these output measures to the other, yet the use of different conceptions would seem to lead to different plottings on Mr. Farrell's diagram 2. He assumes, then, that a unique set of outputs can be derived but when there is a choice, as in the case just quoted, different measures of output yield different diagrams and therefore different results, according to the choice. This problem is present, surely in Mr. Farrell's own example of American agriculture: there are different ways of regarding agricultural production: for example, as a transformation involving feeding stuffs into milk and meat or as the production of milk and meat with feeding stuffs as an input. As an example, a farm may buy cattle and eventually sell them for meat. This could be treated as a transformation, or the cattle sold could be treated as on output in which case cattle bought would be an input. The lack of a uniquely defensible measure of output means that there will be not one but more than one point at which output is maximized for a given input.

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* For a discussion of conceptual difficulties involved see Margaret Hall and John Knapp, *Productivity Measurement Review*, No. 8, February, 1957, pp. 24 and 25 (English edition).

Mr. Farrell's forcible general case for considering all inputs in measuring efficiency is associated with what are, I think, too general structures on the use of partial input measures, in particular labour productivity. The use of this measure has sometimes been dictated by lack of data in contexts where it is unsuitable. This should not blind us to the fact that in other contexts, it may well be the most appropriate and interesting measure. In retail trade, for example, the capital involved is mostly in the form of buildings and fixtures, forms of capital which are inflexible in the short or medium run. We should therefore only wish to bring in capital in thinking how to improve efficiency in retail trade in the long run. Moreover, wages cost is such a large proportion of the total expenses of retailing that, at the moment, the principal increases in efficiency often result not from substitution of capital for labour but from the redeployment of labour.

Finally, heterogeneity of environmental circumstances is inherent in retail trade. Neo-classical economics referred to this feature as the imperfectly competitive character of the retail market. In his treatment of economies of scale, relevant for example to questions of the optimum size of shop, Mr. Farrell assumes that the simple choice is between maximizing output for a given input and minimizing input for a given output. The presentation of these alternatives seems to be on purely formal mathematical grounds. In practice, the most efficient size of productive unit will be influenced by a number of exogenous environmental factors, for example, the size of shop which is suitable in Piccadilly would be too big in a small village. It is not clear how these environmental factors can be reduced to "quasi-inputs" as Mr. Farrell suggests. If, for example, higher *per capita* incomes mean that the average size of transaction is larger in one area than in another, this might mean that the amount of retail (service) output involved in selling one pound's worth of merchandise was less and sales per person engaged and sales per unit of total cost were higher. Before we can regard this factor as any sort of input, we have to be assured that it is a variable factor and not a constraint and this assurance is lacking. But even if we go along with Mr. Farrell in supposing it, it is not clear whether this factor is a quasi-input or a quasi-output. It reduces the cost of selling a given quantity of merchandise but it also reduces the work involved in selling a given quantity of merchandise. In any case, it is not clear how Mr. Farrell would suggest incorporating his ingenious quasi-inputs in his generalized efficiency measure. Consider, for example, a given increase in *per capita* income. The extra trade it brings about can easily be accommodated by shop A, costs are reduced, efficiency increased. Shop B, however, is on a smaller scale, cannot easily accommodate the increased custom, and incurs an increase in costs through overcrowding. There has been an identical increase in a quasi-input yet its effect on different production units has been different*; how is this quasi-input to be fitted in to the analysis?

The above examples give some idea of practical difficulties of a particular exercise.

Mr. Farrell (in reply): I should like to thank those concerned for the extremely helpful discussion. Mr. Clark has suggested that I cannot deal with increasing and decreasing returns to scale. This is not so—decreasing returns are quite easy to handle, while increasing returns, although more difficult, are by no means impossible.

The only point on which I disagree with Dr. Easterfield is his suggestion that he is disagreeing with me—I wholeheartedly endorse his positive comments. I fully agree with Mr. Sturrock about the importance of the heterogeneity of land inputs and, to a lesser extent, of agricultural output. As I said in my paper, I regard some solution of the former problem as essential to a definitive study of agricultural efficiency, and the use of real estate values in place of acreage, while, it has obvious drawbacks, may well be the best solution. I am grateful to Dr. Hoffman for his erudite contribution, but I hope the prospect is not quite so black as he paints it. In particular, I cannot see why the first method of section 5.2 must be restricted to "some" of the observed points.

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* This idea was suggested to me by Mr. Michael Posner of the Oxford Institute of Statistics.

Mr. Winsten has raised a great many points. I shall attempt to deal with only two to-night. First, it is quite true (as indeed I emphasized in section 3.1) that the measure of efficiency depends on the inputs considered, and that the specification of the relevant set of inputs is therefore a necessary first step in any application of the method. But I cannot see that this constitutes any sort of objection to the validity of the measure itself. Secondly, while I agree that the measurement of capital is a difficult matter, I feel that Mr. Winsten's problem is a bogus one. So long as two types of capital equipment have the same production characteristics (in the sense of my footnote to section 3.1) it is irrelevant in what proportions they are used or whether these proportions can be varied.

Mr. FARRELL subsequently replied in writing as follows:

Mr. Winsten's example, where material inputs are (almost) the same for all plants, but labour inputs vary, is certainly spectacular and seems at first glance to raise an important objection to the method. However, on closer inspection it appears to be no more than a suggestion that, if certain additional technical information is available, a different way of splitting over-all efficiency into price and technical efficiency is possible and may have certain advantages. In diagram 1, the comparison of the observed point P with the optimal point Q yields the measure of over-all efficiency, OR/OP. This comparison I split into two parts, first a comparison between P and the point Q on the efficient isoquant that uses the factor in the same proportions, yielding the measure of technical efficiency, OQ/OP; and secondly, the comparison between Q and Q', yielding the measure of price efficiency, OR/OQ. Now suppose a line through P and parallel to Oy is drawn to cut Ox in U, AA' in V and SS' in W. Mr. Winsten suggests that, in his example, it would be better first to compare P with W, yielding technical efficiency UW/UP, and the W with Q', yielding price efficiency UV/UW. The final comparison is still between P and Q', and the over-all efficiency UV/UP is naturally equal to OR/OP. Thus, he splits the same measure of over-all efficiency so as to give what can easily be shown to be a smaller technical efficiency and a proportionately larger price efficiency.

Now my measure of technical efficiency can be interpreted as measuring how much more output could be obtained from the same inputs; that is to say, it was deliberately designed to avoid all the questions Mr. Winsten raises as to what variations in inputs are "possible" or "desirable". It is the measure of efficiency based on the most conservative assumption that no inputs may be varied, and is consequently the most charitable measure of technical efficiency. If additional technical information such as Mr. Winsten postulates, is available, one can of course construct a more stringent measure of technical efficiency, by asserting that certain variation of inputs may properly be demanded of a manager. In the extreme case, where all inputs are regarded as variable, technical efficiency will fall to over-all efficiency and price efficiency will rise to unity. It will be seen that such an extension of the concept of technical efficiency is rather a tricky business, and where possible one would prefer to stick to the original and quite unequivocal concept.

However, as Mr. Winsten suggests, cases may arise where the ratio of one input to output is so nearly constant that my measure of technical efficiency is unduly charitable, so that it is necessary to bring in extra information and define a more stringent measure. For the most part, this is likely to happen in cases like that cited by Mr. Winsten, where the process is a very simple one and all the plants use the same technique. Fortunately, the necessary technical information is likely to be easily obtained here, while in more complex cases, where the information is harder to come by, it is unlikely to be needed. A rather interesting case in point is Lady Hall's query as to whether the goods purchased and sold by a distributor should be counted as an input. As the correlation between purchases and sales is likely to be high (except in the case of perishables) this seems to be a good instance of the desirability of using this information to provide a more stringent measure; which can be done by simply omitting purchases from the list of inputs.

I found Lady Hall's note most refreshing, and the application of my method to distribution certainly raises some interesting problems. I think, however, her remarks on capital betray a misunderstanding of the problem. It is essential to bring capital into the computation of technical efficiency (which we have seen to be relevant in the very shortest run) because otherwise we shall make the mistake of regarding the most capital-intensive firms as the most efficient. (I was surprised to learn that in retail trade the capital is mostly in the form of fixtures, as I had always supposed stocks to be very important). The correct specification of quasi-inputs for an analysis of efficiency in distribution requires, of course, a good deal of thought, but I would suggest tentatively that differences of location could be treated by regarding the rental value of the premises as an ordinary input. I think it would be a mistake to treat the incomes of customers as a quasi-input. If they are regarded as simply producing an increase in demand, the paradox suggested by Mr. Posner is easily resolved—it is merely an illustration of the law of diminishing returns.

Before the increase in demand, B has its inputs in the optimal proportions but A is using too much space; after the increase, both increase their inputs of all factors save space, which is assumed fixed. This improves A's proportions but worsens B's.

I agree with Mr. Brown and Mr. Sturrock that the problem of random errors (discussed in section 3.6) is both difficult and of some importance. Indeed, I still hope that some theoretical statistician will take up the challenge offered by this knotty little problem. It is interesting that Professor Kendall's ranking method gives a similar ranking of states to my efficiency measure, but I fear he has missed the point of my paper. For I have attempted to produce a measure with a clear economic interpretation, not a method (as Dr. Easterfield would say) of handing out medals. I think the same point applies to Mr. Quenouille's ingenious proposal for a logarithmic transformation of the variables. I am most grateful for the interest these theoretical statisticians have shown in an economist's problems, but I must emphasize that the possibility of economic interpretation of any measure is vitally important. The field of economic index number theory is a veritable graveyard, so full is it of mathematically ingenious indices that are useless because they have no economic significance.

As a result of the ballot taken during the meeting the candidates named below were elected Fellows of the Society:

David Elliott Barton.
William James Chambers.
David Henry Davies.
Robert Joseph Deam.
Alexander David Threipland Deans.
Robert Dow.
George Wallace Henning.
Edward Francis John.
Kironde Kabunda.
Brendan Moore.
George Oyakhire.

A DISCUSSION ON THE USE OF ELECTRONIC DATA PROCESSING EQUIPMENT

Opened by J. H. H. MERRIMAN, D. T. CAMINER, H. L. BARMAN,
D. EVANS and H. DURANT

[Held before the ROYAL STATISTICAL SOCIETY, April 17th, 1957, the PRESIDENT,
Professor E. S. PEARSON, C.B.E., in the Chair]

Mr. J. H. H. MERRIMAN (H. M. Treasury):

AUTOMATIC DATA PROCESSING SYSTEMS

Introduction

I propose to divide these introductory remarks into two parts; in the first I shall consider some of the more important characteristics and limitations of automatic data processing systems and in the second survey very briefly some of the possible applications of automatic data processing systems to statistical tasks in Government Service. Before beginning, however, it is useful to have an agreed terminology. Reference to current literature will show that the terms "computers", "electronic data processing", "automatic data processing", "integrated data processing" are all used with little discrimination. For a variety of reasons, these terms are not satisfactory nor can they be regarded as interchangeable. I shall use the general term "automatic data processing (A.D.P.) which can be used in a generic way. It includes elements which are essentially mechanical and which the term "electronic data processing" tends to undervalue. (In fact, it is thought in some quarters that over-emphasis on the term "electronic data processing" has resulted in some very real neglect of those essential components of data processing equipments which have their origins in non-electronic skills, e.g. high-speed printing devices, automatic paper handling, automatic character recognition, and the like.)

In introducing this brief survey of characteristics and limitations of automatic data processing, it is as well to recognize that the technique of digital pulse manipulation has emerged from skills that were deployed so effectively in fields of electronics during the war years for radar and its associated computing devices. These skills enabled electronic components and associated devices to be assembled to form basic "bricks" which are capable of the essential underlying processes of logic, the "and", "or", and "not" circuits. It is these basic units which, when assembled and organized into the large units that we call computers, enable complex tasks of mathematical and logical analysis to be undertaken.

Characteristics and Limitations of Automatic Data Processing Systems

The essential external characteristics that differentiate automatic data processing systems from office machinery of the traditional kind are:

(i) Ability to process data to a predetermined programme of instructions without direct human intervention.

(ii) Ability to select the most appropriate course of action to process that data from a variety of possible courses of action contained within that programme.

(iii) Ability automatically to translate information presented to the system into the appropriate language required to operate the machinery.

Secondary characteristics of degree rather than kind contributing to this differentiation are:

(iv) Ability to process data at very high speeds.

(v) Ability to manipulate very large volumes of data.

These essential and secondary characteristics of data processing systems enable four main types of function to be performed:

(*a*) Routine clerical operations of a high repetitious and precise nature.

(*b*) Recording and measurement of events (e.g. stores transactions, census counts, etc.) as well as the subsequent analysis of these events from which either

(*c*) further processing to assist lower level management decision making, or (*d*) still further processing will make it possible to exert more exact overall management and control.

Basically, automatic data processing systems can perform only five simple operations:

(i) They can read, not yet written matter, but information that has been coded on cards, paper or magnetic tape, at rates up to several hundred twelve-digit numbers per second.

(ii) They can remember what has been read by storage devices using either relays, valves, magnetic cores, or tape, or circulating pressure waves in columns of mercury. Memory may be deep—almost "subconscious" and require considerable time and effort to reach, or it may be virtually instantaneous. It may be extensive, say of the orderof 10^9 bits, or restricted, say only 1,000 bits.

(iii) They can do basic arithmetic, multiplication, division, addition and subtraction. Addition is easy and fast, adding say two twelve-digit numbers together in a few tens of microseconds. Multiplication and division usually take longer, say, several hundreds of microseconds.

(iv) They can exercise choice. They can choose between one course of action and another, depending on the relative magnitude of quantities stored in the memory.

(v) they can write, on card, tape or paper. Printers are available that will print up to 300 lines of print each containing over one hundred characters in one minute.

These five essential functional components of automatic data processing systems suffice for most foreseeable tasks.

In very nearly every application which can be foreseen data are received in random sequence; for example, entries on stores ledgers are random over sequences of item catalogue numbers; the basic entries on shipping documents out of which statistics have to be derived contain very little order, except certain natural groupings within very small ranges. Although initial recording is most usually random, it is however traditional for large volumes of data to be stored serially. Indeed, accessibility to stored data has, up to the present, been possible only if the data storage has a recognized order or pattern, usually a straightforward numerical or alpha-numerical serial pattern. For example, entries in registers of births are filed in time serial and within that grouping, in alpha-serial. Registers of library information are filed in some classification serially. An essential internal characteristic of most data processing systems is, therefore, that they shall be capable of carrying out the formidable tasks of random and rapid access to large volumes of information stored serially but interrogated in random sequence.

Without considering technical detail, it is useful to review some of the essential limitations of mechanization of office procedures and systems. The more important appear to be:

(i) Machines are the quintessence of orderliness. They cannot "stretch a point", neither can they answer the sudden awkward question. Unless every minutiae of the clerical process can be reduced to a rigid logical set of rules, the task is not amenable to mechanization. For example, no computer can be visualized that would manage discretionary outpayments from a benevolent fund.

(ii) Machines have no peripheral vision, i.e. they cannot solve a specific problem which is contained within specified boundaries and at the same time bring to bear upon that problem consideration of effects which lie outside those boundaries, unless those conditions have already been written into the machine programme. (Although machines have no peripheral vision, they can be extremely powerful in seeking for and recognizing pattern out of chaos. In fact, it is through the deployment of such techniques that the machines may be so ordered as to recognize past inefficiencies or errors and, therefore, are sometimes spoken of as having a "learning" capacity.

(iii) Computers can be instructed to learn but only in so far as it is possible to teach the machine those things the programmer had in mind when he wrote the programme. It can thus be programmed so that it operates with conditioned reflexes. For such purposes, the computer may be visualized as having a second computer by its side analysing its own performance and directing it. Whilst the over-all system represented by the two computers can be regarded as improving its performance asymptotically, the final performance achieved by it cannot exceed that which its designer had in mind for it.

Future developments of data processing systems may be expected to yield designs that are improved in speed, size, cost, power consumption and reliability, together with the ability to read directly from printed documents and with a high degree of self-correction of errors, even to the extent of the machine refusing to accept information containing errors that humans try to feed into it.

It is also likely that programming will become easier by the use of automatic translation routines which convert from conventional (but stylized) prose, containing instructions in fairly broad terms, into detailed orders in a coded language understood by the computer. It also seems possible that as machines are applied to office work on an increasing scale, more complex but more flexible arrangements of equipments will be produced. These may, for example, enable an assembly of equipment to accept, in random order, documents relating to a variety of processes which the machine will sort and deal with in terms of a range of stores programmes which will be called into play as needed by the flow of work. In the interests of flexibility and reliability, large machines in widely separated offices may well be linked by data transmission systems so that they can operate as an entity, exchanging data between each other as the need arises.

A Brief Survey of Possible Applications of A.D.P. to Statistics in Government Service

Two broad areas of statistical work are being studied for applicability and suitability to automatic data processing; those generally spoken of as being census-derived, and those which are derived as a by-product of other work. An example of the former is the census of production undertaken by the Board of Trade. Examples of the latter are staff statistics derived as by-products of pay roll or stores accounting operations. All this type of work is characterized by very limited mathematical treatment of data, but it calls for large and complex storage and access devices to enable sorting and analysis to be successfully undertaken. It is also characterized by the very diverse and uncontrolled origin of much of its information, a fact which introduces very difficult problems of data interpretation, verification and translation.

The application of automatic data processing techniques to statistical work can be regarded as divided into three phases, although these phases will not be clear-cut and the boundaries between them will overlap considerably in time. In the first phase, we shall be planning systems which can be regarded as minor excursions into the field of data processing. In these applications we shall be undoubtedly not using A.D.P. techniques to their maximum potential advantage. As we now see it, we shall be limiting the automatic data processing technique to ancillary tasks such as credibility testing. Even with such a circumscribed target, however, we should be able to achieve significant economy in clerical staff. In the second phase, we hope to be using major A.D.P. installations for over-all processing of entire blocks of statistical work. Before this may be done, however, more comprehensive techniques for the manipulation of very large volumes of data, as well as for the extraction and grouping of essential parameters of that data, have to be introduced. In the third phase, it may be possible to eliminate the very large volumes of clerical work associated with translation of initial documents into a language understood by automatic data processing equipment. This predicates the availability of equipment which can "read" the initial data documents and convert the information contained in the characters to electrical signals without human intervention. It is possible that developments now in hand will result in commercially available equipment to read typescript in the relatively near future. It is then a problem of organization to ensure that the information presented to the statistical offices is, to as large an extent as possible, in the form of type which is acceptable to the automatic reading device.

Advantages that A.D.P. systems are expected to yield include more rapid processing, progressive improvement of limits of credibility checking, an increase in over-all accuracy and the ability for more powerful and direct analysis of trends.

Conclusion

In a brief introductory statement such as this it has been possible to do no more than indicate the perimeter of the subject. Following contributors will wish to deal, no doubt, in detail with aspects that it has not been possible to include in this introduction, aspects such as the human problems that are emerging as a result of the need to train staff in the techniques of A.D.P., the characteristics required of A.D.P. operators and programmers, planning and operational problems of matching tasks and machinery, exploration of techniques such as linear programming or the analysis of organizational problems posed by the insertion of an A.D.P. system. In all these problems it seems clear that, to an increasing extent, there must be the closest co-operation between A.D.P. system designer and user, particularly in this field of statistics where it seems possible that the users' requirements may predicate machine design to an extent not found in other applications.

Mr. D. T. Caminer (LEO Computors Ltd.):

Some Remarks on Data Processing on LEO

The time-table of computer loading for a representative week in January of this year will serve as a set of pegs to hang a few otherwise disconnected remarks on the LEO experience of data processing.

Apart from time spent on tests, maintenance, and trials of new and amended programmes it will be seen that time is allotted to jobs with four different prefixes. The first three are productive jobs with the prefixes L, E and O respectively. The fourth covers, so to speak, computer housekeeping jobs with the prefix M.

The L jobs are the clerical data processing jobs carried out for the parent company, Lyons. The E jobs are the clerical data processing jobs carried out on a service basis for other companies. The O jobs are mathematical and statistical jobs carried out for the Lyons' laboratories and market research organization and for other research bodies, nationalized industries and so on.

Although the nature of the calculations to be carried out on these different kinds of work is very different in character and though the end products serve very different purposes, they are still, in terms of operations to be carried out on the computer, very similar. If an onlooker walks into the computer room, he will not without some questioning of the operators be able to see at a glance whether it is an L or an E or an O job or, for that matter, an M job, that is being carried out. He will generally see data being fed to the computer by cards and tape through two or three input channels and he will generally see results being produced on a line printer and hear the concurrent crunch of a card punch. They are all data processing jobs of one kind or another and jobs on LEO tend to look very much alike.

The same computer operators run all jobs on LEO: there is no question of bringing in mathematicians to run the computer while mathematical jobs are being run or to have particular programmers run jobs on the computer because they happen to know more about them. It is our belief that if data processing jobs are to be run accurately and economically in a controlled manner on a computer they must be run by experts, with the particular qualities of ability to adhere precisely to a set procedure, of calmness in face of unexpected happenings, and of resourcefulness in face of machine stoppages or confusions caused by human errors in the original preparation of data.

The operators must be people with the ability to switch their minds and the computer rapidly from job to job, clearing up tidily after each of them. For example, on the timetable for Thursday morning it will be seen that five different jobs are scheduled in the space of less than 3½ hours. Each of those will have had its own special data to assemble for the computer and in each case the line printer will have had to be set up with the particular stationery required for the job. In four of the cases preprinted stationery will have had to be aligned ready for use. There are only two operators on the floor at any time, assisted during the day by a girl assembly clerk. They all work on the basis that computer minutes are extremely precious things that, if lost, cannot be made up by any extension of working hours, since the computer is already scheduled to be switched on for 168 hours a week.

I have said that the onlooker will not easily be able to distinguish the nature of the jobs from the visible manifestation of the computer. It is probable that there has been overmuch distinction in the past between, on the one hand, the clerical job with its emphasis on a high data and result content and elementary calculations and, on the other, the mathematical job with little data and a great mass of obtruse calculations and a mere line or two of results.

LEO TIMETABLE

	8.30	9.30	10.30	11.30	12.30	1.30	2.30	3.30	4.30	5.30	6.30	7.30	8.30	9.30	10.30	11.30	12.30 a.m.	1.30	2.30	3.30	4.30	5.30	6.30	7.30	8.30
Mon.	Maintenance and tests		L5	Trials	L5 L1		L2		M15 E1			O49	O51												
Tues.	Maintenance and tests		L5	L1	L5	Trials	L2		L1					O51											
Wed.	Maintenance and tests		L5	L1 L5 O81	Trials		L2		L2C				O51												
Thur.	Maintenance and tests		L5	O65	L5 E19 O9	Trials	L2		M14 Trials		O85 O74			O51											
Fri.	Maintenance and tests		L5	L8	L6 L5 Trials		L2		E1					O51											
Sat.	Tests	L4		L2			Trials	L1		O67	O51														
Sun.	Tests	O51																							

It is true that some mathematical jobs tend to be *triggered* by their data, rather than to follow the clerical character of *processing* their data, but those are probably the exception rather than the rule. Indeed, in both the mathematical jobs on this timetable, O67, which is concerned with the performance of an experimental diesel engine, and O74, which is concerned with the design of a guided missile, the data content is very significant indeed and so, too, is the volume of results that is printed. It emerges that one of the major reasons why mathematical jobs so often provide for so few results is that the results are expensive to produce on digital computers designed in the first instance with an eye to computing speeds alone.

I believe that a more profound difference between the clerical and the mathematical and statistical data processing applications is that generally the clerical jobs have to be done to a very precise timetable while the mathematical and statistical jobs, on the whole, have a much wider tolerance. Looking at the loading schedule again it will be seen that the L2 job starts each afternoon on LEO at 2.30 p.m. except on Thursday, when a little extra has to be done in preparation for the week-end. The L2 job is concerned with the goods to be sold next day in the 150 Lyons's teashops in the greater London area. The goods are in the main being made while the job is going on. The final production orders for the more perishable lines are provided by LEO. The despatch organization which begins assembly of goods in the early evening relies upon LEO for the packing strips for the individual batches of goods for the different teashops. The time margin is very small indeed.

Again, L1, the Lyons' pay roll, is carried out at set times. The Lyons's cashiers are geared up to expect the pay slips at precise times in order to make up the packets with cash before pay day. Similarly, E1, the Ford pay roll, has its appointed times. A plain van brings the clock cards at the agreed time and a plain van carries away the completed pay slips at another agreed time. Again, the cashiers are waiting at Dagenham to make up the pay packets, and delay means dislocation. Altogether, the pay of nearly 20,000 Lyons' and Ford's employees depends on LEO doing its job at the right time; starting with 1,700 employees three years ago, with steady increases as circumstances permitted, the pay slips have been produced by LEO without any recourse to standby whatsoever. During all that time there has been no standby computer to which recourse *could* be made.

All this calls for a standard of hour-to-hour reliability such as is not, in general, demanded of computers designed for and dedicated to purely mathematical applications. It is an aspect which, conscientiously considered, means that the most careful deliberation must be made before any new input/output or storage medium is linked to a computer developed for clerical data processing and before any new component is incorporated in the design. With reliability the keystone of a computer that will do its job as regularly as a building full of clerks, experiments have to be carried out in the laboratory and the workshop rather than in the automatic office which has already taken responsibility for a clerical work load.

My remarks add up, I think, to something that is, I suppose, generally acceptable but may not be obvious with the still limited experience in the field of data processing in this country. That is, that after the honeymoon of consideration of computer potentialities and application planning has passed there comes the time when you have made your choice and must live with your machine. When that time comes, if the computer is to earn its keep on commercial data processing, it must be operated to a timetable in a workmanlike manner and it must work reliably. Those are positive preconditions to economic working.

LEO Operation January 1957

*L*1—*Lyons Pay Roll*

Working from pay hours LEO calculates gross wages and, from brought forward data automatically produced by the computer, calculates net pay and prints a pay slip giving details of the make-up of wages for each person on the pay roll. The calculation includes computation of income tax and all other deductions, the preparation of a coin analysis for use in the physical make up of pay envelopes, and the provision of all other information required for maintaining company accounts, cost accounts and other statutory and statistical returns relating to the pay roll. At present the application covers 10,723 employees and is to be extended to cover the remainder of Lyons's staff as soon as the second computer becomes available.

L2—Supplies to Branches

This covers the supply of up to 250 items of goods each day to 150 Lyons' branches. LEO holds approximately 30,000 daily standing orders and every day approximately 8,000 revisions to these are received by telephone. Within an hour of receiving the last standing orders, revisions are collated by LEO to produce the total production orders, the sub-totals required for the movement of goods in bulk for despatch and the details required for packing goods for each branch. In addition deliveries to each branch are valued and cash totals accumulated. Stock control totals are provided and statistics are produced for management.

L4—Stock Control

LEO is supplied with details of stock purchased and issued. Each week LEO calculates a new average price for each of 750 categories of tea and produces a statement showing the quantity, price and number of weeks' stock in hand, the quantity to be purchased to bring stock up to minimum level, exhausted and slow-moving stock, the trend of current prices and a comparison of actual with standard costs for each production batch. In addition, LEO produces each quarter a stock valuation sheet at actual and average price. The weekly statement showing the position at the close of business on Friday is available to management on Monday morning, and the entire job involves the making of 30,000 separate calculations each week.

L5—Invoicing

L5A—This is a daily job carrying out the routine clerical tasks relating to valuing orders, compiling packing instructions and producing day book and stock record figures.

L5B—In this job the cash collected by salesmen is checked, and their under- or over-banking reported.

L5C—Here, collection lists are produced for each salesman, showing the value of orders despatched to customers and previously uncollected amounts.

L5D—A weekly task for compiling sales statistics and calculating salesmen's commissions.

The over-all time for dealing with all these jobs amounts to five seconds an invoice. Until such time as a second computer is available the application covers only a limited number of areas.

L6—Sales Statistics and Delivery Planning

The customers are grouped into classes and LEO is required to compare their purchases with standards which vary according to weather and other conditions. LEO then reports those agents whose purchases deviate significantly from standards and the information produced enables sales management to plan calls upon customers economically. In addition, LEO produces sales statistics for individual customers or groups of customers.

E1—Engineering Pay Roll

A pay roll similar to that referred to above is carried out on a service basis for a large engineering concern. It is at present limited to 7,000 employees, but is to be extended. In this case, the computer starts with hours worked as shown on the clock cards. It embraces a very wide range of labour and personnel statistics.

M—Mathematical Work

These are all service jobs and mostly not regular in their incidence. They include market survey, statistical and actuarial tables and a wide variety of scientific applications.

O67—Combustion calculations. Data read from photographs of the display shown by an oscilloscope during the running of a diesel engine are processed and presented in the form of graphs and tables.

O85—Pipe-line pressure calculations. The computation of needle-lift, flow and pressure, including cavitation effects, in a system consisting of a pump, pipe-line and nozzle.

O49—Pneumoconiosis research. The classification of miners with respect to exposure to dust during mining to assist in the prevention and cure of pneumoconiosis.

O81—Crystallography. The production of electron-density values to assist in determining the precise location of atoms within a molecule of a particular substance.

O9—Market research tabulation. The classification of consumer goods with respect to various sections of the general public.

E19—Pension scheme quotations. Calculation for an insurance company of the running costs, to an employer, of group policy insurance schemes.

O65—Actuarial tables. The computation of four volumes of varied actuarial functions based on a new mortality table.

O51—Railway distances. The calculation of the shortest distance between any two stations in England, Scotland and Wales.

O74—Guided missiles. Calculations in connection with design of guided missiles.

LO—Maintenance etc.

This includes routine maintenance, programming trials, and testing of new equipment, and also:

M14—Computer units movement control and valve statistics. Selection of units due for routine servicing.

M15—Weekly analysis of computer usage. The classification of effective time spent on each job and calculation of weekly computer efficiency.

Mr. H. L. BARMAN and Mr. D. EVANS (Rolls-Royce Ltd.):

THE USE OF ELECTRONIC COMPUTERS IN ROLLS-ROYCE LTD.

Mr. H. L. BARMAN: The introduction of electronic computers to the Company which I have the honour to represent here to-day goes back to the year 1952. Up to that time the Company had been using the normal hand methods, with hand-operated calculating machines, to make calculations connected with the design of various parts of jet engines, and with analysing the results from wind tunnels and test beds. From what was known of the work being done in other fields, it was clear that more powerful tools were becoming available to enable the Company's mathematicians to carry out stress calculations with greater ease, and by 1953 the Company had introduced a small electro-mechanical computer with a small memory. This machine proved extremely useful and very soon became overloaded. By the end of 1953 it was clear that a more powerful machine would be needed to provide greater detail and more accurate results for the designers, so a further machine of greater power was ordered.

In addition, at this time, it was realized that the work being done by Messrs. Lyons and in America using an electronic computer could be applied to commercial and office work. A small team was set up in 1954 to survey the machines available and the work being done in the United Kingdom, culminating with a visit, early in 1955, to the United States to find out what was happening there. In the meantime, the mathematical work was increasing as the design departments and development engineers realized the service that they could obtain, but even the second machine was not enough and a still more powerful machine was necessary. To the great regret of the Company, no British machine of the necessary power and reliability was available at that time, so an I.B.M. 650 computer was ordered and delivered at the end of 1955. As this machine was also to carry out commercial data processing I must make a digression here to talk about data processing from the commercial angle.

Rolls-Royce, like all big companies, went through a phase of expansion during the last war, which coincided with a time when it was difficult to get staff and that staff had to be dispersed into outlying districts because of air raids.

In 1938 the Company introduced punched cards, but only in a minor and experimental way, to assist the Works in controlling production. It was fortunate, as it turned out, that the Company had this punched card unit because during the war it found itself forced to expand and to mechanize many of the office processes, utilizing the female labour that was available instead of clerks doing more complex analysis, additions and postings. Thus, during the war, all through the factories of the Company, in spite of some opposition from the more conservative managers and supervisors, a thorough-going punched card mechanized system was introduced and accepted by the staff. This formed the basis of a system that might have taken many years and great effort to introduce in other circumstances.

When, in 1956, the problems of introducing electronic data processing came to be considered, the ground had already been prepared. I am making this point as we have learned from the jobs we have done that it is much easier to apply routines that are already on punched cards than those that demand clerical effort. My advice to those who are thinking of getting electronic computers to do office work is first to mechanize on to a punched card system, then change over to the electronic computer.

When we first considered the office problem we thought that we should be able to introduce an integrated system of data processing: for example, the operation of collecting information from the shop floor to calculate a man's wage could be combined with the collection of costing data and with the information necessary for production control. We very soon found that this was not advisable; an integrated system of this sort is very desirable, but to apply any problem to a computer calls for meticulous investigation of every detail, and the time taken to do this is so great, and the reorganization of paper work so extensive, that we should get nowhere. We, therefore, changed our tactics and now we are applying, to the computer, jobs which are being done on punched cards. When we have got each running satisfactorily we shall then start integrating one with the other.

Of the various jobs we have tried one or two are quite interesting. We have, of course, applied a pay roll—this is a standard job which practically everybody who has an electronic computer attempts—also stock control, and we are examining the problems of production control, that is, getting the right article in the right place at the right time. A job of great interest which involves a principle that will have to be introduced extensively, is control by exception. With the volume of information to be processed it is essential to sort out the wheat from the chaff. For instance, we have 50,000 piece numbers, and much ramification of data required, in one form or another, throughout the organization. It would be advantageous if the electronic computer could sort over all the data, digest those which are required, compare the results with pre-determined data, and only throw up the major differences.

As an illustration of this, we have started and are running as a regular job, a monthly analysis of all our spares sales. All the detail items on the invoices are fed into the computer. Firstly the computer checks that all the calculations have been done correctly, it then turns to its memory, picks out the standard costs for those items, calculates the actual cost by applying the monthly variance ratios that have been already fed into the machine, compares the selling price with the actual cost and finally works out the margin. There are 48 different categories of sales, and in each of these categories the margin expected is different. For each sales category a high and a low limit are set, the machine checks each individual item on the invoice against these limits, and should the margin fall within these limits it ignores it, but should the margin fall outside one limit it prints out an exception. These exceptions can then be examined both by the sales and cost people to find out why the margin is not the expected margin. This, of course, reduces the volume of work and checking enormously; we only have a few items to deal with instead of many thousands. In addition, this operation provides the necessary total figures for posting to our accounts and cost ledgers. You will note that we started with an invoice. When we integrate we propose starting at an earlier stage from the documents in the stores, and preparing the invoice in the computer—this illustrates integration. We hope to apply the same sort of techniques to production control where we expect a manufacturing operation to be done on a certain day. If it is not done on that day the machine will produce a list on which the management can take action.

Mr. D. Evans: Before dicussing in detail some specific technical problems, I should like to give a broad outline of the work which we are called upon to complete. The work embraces the jet engine, nuclear reactor and rocket fields, and in the past 15 months we have completed 125 programmes, 74 per cent. of which have been major studies.

To date, we have handled a considerable amount of work relating to jet engine performance and analysis; reduction of wind tunnel test, flight test and engine test bed data; solution of simultaneous linear and non-linear equations; solution of simultaneous linear and non-linear differential equations; solution of partial differential equations; least squares curve fitting; smoothing of data; Fourier analyses; random number generation; Monte Carlo processes and many other technical problems. Some of these topics involving statistical techniques, e.g. the handling of graphical data in computation and a random walk problem, will now be discussed. It is hoped that the ideas contained herein will help to stimulate discussion on these topics.

Most important forms of engineering computation utilize data in the form of graphs. Often these data are empirical in nature and not amenable to analytic representation, e.g. jet engine analysis methods for a single data point may require the relating of data from a large number of graphs. A manual solution may take 2–3 hours per point. We have evolved techniques which cope efficiently with this type of problem, and a solution can be obtained on the computer in 30 seconds. The setting up of such problems on the computer involves representing the graphical data by a mesh of points. This mesh of points may be in two- or three-dimensional form. The computer is then capable of locating the closest point in the mesh of points and calculating the corresponding value of the function by polynomial interpolation.

A description will now be given of an application of the Monte Carlo method applied to certain neutron diffusion problems. These problems, involving complicated partial or integro-differential equations, have recently been solved by utilizing various probability techniques and sampling methods. The Monte Carlo approach consists of permitting a neutron to play a game of chance, the rules of the game being that the actual deterministic and random features of the physical process are, in turn, exactly imitated by the game. By considering very large numbers of neutrons, the distribution of the neutrons at the end of a certain period of time can be determined. One important feature of this approach is that the partial differential equation describing the diffusion process is by-passed completely, the probability model used being derived from the process itself.

For this particular example, an arbitrary number of neutrons are permitted to go off in a random direction, travel a random distance, collide with a neutron determined in a random way and lose an amount of energy determined in a random manner. Hypothetical case histories for a number of these neutrons are built up as they undergo a series of random collisions. The main work of this problem is to follow the selected neutrons through successive collisions until they are absorbed or fall below a certain energy level. Final compilation of the data is usually frequency distributions such as distribution of the neutrons at the end of a certain period of time.

An associated problem with the Monte Carlo process was the generation of random numbers. During the early stages of the problem it became obvious that access to an unlimited supply of random numbers was necessary. The only practical solution to the problem was to generate them at high speed internally in the machine during the course of the calculations as required. Therefore, attention was confined to the generation of pseudo-random numbers by arithmetical processes on computers. A multiplicative method of generating a sequence of numbers of period $5 \cdot 10^7$ was chosen and 4,000 pseudo-random digits were produced per second. Tests of randomness were applied to numbers generated this way and the results were found to be completely satisfactory.

Dr. Henry Durant (Social Surveys (Gallup Poll) Ltd.):

ELECTRONIC PROCESSING EQUIPMENT

Whether I belong properly to the present company I am not quite sure. Our problem is a special one. Our experience in the electronic field has been slight: nevertheless, I am assured that it is interesting and, comforted by this assurance, I will contribute what I can.

Our problem is to calculate approximately 1,000 percentages each working day, using a considerable number of bases for the calculations. If there were only one base, there would be no problem whatsoever. The problem is not only one of magnitude but one of speed, because opinion and market research reports are always wanted in a hurry.

We attempted to solve the problem by using IBM machinery, installing the following battery. With an IBM 101 electronic statistical counter, were coupled two automatic punches each punching a statistical card for 30 print positions, since the 101 will print 60 at a single run. These statistical cards were then fed into a 626 electronic calculating punch which calculated the percentages and in turn the cards with the calculations were fed into a 420—a tabulator, which printed out the results. In theory the process was a simple one; in practice it did not work, in terms of speed or cost. Where we had only one base for calculation the speed was quite adequate, but such a situation arose only in straightforward opinion surveys, constituting a small proportion of all our work. Probably many of the problems could have been settled if we had been prepared to pay more money for bigger machines, but money is one of those items to which the economist's dictum applies, the application of scarce means to alternative resources. The other solution is, of course, that the producers of electronic equipment should specifically design machines for small operators such as ourselves. With the battery that we had, we believe that it would have been possible to have eliminated the 420 completely and for the 626 to be linked directly and solely with the 101. The 101 would then have counted the cards, fed the results into the 626 which would have made the calculations and then fed back the calculations to the 101 whose mechanism would have been utilized to print the percentage figures. The storage problems would not have been great and this simple battery would have worked very quickly and economically in terms of price and of space.

However, the demand for this kind of combination is not great and it is the demand, the estimated demand at any rate, which determines the state of the market at any given moment.

May I, at the same time, enter a plea for standardization of machines and of operations together with what is its opposite, as much flexibility as possible. Let me illustrate what I mean from our own experience. On the IBM 101 there were three sort-selection switches which were to be used separately according to the book of instructions handed over to us with the machine. Together with this was a plug-board which needed to be plugged up for each operation. By using the switches in conjunction, and not separately, it has been found possible to develop a standard plug-board which will cope with any piece of tabulating which we need today. The gain in time can be imagined, since all that our operator has to do is to move five plugs and five only. The other gain is that we do not need to employ highly skilled operators, and this is a very important consideration indeed. I may say as an aside that we have definitely given up the idea of increasing the number of one type of calculating machine we have, simply because the operators cost so much money, and we have chosen an alternative.

Standardizing the machine, of course, reduces maintenance time and our experience in the electronic field with electronic machinery is that it is well worth allocating time for maintenance. Under great pressure of work we refused to let the mechanic come near the machine but we have paid for it in terms of breakdowns. Our experience with non-electronic tabulating machines was that the best policy was to let them continue until they had a breakdown because the interference of the maintenance man, whatever may have been the benefits that accrued as a result of his visit, often resulted in the machine requiring further attention the next day, although at the time of his visit the machine might have been running very well indeed.

Whilst on the subject of the machine itself a further comment might be made that it is a pity that the manufacturers do not have in mind very clearly the need for studying the ease of operations for the operator. Not so long ago there was correspondence in *The Times* between one of the Engineers' Associations and one of the Designer's Associations, each claiming that they should have a large say, perhaps even the final word, in the design of a machine. I would not give it to either; I would give it to people who have to work the machine, because if it is unskilfully designed then much time and energy are wasted. With the 101 machine it is necessary to go to the side and open a door, to bring out the plug board. This takes time and it needs space to allow the plug-board to be taken out. The question has been raised as to why there should not be a kind of telephone exchange board placed at the eye-level of the operator so that, when sitting down, any changing of plugs can be affected without the operator moving, and certainly without the operator bending down. It seems likely that this suggestion will be adopted and, when it is, the operator's life will be that much smoother and more productive.

There remains one final point to be made concerning the machines. The failures that we had were almost invariably those arising from the electro-mechanical parts of the machines. The electronic parts seldom failed and when they did, being in standard units, the faulty part could be easily pulled out and replaced by a new one. As anyone will know who has had experience of the machines, this is by no means the case with the electro-mechanical parts and it often takes time for the faults to be traced. With machine rentals being as high as they are, this is a costly and frustrating incident. So far as I can see, the engineers do not pay sufficient attention to this aspect of the construction of their machines. They are now so hypnotized by the electronic part that the rest receives scant consideration. They are content to buy standard components from other manufacturers and whilst it would be unfair to say that their quality leaves something to be desired, it can perhaps be said that the specifications laid down by the purchasers are not high enough. I, myself, would hazard a guess that the best way of improving the efficiency of many machines at present is by devoting time and attention to improvement of the electro-mechanical parts of the machines. Much machine time, wasted at present, could then be utilized productively.

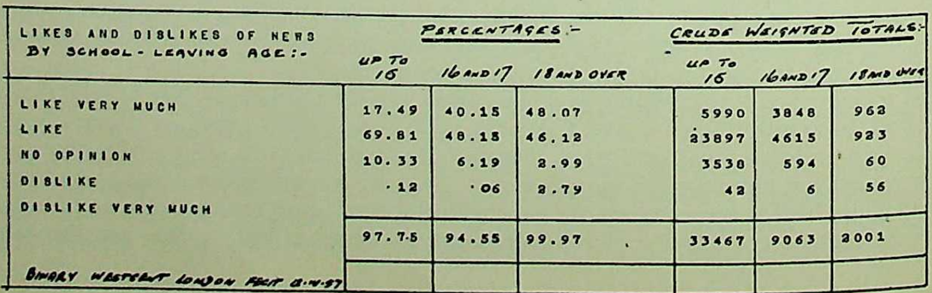
Mr. B. P. EMMETT: I am prompted to say a few words about electronic data processing as applied to survey analyses because, unlike the many other jobs that our speakers have described as successfully completed, there seems some doubt about the feasibility of this particular application. Though Mr. Caminer mentioned market research analyses among LEO's "O-jobs" (and incidentally I for one would welcome information about their methods of operation and costs) Dr. Durant's experience suggests that a range of simpler equipment may be more practicable than a large computer.

As he said, figures in the form of percentages are the fundamental results of market and opinion surveys. If the base of the percentages requires to be changed this, I would guess, must be for one of two reasons: either subsections of the sample have to be studied separately, e.g. age groups, sexes, etc., or certain questions are asked only of certain informants (e.g. those replying "yes" to a certain question are asked "follow-up" questions).

On a computer, more readily than on the intermediate range of electronic equipment such as the 626, the first of these changes of base can certainly be accomplished easily enough; and the second can certainly be done in theory and probably also in practice. A third possible reason for changing the base would be to eliminate the (usually small) number of persons who failed to respond on certain questions. This could be a very finicking thing to do on a computer and I personally feel that it need not be done at all—that it would be better always to include the percentage of non-respondents when presenting the results on each question. The real question mark, in fact, lies not in the difficulty of organizing the job on a computer in order to give the percentages required, but in the cost of doing so. Quite apart from running costs there are programming costs and there can be no doubt that unless standard programmes are devised that can be used over and over again on different surveys, electronic processing will not be worth while, except when speed is paramount.

However, such standard programmes *are* feasible. Mr. Caminer, I feel sure, has considerable experience of this and so I understand have many others, particularly Rothamsted, in dealing with a wide range of experimental designs. We in the British Broadcasting Corporation Audience Research have recently had a survey analysed on a computer using a programme that could readily be adapted for most of our surveys. We were prompted to use a computer in this first instance because the sample was unusually complex. It was a probability sample selected from the electoral registers but to say that it was non-self-weighting is an understatement. Every interviewer's work received a different weighting factor and, furthermore, jurors and non-jurors also needed different weights. In these circumstances a computer was a tremendous help—to do the analysis on punched card machinery would have meant reproducing a considerable number of cards and increasing the already fairly heavy load of calculations required on the counter-sorter results.

The end product of the analysis is exemplified below where one of the many tables produced is shown. The computer output consisted of two cards for each row of information, the first containing the percentaged results and the second the raw weighted totals. These were sorted into order together with previously prepared alpha-cards which gave the row descriptions.



LIKES AND DISLIKES OF NEWS BY SCHOOL-LEAVING AGE:-	PERCENTAGES:-			CRUDE WEIGHTED TOTALS:-		
	UP TO 16	16 AND 17	18 AND OVER	UP TO 16	16 AND 17	18 AND OVER
LIKE VERY MUCH	17.49	40.15	48.07	5990	3848	962
LIKE	69.81	48.18	46.12	23897	4615	923
NO OPINION	10.33	6.19	2.99	3538	594	60
DISLIKE	·12	·06	2.79	42	6	56
DISLIKE VERY MUCH						
	97.75	94.55	99.97	33467	9063	2001
BINARY WESTEND LONDON FECIT 12·4·57						

The programme in fact led straight from the punching of the material to the final tables required.

But quite apart from such non-self weighting samples, which must be the exception rather than the rule in market and opinion research, our experience suggests that computers can compete with other equipment as regards costs, even on more normal samples, provided of course that the programming costs are spread. Even if the sample itself is self-weighting, the results may not be—a weighting by price might be required if consumers' purchases are being studied, and so on. And this is an expensive procedure on a desk machine. Electronic analyses may even show a net gain if further refinements are introduced such as limiting the output to those figures which—on some set criteria—are deemed to be important. Mr. Barman pointed to one instance where this was an advantage. In market and opinion research also there appears to be a need for this kind of discriminatory process. And of course it can be done. In fact, subject to correction, I understand that on certain LEO jobs it is being done.

There is much more that might be said on this aspect of electronic data processing of survey results but the real need is for the further development and improvement of multi-purpose programmes. Practical experience of working costs could then very rapidly be accumulated.

Mr. MANDEVILLE: I found myself using punched cards in the analysis of Market Research questionnaires for the first time some twenty-five years ago and one of my first clients was Mr. Robert Silvey who is now head of Audience Research at the British Broadcasting Corporation. From that time onwards it has been my fate to be concerned in applying machines to the analysis of more and more complicated questionnaires. This has called for a fair amount of experimental machining, the success of which could seldom be determined with any certainty until the figures had passed control. Mr. Silvey has in the past been a victim of such experiments but in spite of that was willing to allow me to undertake the further, greater and more hair-raising experiment of putting the analysis of one of his surveys on to a computer. Owing to the complexity of the weighting desirable in this case, as described by Mr. Emmett, there was much to be said for this venture.

We were lucky in being able to draw on the great store of information built up in this country at the National Physical Laboratory and by the English Electric Company and to be able to hire Deuce by the hour at Marconi House. I am glad to emphasize that this work was done on a British computer. But although we could use certain library sub-routines the bulk of the programme had to be worked out from first principles and cost us months of programming and testing.

We are proud of this achievement because it has, as it were, "broken the sound barrier" of survey analysis, producing not only the counts but the percentages in double quick time. We took some 1,500 cards to the Deuce on a Friday and the following Wednesday were able to present our client with the tables, listed as is shown in Mr. Emmett's illustration, containing over 12,000 total weights and the 12,000 percentages corresponding. This was done in under a week but it could have been done in a day. The manner in which the results are presented could break yet another barrier to speed in analysis—the printing barrier. Our lists could be in such a form as to enable them to be photographed and tables run off on any office offset press, or the lists could be made on stencils previously ruled and headed.

Suppose we had had 12,000 cards to put through instead of 1,500 the difference would have been about four hours—running the cards through at 200 a minute in four different arrangements giving 11 separate sets of output cards, each containing the totals and percentages for one breakdown.

I am happy to present this after so many years of battling with the comparatively slow methods of ordinary punched card analysis, and I hope this venture may encourage others to think along these lines and venture yet further.

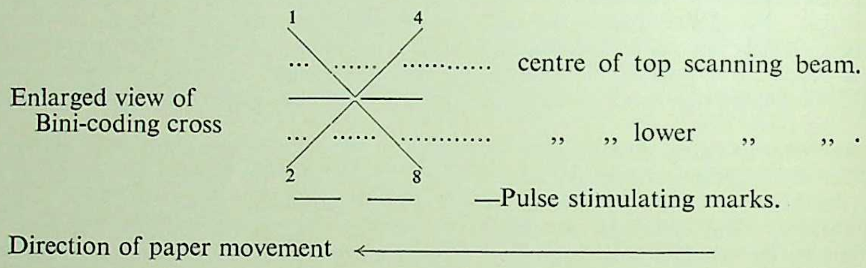
Added in writing:

A further matter within the scope of this discussion is the need for the development of special types of equipment enabling the Census to be analysed using the computer capacity available in 1961. In my paper on "Improvements in Methods of Census and Survey Analysis" (this *Journal*, 1946) I showed that development of the punched card system arose from the efforts of the American Government to speed up the analysis of the Census, and it was only after such machines had been developed that their further uses in accounting were realized. Had the Government waited for the commercial development of such machines much time would have been lost. I argued that the production of scanning equipment enabling the census forms to be converted to punched cards was the type of development that could be helped by the Government using the Census as an encouragement to invention along these lines. I indicated that a possible solution would be found in altering the method of coding used in the Census by introducing a script designed to facilitate scanning.

That no such development took place ten years ago is not surprising because there was at that time so much development waiting to be done in the design of electronic computers that the design of script-scanning machines was seen to be a stage that might be deferred. Times have now changed and there is already on the market a device for scanning typewritten script using conventional characters. I would once again urge that attention be paid to the very special requirements of the Census and would suggest that the following possibilities beyond those I proposed in 1946, are worthy of consideration by the Government. The contribution made by Mr. Merriman of the Treasury encourages me to think that such suggestions may not this time fall upon unresponsive soil.

Firstly, the census form could be printed with a series of pulse-stimulating marks. This would get over the difficulty of feeding such documents to exact register after they have been handled by coders.

Secondly, the codes used by the coders would be written in a functional script that took advantage of the accuracy of the pulses and was read not only by two scanning beams as suggested in my paper but at two separate pulse-times per character. This system would enable parts of the ordinary cross, or capital X, to signify binary values and would allow each character a range of values of 1 to 15 (a form of binary called Bini-ten). The outlines of the coding positions showing the coder where to put the parts of the cross would be in a colour or of a thickness insufficient to actuate the photo-electric cells. Such a cross would be as shown in the figure below:


Enlarged view of
Bini-coding cross
1
4
2
8
centre of top scanning beam.
,, ,, lower ,, ,, .
—Pulse stimulating marks.
Direction of paper movement

Thirdly, the census forms would be moved at a speed of 6,000 an hour or more past a photo-electric cell scanning system by the automatic paper-feeding device, well-known in the printing trade, to which had been synchronized a magnetic-tape feed. This slow-speed tape would record in parallel six or more sets of coding pulses representing the six or more persons recorded on one census form. This scanning unit would be designed to be made in quantity and I would propose that each Census centre should have its coders and tape-producing scanning unit.

Fourthly, the tapes would be sent to a central point where a special machine would convert the pulse record on the tapes into records on magnetic tapes or films suitable for computers.

Fifthly, the analysis of the Census could then be carried out on the computers available in 1961, using them at night to produce summary cards from which the tables could be printed.

It is not necessary to enlarge on these possibilities and I offer them to Mr. Merriman to be included in some Treasury O & M file relating to 1961 Census analysis; recalling, as I do so, the happy days I spent in that division during 1942.

Mr. Berners-Lee (Ferranti): I was particularly interested in Dr. Durant's remarks about the attitude of manufacturers to the user in the design of equipment. We in the Ferranti organization claim—and I think this claim is completely justified—that we were the first organization to make a computer with the specific object of its being easy to use. It very nearly created a revolution among the engineers—they did not want to do that at all.

However, we were very fortunate in having the services of an extremely experienced and able person, Mr. Christopher Strachey, who had been working in Canada on the St. Lawrence seaway project, and had worked in Cambridge. Also while in America, he had familiarized himself with a number of American machines. As a result he had a very clear impression of what a computer ought to be from the user's point of view.

To apply these ideas involved building a computer with substantially more components than would otherwise have been the case. I remember very clearly the decisions being taken and at the time I had a lot of sympathy with the engineers. I thought they were going to be asked to do a very great deal of work and the benefit was not clear. I am glad to say I have been proved decisively wrong. The result of the policy of making a machine which is supremely programmable is that a lot of people have a lot of programmes and one can compare the time taken to get comparable programmes working on different machines.

A striking fact which has come out of all the work is that the whole question of the difference between commercial work and mathematical work has been very much over-estimated. I have spent some time on commercial punch-card working, and my conclusion is that people who say a mathematical machine is of no use for commercial work are commercial people who know nothing about mathematics, and those who say a commercial machine is of no use for mathematical work are mathematicians who know nothing about commercial work. On most programmes, whether commercial or mathematical, during 80 per cent. of the time the machine is working it is doing no sums at all, and all this organizational work is common to a mathematical calculation or a processing calculation. The proportions may vary slightly.

We designed a machine to be really good at mathematical work. This meant that as far as possible we had to get the times taken to gain access to information and the times taken to use information in balance. When we turned to commercial processes such as sorting we discovered that the machine was capable of carrying out sorting processes at a very remarkable speed. It has been said, and is probably true, that the computer is the most expensive sorter that has ever been built, but the convenience of having a completely mechanical operation is a factor which weighs very heavily.

Like Mr. Mandeville, I am interested in the question of output of stencils. Among the many jobs done in our department was one which involved the calculation of well over 400 tables, and 500 copies of each had to be reproduced on "Multilith" masters. Although there were various errors in the early stages owing to incorrect programming, in the actual computer work, in about 1,000,000 characters, there were only two errors in about a couple of miles of tape. One concerned a decimal point and the other was the omission of a space. This certainly proved the equipment.

Mr. LUERY: We have heard a great deal about the scientific uses of this machine and something about the commercial uses. The big bugbear from the commercial point of view seems to be not the time taken within the machine but the time taken on input and output. We have heard very little about this in the discussion. I know that output is being speeded up and that, for example, Elliott's are producing something which will print about 600 cards a minute. I am interested commercially in producing 2,000,000 or 3,000,000 statements in about four to six weeks and wonder if anyone can tell us of progress in that field.

Mr. BATTERSBY: I work in a small organization with about 1,500 employees. Our managing director has recently set up a computer committee, one of whose jobs is to investigate the applications and economics of computers in the medium-sized or small organization.

Much has been said about cost, but it seemed to us that the only way to find out about that is by trial: so we have tackled the question as a piece of pure research. A certain sum of money has been allocated for computer investigations, and it is realized this may have been thrown away. We can carry on our investigations as for any other research question. At any time we can drop them or continue them, and this method is made possible by the growing existence of computer service centres. By making use of the facilities which these centres offer for buying computer time by the hour or even by the minute, the small company can gain experience without incurring any heavy capital cost. We have already found one advantage in tackling the problem in this way and that is that one immediately has a marked appreciation of the problems of programming.

Dr. YATES: In view of the interest in survey analysis that has arisen tonight I may mention one job that we intend to put on our computer. This is the Survey of Fertilizer Practice, which is a large recurrent survey designed to ascertain how farmers use fertilizers on the various crops, rates of application, composition and so on. The number of farms surveyed per district is not very large, about 60, but at each farm information is obtained concerning all the crops on that farm, so that information relating to 1,000 or so fields per district has to be analysed. By the nature of the survey the information has to be weighted in a rather complicated manner.

There are two points I want to make about this survey. The first is that we have in the past tried to carry out the analysis on punch-card machines, using a rolling-total tabulator, coupled with a reproducer-summary punch. This was not a success, and we reverted to manual processes using Cope-Chat cards. One reason for failure was because of the difficulties in multiplication on our punch-card machines, but I do not think this would be overcome by the installation of more elaborate machines because of their high cost and relative inflexibility, and the need to punch intermediate results on cards, and so on.

The second point is that to analyse the data for about ten districts, more or less the maximum number surveyed in any one year in the past, occupied a team of about three computers for something like half a year. This year for the first time the Fertilizer Manufacturers' Association is co-operating and consequently the survey is going to be substantially larger than ever before, covering some 45 districts. Without an electronic computer we should be faced with an extremely difficult and serious problem of analysis, both from the point of view of recruiting and training staff to do it and also from the point of view of getting the job done with adequate speed. (The somewhat leisurely tempo of most previous surveys definitely requires to be improved if the information provided is to be of real value to the fertilizer industry.) I believe, although our plans and programmes are not yet complete, that the computer is going to solve our problem in both these respects.

If the computer can do this, even if the total cost in computer hours, etc., comes out at about the same as that of using a hypothetical staff, assuming they could be recruited, I do not think there is any question of the computer not paying hands down. (In this connection we should keep a sense of proportion. The cost of the field work of the survey will be of the order of £10,000; the annual fertilizer bill is about £60 million.)

There is one other point which is of interest. When we got down to this job we came to the conclusion that it was better to use punched-tape input rather than punched cards. There are so many items for which there is "null" information, i.e. blank or *nil* entries, that it is much easier to punch the remaining information serially, with appropriate designations, than to try to get the information in the required columns on the punched cards. This does indicate the extreme flexibility of electronic machines and the need to get rid of the notions one has when one is used to handling punched-card equipment.

Dr. TOCHER: In the past my interest in computers has been in their application to scientific problems, as well as the problems of their actual design and construction. It is only recently that I have begun to take an interest in their application in commerce and industry, and consequently my remarks based on a limited study may seem naive.

However, in listening to the various people who have described the process of the change-over from some hand- or part-mechanized method of processing their data to one of the new automatic methods, I could not help feeling that they had forgotten to ask themselves one important question. When one is going to make any change it is always well worth asking "do we really want to do this job at all?". It may well be that this is a task which in its entirety, or in some large part, is being carried on as a traditional procedure, and in fact plays no real part in the organization at all. Even if we decide we do want to do this job we must still ask "do we want to do it in the way that we are doing it now?"

One can think of data processing as a production flow; a flow of information. The problem of data processing is similar to the problems of production control in that we require to ensure that the information gets to the right points at the right times, in the right quantities, and in this context the "right" is related to the flow of real production in the plant. Because of the limitations of our old hand methods these objectives have probably not been met in the past, and with the prospect of automatic data processing these deficiencies should be uncovered and plans laid to eliminate them.

What this really amounts to is a plea that in a plant or organization one should not consider isolated data processing procedures, but should look upon the problem as a whole for the plant as a whole. This study of large-scale organization as an entity is a serious and difficult matter, and it is fortunate that a body of experience has been built up to deal with just this situation. I refer, of course, to operational research which has in the post-war years developed a number of powerful techniques for investigating and analysing intricate and complex systems, and I feel that this discipline should play a large part in the early stages in installation of automatic data processing.

Mr. BERNERS-LEE: I should like to say in reply to the last comment how much I agree with it from the manufacturer's point of view. The task of surveying how computers ought to be used, is ideally suited to operations research techniques. I think we can say that those firms which have approached their data processing problems in a scientific way and looked at them as problems to be thought of in a spirit of enquiry are making a far more practical and realistic and steady progress towards their goal than are those firms which begin by saying they must have something that is going to do the whole job from A to Z, without asking whether it is a reasonable or an economic thing to do.

It is possible that the really satisfactory uses of computers are not going to come through the replacement of clerical operations. I am inclined to agree with Dr. Durant that in fact this may not always be an economic thing to do. This may sound like heresy. The most economic operations may come when one has decided that one wishes, for example, to optimize something. I think the Shell Company have shown a lead and made extremely successful use of their computer in Amsterdam in carrying out such work. Similar developments may be expected in the engineering and chemical industries.

Such developments can result in substantial savings. Here one is trying to save capital rather than labour by employing it more efficiently and I think this is going to play a very important part in the realistic use of computers.

The opening speakers, in reply:

Mr. Merriman: Very briefly, may I underline the previous contribution, namely, the need to precede the installation of computers with exceedingly careful studies designed to reveal the true objectives of the organization.

Mr. Barman: On the question of operational research, it is agreed it can be helpful, but one thing that cannot be done with a computer is to change everything at once. Changes have got to be taken slowly and step by step or else the risk of failure is serious. There have been cases in America where the whole of the production control in the factory was put on to the computer and although many man-years were expended in preparation it failed and the whole works came to a standstill. Problems must be dealt with slowly and surely on the computer, because it can be a very hard taskmaster.

Dr. Durant: I should like just to emphasize the need for deciding beforehand what is really wanted from a computer, and whether what can be obtained is going to be of any practical use. To give a modest example; within the last few months two firms have approached us to ask whether we could take over the data which even an elementary punch card system is producing. The system had been sold to them chiefly on the basis that it could produce such data. Yet, when produced, they have found themselves absolutely overwhelmed and unable to cope with them. Hence their approach to us.

Mr Caminer subsequently replied in writing as follows: The points Mr. Emett has raised about standard programmes for survey analysis are of very considerable importance. There can be no doubt that time and programming costs can be saved if the consumers of computer services can agree to make use of programmes off the peg rather than "Saville-row" cut programmes designed to meet every undulation of their figures. Standard matrix manipulation programmes have long been in use in the aircraft and civil engineering industries, and statisticians in the survey field might well follow their example.

There has for some years been available for use on LEO a comprehensive survey analysis programme which automatically weights the information from the field in two dimensions, produces percentages on different bases, and prints selected cross tabulations as designated by the user. Several undertakings have used this programme, though not nearly as many as one would have imagined, having regard to the nuisance of carrying out weighting and vari-based percentaging on conventional equipment of the type that Dr. Durant describes.

Other programmes available for common use deal with concommitance and regression analysis. And, of course, there would be here such programmes if only the survey and motivation analysts would resolve minor differences and make their corporate needs known.

As a result of the ballot taken during the meeting the candidates named below were elected Fellows of the Society:

Nazir Ahmed.
Pamela Anwar.
Terence Stoneleigh Aram.
Denis Charles Benson.
Kenneth James Britt.
Ronald Salthouse Bunstead.
Francis Callis.
Victor Chew.
William Dennis Coles.
Mary Davison.
Geoffrey Donald Giddings.
Mohamed Ali Jaafar.
Kershi Dossabhoy Parakh.
Kenneth Arthur Pettet.
David Edward Rendell.
Peter Harrison Swan.
John Kirby Taylor.
Peter Thursfield.
Arthur James Verity.
Colin White.
John Keith Wyatt.

AN APPLICATION OF LINEAR PROGRAMMING TO THE TRANSPORT OF COKING COAL

By A. H. LAND

1. *Introduction*

THE application of the technique of Linear Programming to minimize (or maximize) a linear function subject to linear constraints is familiar and well documented (Koopmans, 1951*a*; Dorfman, 1951; Charnes *et al.*, 1953; Kuhn and Tucker, 1956). Probably the most frequently used form of linear programming, and certainly the simplest, is the Transport Model, associated with the name of T. C. Koopmans (1951*b*). In the transport model one is concerned with finding the best allocation of a homogeneous commodity available in known quantities at n sources and required in known quantities at m destinations, in such a way as to minimize the total cost of transport.

One commodity which is moved about the country in large quantities and at heavy cost is coal, and the supply of coal is in the hands of a single organization, the National Coal Board. These facts suggested that the possibility of applying the transport model of linear programming to the allocation of coal would repay investigation. The problems encountered in the allocation of coal are, of course, far more complex than those solved by the simple transport model: one would not, for instance, attempt to allocate the total coal supply to the different users as though it were a homogeneous commodity. But it was felt that ways could be found of adapting the transport model and of dealing with a system of interacting programmes in such a way as to take account of some if not all of the complexities of the actual situation.

The first step in the investigation was to test the simple model on the allocation of a type of coal sufficiently limited to be regarded in the first approximation as a homogeneous commodity. This test programme, to be interesting, required that the data should be in such a form that a comparison could be made between the actual solution and the theoretical optimum solution. Owing to the decentralization of the National Coal Board this proved to be a very limiting requirement. However, a survey had been taken (in connection with pricing policy) of the source of every consignment of coal received by all coke ovens during one particular month, and these data were made available for this investigation. For the purpose of carrying out the test programme, it was assumed that each coke oven required exactly the amount of coal which it actually received during the month of the survey, and that each colliery would have available for supplying coke ovens that amount which it actually supplied to coke ovens during the month of the survey. In order to limit the problem, South Wales was excluded from the programme. The result was a transport model containing 154 sources (pits) and 65 destinations (coke ovens). The optimum solution to this showed a saving of 10 per cent. in total ton-mileage over the actual solution. It is estimated that at present costs and volume of coal going to coke ovens (including South Wales) a saving of 10 per cent. in total ton-mileage is approximately equivalent to a saving of £500,000 per annum.

It is perhaps necessary to emphasize that the fact that there is a 10 per cent. difference between the actual allocation cost and the theoretical optimum cost in no way constitutes a criticism of those responsible for carrying out the actual allocation: and this for two reasons. In the first place, the model is based on several simplifying assumptions, which will be discussed in some detail, and this may be responsible for overstating the potential saving. In the second place, even if it is eventually demonstrated that there is a considerable potential saving to be made after taking into account all the complexities of the real world, including the cost of instituting control by linear programming, this would be merely another illustration that linear programming is a useful tool of management. It is because it has been found possible in some applications to handle large numbers of variables more efficiently by linear programming than by the exercise of experienced judgment that the use of the technique is growing.

2. *Simplifying Assumptions*

The simple transport model fails in many respects to correspond to the actual situation. Coking coal is not a homogeneous commodity with certain standardized properties, and it is not, therefore, a matter of indifference to the consumer which colliery supplies him. The properties of coal—size, chemical composition, dirt content, etc.—vary greatly from one pit (and indeed seam) to another, and coke ovens have been designed to use different types, and different combinations of types, of coal. Then again, marketing the coal is to some extent a matter for local arrangement and the data for such a nation-wide planning of the detailed allocation may not normally be available centrally. The amount of coal from each pit which is to go to coke ovens is not, in practice, determined entirely independently of the allocation between different coke ovens. For the purpose of the initial computation, however, coking coal has been assumed to be a homogeneous commodity available and required in known quantities before the allocation for the month is carried out.

In the standard form of the transport model the minimand is normally the total money cost of transport. However, as will be described, a modification of the model was used in this application which necessitated minimizing total rail ton-mileage travelled. In so far as there are special rates charged by British Railways on some routes, or cheaper forms of transport available, this model would not minimize the money cost of transport. However, it seems reasonable to expect that a significant reduction in ton-mileage would have associated with it some substantial reduction in money cost.

It has also been assumed that cost is a linear function of tonnage on every route as non-linearity in the function to be minimized normally prohibits the use of linear programming. In this case non-linearity would mean that the transport cost per ton varied with the volume of tonnage on the route. This would arise if there were a cheaper rate charged for larger quantities or if the rate on some routes were raised for larger quantities in order to prevent congestion on the route.

Although so many simplifying assumptions were made for the application of the transport model to the allocation of coking coal, they are by no means all essential. Some indication will be given of the way in which one would adapt the model if it became apparent that important factors had been neglected. The only actual computation which has been completed is this trial programme. But it is considered that the results are sufficiently promising to merit a detailed study of the solution by those in a position to point to the real constraints which have been violated. Alternatively, it might be more interesting to carry out a fresh computation on planning data rather than on retrospective data. This could utilize much of the work which went into the original computation. It should then be possible to construct a programme or programmes which would yield a workable allocation and to make an accurate estimate of the possible saving.

3. *The Data*

Most of the data for the computation were obtained from the survey, which lists for each coke oven in the country the amount of coal which it received during the month from a list of collieries, the pithead price and the delivered price of the coal. The coal is classified by size but not according to any of the other qualities which affect its usefulness. For the purpose of the computation, the size classification has also been disregarded and coking coal treated as being completely homogeneous. The total amount of coal received during the month at each coke oven has been taken as the quantity required at each destination, and the consignments reclassified under collieries of origin to give the amounts available at each source.

The remaining data required for a standard transport problem are the transport costs between each source and each destination. For the 355 routes used in the actual allocation (or 322 if those where the colliery and the coke oven are adjacent are omitted) the transport cost can be obtained as the difference between the pithead and the delivered price of the coal. But altogether there are 10,010 (65 × 154) potential routes, and some method had to be found of obtaining transport costs. It is understood that where there are no special rates on a route, the rail charge is based on the shortest rail distance between the points. At the present time there is no up-to-date set of point to point distances for the British railway network although such a table is at present being computed on an electronic digital computer from basic data of adjacent point distances. Even these basic adjacent point distances were not available in a tabulated form for this research, although they can be obtained from railway working timetables. It was found however to be easier and not appreciably less accurate to obtain the adjacent point distances by measuring on 2½ in. to the mile Ordnance Survey maps.*

To find the shortest rail distance between 10,010 pairs of points given only the adjacent point distances would have been a prohibitively large job. But it was found possible to adapt the standard form of the transport model to use the adjacent point distances themselves in such a way that the computation simultaneously minimizes transport cost and obtains the shortest rail distance of the routes used in the solution.

It is useful at this point to distinguish between the allocation and the routing of the coal. By "allocation" is meant the decision as to which pit is to supply which coke oven. By "routing" is meant the way in which the coal actually travels from the colliery to the coke oven. The usual form of the transport model is concerned with taking the allocation decisions in such a way as to minimize transport costs when the cost of transport is known for every possible connection between consuming points and producing points. If these costs are based on the shortest route between the two points, then this shortest distance will have to be computed first, for each possible connection.

From the point of view of the authority making the allocation plan, the best plan is presumably that which minimizes the money cost of transport. If the transport authority is following the recommended pricing policy for public monopolies and charging for each route according to their own marginal costs, minimizing money cost will also minimize the actual cost of transport, and there is no conflict between the two. But if the rates charged for transport do not reflect the costs actually incurred by the transport authority then the allocation pattern which minimizes the total amount of money paid over to the transport authority may not make best use of the routes which are actually cheapest to run. In this instance the money costs of transport do not reflect the real costs accurately. Rail charges are based on the shortest rail distances between any pair of points, without regard to whether that distance is the one actually travelled by the route. In fact the shortest rail distance would not usually coincide with the shortest practicable working distance.

There is no reason in principle why charges should not be based on minimum working distance instead of on minimum rail distance. Still closer approximation to marginal cost pricing would be to allow charges on congested routes to rise sufficiently to cut the volume of traffic on them down to a manageable level. This would mean charging according to the distance of the alternative route, by-passing the congested segment, travelled by the "marginal" ton. But although this marginal cost charging by the railways is apparently a possibility for the future, there remains the present question as to whether the allocation of coal ought to be based on money cost to the coal trade or on the costs incurred by the railways for each route. Minimization of the genuine transport cost implies co-operation between the National Coal Board and the transport authorities in the joint planning of the allocation and the routing of the coal. It means that the Coal Board would have to allocate coal in a way which would not minimize the money cost of transport. If it were costless to compute minimum transport cost programmes, it would pay the transport authorities to co-operate with the Coal Board in allocating coal according to the actual transport cost even though the Coal Board were charged a total sum of money based on a minimum money transport cost allocation, computed but not used.

It is inefficient from the national point of view that the allocation should be carried out on the basis of transport costs related to minimum rail distance and then the routing of each individual allocation planned by the railways to minimize its actual cost: the best routing plan can only be made if the allocation of the coal between the different pits and coke ovens is also free to vary. The model used in the computation is such a combined allocation and routing plan, but its basic cost data, the adjacent point-to-point distances, are such that the routing is planned according to the shortest rail distance, instead of the feasible working distances. As the rail charges are based on these distances, the allocation part of the solution of this model is that which will minimize the money cost of transport. The model is easily modified to carry out the actual cost allocation and routing plan, if this should be required either instead of the money cost plan or additionally to it.

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* This may have introduced some unsuspected error into the data, and a re-run of the computation using the British Railways adjacent point distances would be more satisfactory.

4. *Method*

In the original development of the transport problem by Koopmans (1951*b*) the cost data were the sailing times in days between the major ports of the world, and the problem was to minimize the time ships spent in ballast in getting from the ports which had a surplus of empty ships to those which required empty ships. The calculation was apparently carried out by drawing the routes to form a tree graph, and making successive improvements.

Dantzig (1951) developed a method of carrying out basically the same operations in a matrix representation. All the sending ports are listed down the left of the matrix and the receiving ports along the top of the matrix. Thus each cell of the matrix stands for one port-to-port connection, and the same matrix can be used to represent the cost of the routes, the tonnage on each route, the revenue, and the profits. His method starts by arbitrarily selecting a number of routes which form a simple type of tree graph, and making successive improvements from there. This works well on small problems for hand computation and may also be satisfactory for electronic computation on large problems, but is not feasible for a hand computation of the magnitude of the pit-to-coke-ovens problem, even if all the pit-to-coke-oven costs were known initially. Furthermore, in this computation where only the "costs" of transport from a point to its neighbouring points on the railway network were known initially the matrix would have been of the order of 1,000 × 1,000*, not 154 × 65 as given by the number of collieries and coke ovens. The computation could follow the Dantzig method with the modification that the diagonal routes (connecting points in their origin aspects to the same points in their destination aspects) would be constrained to carrying non-positive tonnages instead of the customary non-negativity constraints.†‡ The bulk of the 1,000 × 1,000 cost matrix would consist of M's (where M is defined as sufficiently large to prohibit the use of the route), with only the routes from one point to the next adjacent points of the railway network being allowable (i.e. something like 2,000 routes out of the 1,000,000 in the matrix).

For this problem a method of calculation has been devised which is to some extent a reversion to the earlier graphical method. But instead of using a map, the routing pattern is represented formally by a chart. The railway network can be regarded as a graph, on which the "nodes" are pits, coke ovens and junctions, and the "edges" are the sections of railway line between two nodes. The final routing pattern can be considered as a subgraph of the railway network. It can be demonstrated that there is always a minimum cost solution such that the routing pattern forms a tree (a graph in which every node, A, is connected by a series of edges to every other node, B, and in which there is only one such connection between A and B, i.e. a tree is a connected graph which contains no loops). It can be shown, in other words, that if the graph of the routing pattern contains a loop, then the loop can be eliminated with (in general) a reduction in the total transport cost or (in the special case) no increase in the total transport cost. For, consider such a loop in the routing pattern, and define a "tour" of the loop as moving from any node A in the loop through all the edges and nodes forming the loop back to the point A. There are always two possible directions for a tour of a loop, which we can simply define as Direction 1 and Direction 2. Consider Direction 1.—Every point on the loop is connected by one route to the point preceding it in the tour, and by one route to the point following it in the tour. The preceding route either sends coal in to the point, or it sends coal out of the point. If it sends coal in, the coal is moving *with* the direction of the tour, i.e. in Direction 1, and if it sends coal out, the coal is moving *counter* to the direction of the tour, i.e. in Direction 2. The reverse is true for the route which leads to the following point of the tour, giving us four possible combinations for any point on the loop, as in Diagram 1.

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* The number of points in the relevant part of the railway network is approximately 1,000 (pits, coke ovens, and junctions). Most of the points are both sending and receiving points and have therefore to be listed in both dimensions of the matrix.

† Actually neither a non-negativity nor a non-positivity constraint is necessary on any route in a Dantzig matrix so long as (i) a negative tonnage is interpreted as a reversal of direction of the route when obtaining the "prices" (at the same or different costs for the two directions) and (ii) the value of θ at each change of basis is given by the first route which becomes zero (either from the positive or from the negative direction) as θ increases).

‡ The computation could alternatively follow the method described by Orden (1956).

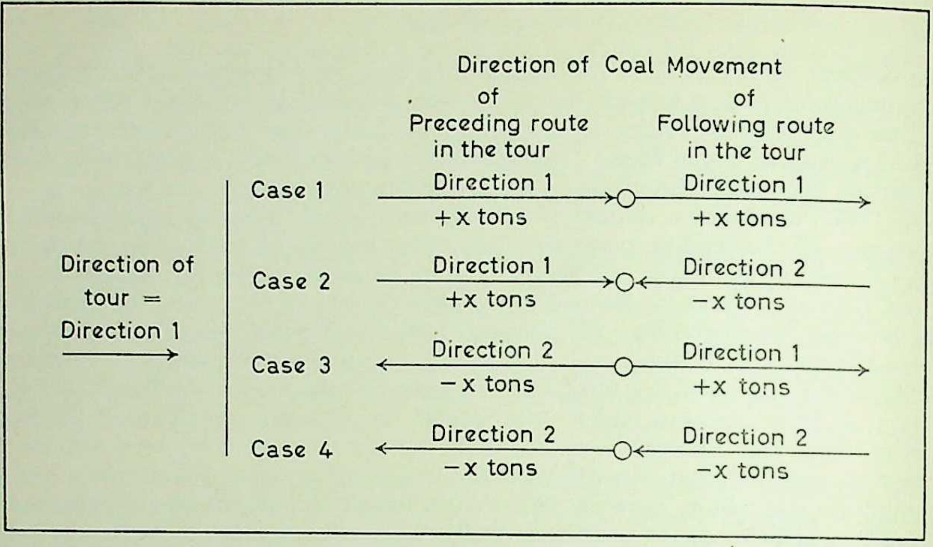

DIAGRAM 1

The loop can be eliminated by adding a certain tonnage, x, to the amount being carried by each Direction 1 route and subtracting x tons from the amount being carried on each Direction 2 route. This will leave the net receipt or despatch of coal at each point unchanged, and if x is the smallest tonnage on a Direction 2 route, say on route $J \rightarrow K$, none of the routes will be changed in direction, and route $J \rightarrow K$ will be removed from the graph, breaking the loop.

The total transport cost will be increased or decreased by the tour of adjustments of x tons according to whether the sum of the costs of the Direction 1 routes is greater or less than the sum of the costs of the Direction 2 routes, and will be unchanged if the two sums are equal. If the Direction 1 tour increases the total transport cost, then a Direction 2 tour to the extent of y tons (limited by the smallest tonnage on a Direction 1 route) will reduce the total transport cost. Thus it is always possible to break the loop by carrying out a tour of adjustments to the tonnages either with a reduction in total transport cost or (in the special case where the sum of the costs of the Direction 1 routes is equal to the sum of the costs of the Direction 2 routes) with no change in the total transport cost. Therefore there is always a minimum transport cost routing pattern which contains no loops.

If the sum of the tonnages available at collieries is exactly equal to the sum of the tonnages required at coke ovens in some subregion of the whole network, there may be no connection between that subregion and the rest of the graph, so that the routing pattern breaks down into two (or more) separate trees (known as a "forest"). It is convenient, however, to connect formally the separate graphs into one tree by the inclusion of routes in the routing graph which carry zero tons of coal.

The Simplex procedure for finding the minimum transport cost pattern, is to start with any routing pattern which forms a tree and to improve successively the pattern by steps known as "changes of basis". A change of basis adds one new route to the graph, so forming a loop, and removes one route (opposite in direction round the loop to the new route) to reform a tree.

Because the routing graph is connected, any point A is connected to any point B by a series of routes. Therefore x tons of coal could be transferred from the point A to the point B without altering the net despatch or receipt of coal from any other point between them by adding x tons to any route carrying coal along the connecting series of routes in the direction from A towards B, and subtracting x tons from any route carrying coal in the direction towards A from B. The net difference which this series of adjustments makes to the total transport cost can be called the "indirect cost of carrying coal from A to B". If A and B are directly connected by one segment of railway line in the total railway network graph, but only connected by an indirect series of routes in the routing graph (the segment $A \rightarrow B$ does not form part of the routing pattern), there is also a direct cost of carrying coal from A to B, namely the cost of the route $A \rightarrow B$. If the direct cost is less than the indirect cost a saving can be made in total transport cost by forming a loop in the graph by bringing into it the route $A \rightarrow B$. The direction $A \rightarrow B$ can be taken to define Direction 1 round the loop, and the saving in transport cost is effected by adding x tons to every Direction 1 route and subtracting x tons from every Direction 2 route, where x is the tonnage on some route $J \rightarrow K$, which is the Direction 2 route bearing the smallest tonnage. Thus the route $J \rightarrow K$ which has x tons subtracted from it is reduced to zero tons, and can therefore be removed from the routing graph.*

The Simplex criterion that the solution has been reached is that there remains no route for which the indirect cost exceeds the direct cost. The indirect cost minus the direct cost is denoted for brevity as the "profit" on the route. Thus the Simplex criterion is that the minimum total transport cost has been obtained if there is no route on which there is a positive profit.†

In practice, it was found most efficient to work with two information stores, called the Chart and the Profits Table. The Chart consists of a formalized representation of all the n points in the system connected by the $n - 1$ segments of railway lines which form the tree graph associated with the current basis. The chart is constructed of paper tape printed as in Diagram 2. The circles contain the code numbers of the points, and the labels on the points contain the tonnage of coal initially available at the points (positive at collieries, negative at coke ovens, and zero at junctions). The lines joining the points represent the routes, and the labels on them record the cost (distance of the segment) and the tonnage. Where the tree branches a cross strip connects the two strips, as in Diagram 3.

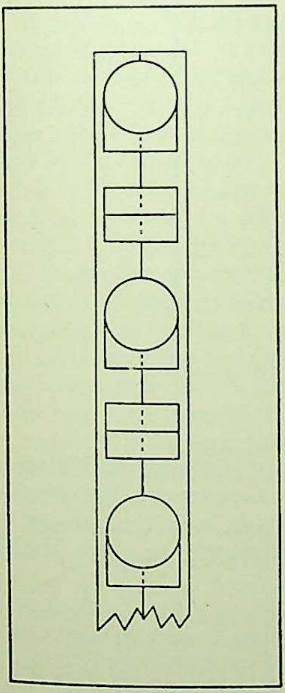

DIAGRAM 2

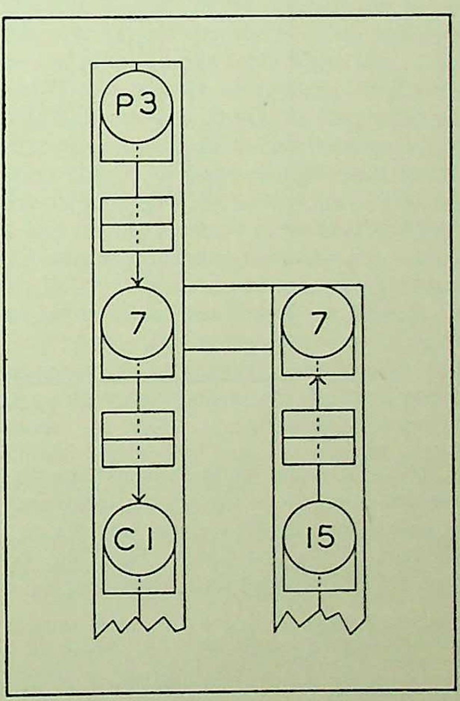

DIAGRAM 3

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* There may be other routes besides $J \rightarrow K$ which were previously carrying x tons and are reduced to carrying zero tons by the tour of adjustments, but only one route must be removed from the graph, the others remaining formally in the graph although they carry zero tons, otherwise some nodes will become disconnected from the graph. In practice it was found most convenient to remove the most costly route from the graph when there was such a choice.

† For routes which form part of the routing graph the indirect cost is simply the direct cost of the route, so that the profit is necessarily zero.

A section of the solution tree appears as in Diagram 4. The whole chart is attached with drawing pins to a board on the wall.

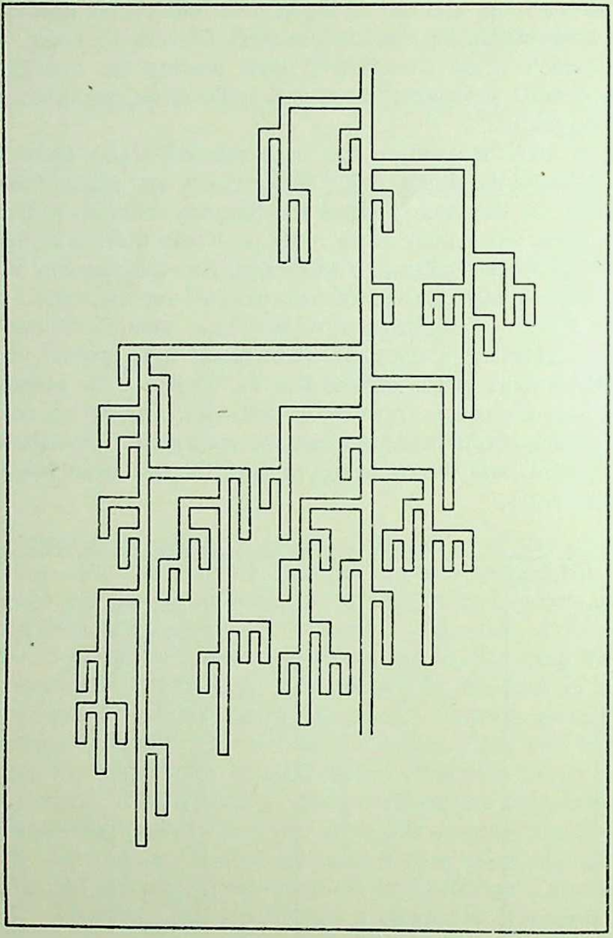

DIAGRAM 4

Any subgraph of the whole railway network which forms a tree of all the points will constitute a feasible first solution of the routing problem.[*] The chart of the first basis is best obtained by working with a map of the railway network so that the most obviously useful sections of railway line form part of the first tree.[†] Next one works systematically through the chart determining the unique set of tonnages and directions[‡] on each route.

[*] This might at first sight seem to conflict with the fact that to obtain a feasible first solution in the Dantzig matrix it is not sufficient that the routes selected form a tree: it is also necessary to ensure that the boundary conditions can be satisfied without there being negative tonnages on any of the routes. But negative tonnages can be interpreted as reversed routes. When the tree is selected from the railway network the directions of the routes are not determined. A segment of line connecting A and B can be regarded as two potential routes, either $A \rightarrow B$ or $B \rightarrow A$. At the next stage the segment will be regarded as $A \rightarrow B$ or $B \rightarrow A$, whichever ensures a positive tonnage. Note that large parts of the routing tree will consist of zero tonnage routes, in which case either direction of the segment could be used. It seems likely that a more efficient computational routine could be devised leaving that choice unsettled, but under the method used in this problem, the direction of zero tonnage routes was simply settled arbitrarily.

[†] If the allocation were being carried out by linear programming as a matter of routine, the first basis would normally be the solution tree for the previous period.

[‡] The directions can be signified by an arrow, or the convention observed that all routes are directed down the chart, and reversed routes denoted by negative tonnages.

The profits table consists of a list of all the routes classified under their sending points.* The profit on each route is computed in two stages. First the "price of coal" at each point is computed by selecting one point arbitrarily as the origin, and working through the chart subtracting the cost of the route when obtaining the price at a sending point from the price at a receiving point, and adding the cost of the route when obtaining the price at a receiving point from the price at a sending point, recording the prices of the points on the profits table. The price of coal at the point A is therefore the indirect cost of carrying coal from the origin point to the point A. It can be readily checked that the indirect cost of a route $A \rightarrow B$ is the price at B minus the price at A. Thus the profit on each route can be obtained by subtracting from the price at the receiving point, the price at the sending point and the cost of the route. The prices and profits need be computed in this way only twice: to obtain the first basis, and at the end of the whole computation as a check that the solution has been reached.

The unit of computation from this point on is the "change of basis" by which one new route which has a positive profit is brought into the chart to form a loop, then the tonnages changed round the loop so that one of the routes going in the opposite direction round the loop is removed. The profits are changed (by adding or subtracting a constant) on all the routes which actually or potentially connect two points, one in each of the two separate trees formed when the one route is removed from the chart and the new route has not yet been inserted.

One question of interest is whether if the shortest distance from every point to every other point were known it would be a better form of basic data for the transport problem than the adjacent point-to-point distances. The answer probably varies with the problem. The size of the tree chart if determined by the number of points used, so that it will be much larger if junctions as well as origins and destinations are included. But the size of the tree is much less important in determining the length of the calculation than the size of the profits table which has to be modified at each change of basis, and this is determined by the number of routes. In this example there were approximately 1,000 points and 2,000 adjacent distance routes, as compared with the 219 points and 10,010 pit-to-coke-oven routes if all the pair-wise distances had been known initially and used in the computation. Furthermore, a given change of basis would cause more profit alterations in a profits matrix than in a profits table of segments of railway line. For instance, consider a basis change for which the prices of one half of the tree have to be altered relatively to the other half. In the matrix form the profits would have to be altered for every connection from an origin in the one half to a destination in the other half. But in the railway network form, only those stretches of line potentially connecting the two halves of the tree have to be altered. Nevertheless the decision cannot be made *a priori*—the size of the railway network may be very much the same even when the number of origins and destinations is very much fewer than in this case, and hence the size of the profits matrix much smaller. In some applications the most appropriate procedure may be a combination of the two methods. It may be reasonable to obtain all the pair-wise costs within each clearly defined geographical region and link the regions together by obtaining the costs from each sending point within a region to all junction points on its boundaries and from the junction points to all receiving points within the region.

The computation is complete when no route has a positive profit. This is the normal linear programming simplex criterion, although the routes referred to in this context are the directed segments of the railway network, rather than the complete pit-to-coke-oven routes. The chart can be regarded as the complete allocation and routing pattern, or the pit-to-coke-oven routes which are admissible in an optimal allocation can be listed from the chart by the rule that any route is admissible if the pit is connected to the coke oven on the chart by a series of segments all directed from the pit towards the coke oven. There were 576 such admissible routes, and any combination of those routes which can be used to allocate the coal from the collieries to the coke ovens* will yield the same total minimum cost.

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* i.e. each segment of railway line appears twice in the profits table, once for each direction. This is not essential where, as in this case, the costs for the two directions of the routes are the same, for then the sum of the profits on the two directions must be equal to minus twice the cost of the route, and the Simplex criterion can be re-interpreted accordingly. But in hand computation it seemed simplest to record the two directions as two separate routes and use the relationship between the two profits as a check on computation.

If linear programming were currently in operation as a method of control, the full list of admissible routes could be issued to the marketing department which could work within it to satisfy as far as possible the particular wishes of the consumers. The drawback to this is that it would probably result in more than the minimum number of different routes being used, and therefore possibly smaller consignments.

5. *Results*

The full tabulation of the comparison of the actual and the optimum solutions is too large to be reproduced. The total actual ton mileage was 31,949,212·18, and the optimum, 28,726,056·38, a potential reduction of 10 per cent.

Every route used or available for use has associated with it a "profit" figure in the solution. The criterion that the solution has been reached was that no unused routes show a positive profit. By definition, those which are used have zero profit. In this calculation the routes are simply the segments of railway lines. But the condition on the profits applies also to the pit-to-coke-oven connections. On most of these routes the mileage is unknown and the (negative) profit is therefore also unknown. But as the mileage had to be computed for the routes used in the actual solution in order to make the comparison with the optimum solution, the profit on them can be computed, and described as the "loss per ton" on the route.

The loss per ton on any route is the excess of the mileage of the route over the price difference between the end points. It can be interpreted as the marginal cost per ton of using that route directly, as opposed to using the (unique) series of indirect adjustments within the solution tree which would also result in transferring one ton from the origin point to the destination point of the route.

The loss per ton multiplied by the tonnage actually sent on a route is a measure of the share of the total excess cost of the actual solution over the optimum solution contributed by each consignment. Where the route is one also used in the optimum solution, the loss per ton is, of course, zero. The sum of the total loss on all routes is equal to the difference between the actual ton-mileage and the optimum.†

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* It is never necessary to use more routes than $n + m - 1$ (where n is the number of origins, m the number of destinations: in this case, 218 routes) in a transport problem. But where two or more routes go through a single point, the solution will be indeterminate between such routes (*cp.* Koopmans, 1951*b*, p. 253). When the connections are restricted to a network, this indeterminacy arises on very many of the routes.

† That it must be so follows from the general theory of linear programming. If the problem is:

$$\text{Minimize } c'x$$

Subject to

$$Ax = b \qquad (1)$$

and

$$x \geqq 0 \qquad (2)$$

to which the minimum solution is x_0, then the dual theorem of linear programming states that there is also a vector y_0 such that:

$$y_0'A \leqq c' \qquad (3)$$

$$y_0'b = c'x_0 \qquad (4)$$

If there is another vector satisfying (1) and (2), say ξ, then

$$A\xi = b \qquad (5)$$

$$\xi \geqq 0 \qquad (6)$$

Define

$$z_0' = y_0'A \qquad (7)$$

Continued on opposite page.

The loss per ton on the different routes varies from a fraction of a mile to over 20 miles. Table 1 shows the effect of allowing routes of the actual solution to be used if the loss per ton does not exceed a certain value.

TABLE 1

Loss Per Ton (Miles)	*Total Loss (Ton-miles)*	*Cumulative Loss (Ton-miles)*	*Cumulative Loss as a Percentage of Difference between Optimum Solution and Actual Solution* (%)
Under 0·25	12,395·02	12,395·02	0·38
0·50	16,167·06	29,562·08	0·91
0·75	9,150·00	38,712·08	1·20
1·00	24,839·00	63,551·08	1·96
2·00	95,291·40	158,842·48	4·93
3·00	262,948·78	421,791·26	13·09
4·00	142,469·44	564,260·70	17·51
5·00	154,806·98	719,067·68	22·31
10·00	709,260·64	1,428,328·32	44·31
20·00	1,097,702·38	2,526,030·70	78·37
Over 20·00	697,125·10	3,223,155·80	100·00

Thus if the routes of the actual solution were permitted for which the loss per ton does not exceed 0·25 ton-miles, and if those routes were used with the same tonnage as in the actual solution, the total cost would rise by 12,395·02 ton-miles, or 0·38 per cent. of the total difference between the optimum solution and the actual solution. If the routes on which the loss per ton does not exceed 0·50 ton-miles were permitted, the total cost would rise by a further 17,167·06 ton-miles, or a total of 0·91 per cent. of the difference between the two solutions. And so on.

Thus the cost of each degree of relaxation in the conditions of the solution can be seen. It might be considered that the extra freedom of manoeuvre by the sales department from issuing a list of permissible routes including not only those on which the loss per ton is zero, but also those on which the loss per ton is less than (say) two miles, might be worth the sacrifice of 5 per cent. of the possible saving from using linear programming. Of course, this implies that the non-optimal routes would not be used more intensively than before the institution of linear programming. It might prove more costly than 5 per cent. of the saving, and the limit might therefore have to be lowered. On the strict solution, 165 of the 355 actual routes are also optimal. The relaxation of the limit to a loss not greater than two miles would leave 215 of the actual routes still usable.

There would, of course, be other routes for which the loss per ton does not exceed two miles, for which no cost information is known in this programme. If the cost is obtained on any route the loss per ton can also be immediately computed, so that the list of permissible routes could be extended as new routes were proposed and examined.

6. *Modifications to Deal with Some of the Most Important Simplifications*

The most serious objection to the application of linear programming to the distribution of coking coal lies in the fact that the coal is not a strictly homogeneous commodity. The computa-

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Then the statement that the sum of the total loss on all routes is equal to the difference between the actual ton-mileage and the optimum is equivalent to:

i.e.
$$(z_0 - c)'\xi = c'x_0 - c'\xi \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

But
$$z_0'\xi = c'x_0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

$$y_0'b = c'x_0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

$$y_0'A\xi = c'x_0 \text{ (from (5))} \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

$$z_0'\xi = c'x_0 \text{ (from (7))} \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

tions of the loss per ton provide an easy way of checking the addition to the total cost which would follow from insisting that route $A \rightarrow B$ must form part of the solution because A's coal is of a special type which is required in the same quantities by the coke oven B as in the actual solution. It is understood, for instance, that Durham coal (also South Wales coal which has not been considered) is particularly suitable for coking, and that in some cases it is required in order to blend it with less satisfactory coal. Durham coal is in fact allocated by the solution to all the other regions, but not to every coke oven in each region. In particular, nearly all the Durham coal going to the Yorkshire and Midlands area is diverted eastwards towards Scunthorpe. However, the general direction of coal movement in that area is southward, and the southward-moving Durham coal comes within a very short distance of the rest of the graph for that area. The loss on that short stretch of line between the Durham coal and the rest of the Yorkshire graph cannot be more than twice the distance of the segment. One could, therefore, take that loss, and the loss on any other segments which would have to be added to the graph to connect Durham to each particular coke oven, as the measure of the additional cost which would be incurred by distinguishing Durham coal as a separate commodity.

A cruder measure was in fact taken of the total loss incurred on all consignments of Durham coal to coke ovens not scheduled to receive Durham coal in the solution. The total loss on these consignments is 294,097·90 ton-miles, or 9·12 per cent. of the total saving from using linear programming. This is really the loss from obtaining coal from the particular pits, rather than just any Durham coal, and therefore it overstates the additional cost which would be incurred if the programme were split into two parts, Durham coal and other coal.

Thus the non-homogeneity of the coal which at first sight seemed to prohibit the use of the transport model turns out to be less serious because the large number of admissible routes which can be used without affecting the total transport cost leaves a considerable degree of flexibility in the final allocation of the coal. If any type of coal (either from a whole region or from only one or two collieries) is not accessible in the solution to a coke oven which must have that type, then the programme must be split into separate programmes, one for each such type. An interesting line of future research is the interaction between the programmes for different types of coal with their varying degrees of substitutability. It seems probable that fairly simple adaptations of the calculation procedure would enable the transport model to be used not only to allocate coal between different users of the same type, but also to assist in the assignment of coal to the appropriate type of users, by taking account of the relationship between the dual solutions for the different types of coal at each place.

The simplification of minimizing ton-mileage instead of money transport cost can also be modified where it fails to correspond to the real situation. If British Railways quotes special rates for some pit-to-coke-oven routes, they could be incorporated in the model as though they were new stretches of railway line, and present no computational difficulty, apart from increasing the size of the computation. Analogous to these special rate routes is the possibility of using road or water transport for some shipments. It would not be enough to know the distance by road or water: some conversion factor reflecting the different costs per ton-mile would also have to be incorporated. But here again the difficulties are not ones of principle. In fact, there is no reason why one should not deal with three networks, rail, road and water, perhaps with a suitable handling charge at the points where goods can move from one network to another. This model would be sufficiently flexible to take the best advantage of seasonal fluctuations in coastwise shipping rates. These other forms of transport have been neglected in the computation in the belief that a reasonable first approximation can be obtained from the consideration of rail traffic only.

Non-linearity of the cost function in the form of reduced charges for large quantities would, strictly speaking, invalidate the use of linear programming. On the other hand, it would probably be found that in fact it was possible to obtain a lower total transport cost by using linear programming, even without allowing for this factor, than by not using it at all. For one thing, the linear programming solution uses the minimum number of routes consistent with fulfilling the programme, and hence gives the highest average tonnage per route. A transport charge which rises with the tonnage or a straightforward quantity restriction on a congested route, could be dealt with by successive modifications of the simple solution.

7. *Further Remarks*

One consideration which is of great importance in an application of linear programming is the cost of the computation itself. The initial computation of a programme this size might take one experienced person two or three months, using only desk equipment. But the regular modification of the programme, assuming that there is some degree of similarity between the amounts available and required from one period to the next, would be a clerical routine taking only about a week or a fortnight. This compares with a possible saving in transport cost of the order of £500,000 per annum, assuming that allowing for the constraints which have been violated by the simple computation would not add greatly to the cost of the programme. However, to make such a comparison we should also have to take into account the cost of the regular flow of information to and from the allocating authority.

The model could presumably also be applied to other forms of coal than coking coal. Making the very crude assumption that the possible saving bears the same relationship to tonnage for all coal as for coking coal (which forms about one-seventh of all coal going to inland consumption), the possible saving might be of the order of £3½ million per annum. There are various possibilities for developments of the model to fit the particular problems of the coal industry, some of which have already been mentioned. One which might prove interesting is that of allocation through time as well as through space. That is to say, a model might be developed which would assist in locating storage points for coal produced in summer for winter use.

8. *Summary*

The transport model of linear programming has been applied to coking coal, available at 154 collieries and required at 65 coke ovens in England and Scotland. The model has been modified to minimize transport cost in terms of rail ton-mileage, using as its basic cost data the distances of segments of railway line between junctions. The simplifying assumptions which were made for the computation are described, and some indication is given of the way in which the model could be modified for practical use. On the assumptions made, it is calculated that a saving of 10 per cent. in the total ton-mileage travelled by coking coal (equivalent to approximately £500,000 per annum) could be obtained by using a linear programming routine in planning the distribution of coking coal.

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Presentation of the results and the conclusions drawn from them are entirely my own responsibility.

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THE MATHEMATICAL RELATION BETWEEN THE NUMBER OF DISEASES AND THE NUMBER OF PATIENTS IN A COMMUNITY

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FOR samples drawn from the morbid population of a country, particular diseases being represented by the numbers of individuals suffering from the disease, we shall suggest among other things:

1. A general parameter measuring heterogeneity of morbidity distributions, which enables us to determine whether samples of widely different size may be regarded as having been drawn from the same population.
2. The mathematical relation between the number of diseases and the number of patients in the population.
3. The influence of diagnostic habits upon the number—and therefore also the type—of diseases diagnosed in a sample.

SECTION I

1.1. Morbidity statistics are still in *statu nascente*, and are sometimes, even by medical statisticians, regarded as of doubtful value. Thus Professor D. Mainland of Yale University adds when speaking about the analysis of hospital records: "if they are worth analysing" (Mainland, 1954). The trouble arises, apart from the influence of diagnostic habits (see Section III), from the fact that such statistics are often derived from samples of very unequal size, which has the consequence that, at a first glance, they hardly seem comparable. In the frequency distributions which are considered here, the conventional statistical constants, such as measures of central tendency and of spread or dispersion, differ for morbidity statistics according to the size of the sample and it is, therefore, in terms of these constants hardly possible to say whether two series of morbidity statistics represent samples from one and the same population. It is this lack of comparability of morbidity statistics which detracts from their usefulness for assessing the state of health in the population.

As material for this investigation, we shall use the report on morbidity in England and Wales for the year 1949 published by the Registrar-General for England and Wales (General Register Office, 1949). Information is taken from hospital in-patients from "several teaching hospitals and a few other hospitals" in England and Wales. There are also some statistics derived from a sample of the war-time E.M.S. (Emergency Medical Service) hospital records. In addition, the Registrar-General refers to a companion volume on general morbidity, cancer and mental health, also published in 1949.

We shall be concerned mainly with the first quoted study, namely, the hospital in-patient enquiry for 1949.

As a first step in the statistical condensation of material, we arrange the morbidity statistics* of Table H 1(*a*) in the Registrar-General's publication quoted above, pp. 20–154, and those of Table H 1(*b*) pp. 156–290 according to the number of times a particular disease was diagnosed in terms of the International Code of Diseases.* The resulting distributions are given in Table 1.

We may regard the International Code of Diseases as the vocabulary used by the physician for diagnosing. We may therefore, think of the 1,000 or so diseases of the international code as so many vocabulary items which "occur" in the diagnoses made by the doctor in the course of time. It is well known that both the amount of vocabulary a writer has at his disposal and the frequency of use of particular words must be considered important characteristics of style. The use of diagnostic terms has much in common with that of other vocabulary. This is only natural if we

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* Except normal deliveries and births.

TABLE 1

Frequency of Diagnosis	*Number of Diseases which were Diagnosed with this Frequency*			
	Teaching Hospitals		*Regional Board Hospitals*	
	Male	*Female*	*Male*	*Female*
1–5	120	137	208	202
6–10	55	63	111	95
11–15	44	46	53	58
16–20	31	30	30	29
21–25	28	36	28	35
26–30	32	35	17	30
31–35	28	31	37	16
36–40	17	19	11	9
41–45	19	17	11	16
46–50	18	15	9	9
51–55	22	11	8	12
56–60	11	12	7	4
61–65	13	14	9	12
66–70	9	6	4	8
71–75	10	7	5	9
76–80	13	9	5	5
81–85	11	12	4	4
86–90	11	5	7	5
91–95	9	5	4	4
96–100	6	9	—	2
101–109	12	10	9	5
110–119	9	10	5	6
120–129	8	12	4	6
130–139	10	14	6	7
140–149	10	16	3	8
150–159	9	7	2	8
160–169	10	3	4	1
170–179	9	9	3	1
180–189	10	3	1	6
190–199	2	7	3	2
200–209	3	9	3	1
210–219	8	5	1	6
220–229	5	8	0	2
230–239	3	4	1	2
240–249	9	3	1	3
250–259	5	5	0	0
260–269	5	5	0	1
270–279	5	1	2	2
280–289	3	7	1	0
290–299	3	5	3	1
300–319	7	8	1	0
320–339	2	5	1	6
340–359	2	7	1	1
360–379	5	3	0	1
380–399	4	4	2	1
400–449	11	11	2	4
450–499	4	6	1	1
500–549	7	5	0	0
550–599	6	1	2	3
600–699	5	12	2	0
700–799	6	4	0	1
800–899	2	4	0	2
900–999	2	2	0	0
1,000–1,449	5	9	4	2
1,500–1,999	1	8	0	1
2,000–2,999	1	3	0	0
3,000–3,999	1	2	0	0
4,000–4,999	2	1	—	—
N	718	747	641	655
M_x	132·2	159·6	45·0	54·7
σ_x	335·8	382·1	123·0	130·4
v_x	2·53	2·39	2·73	2·38

consider that the diagnostic terms represent what is called a professional linguistic code, which is a special kind of the general linguistic code represented by language. However, the distribution of diseases according to the number of patients suffering from it is clearly not only of a linguistic nature. In so far as we are dealing here with the numbers of individuals affected by certain biological changes, the distribution may also be compared with the distribution of animal species in a random sample drawn from a population of animals. The formal similarity between the three types of distribution, morbidity, linguistic and biological, is undeniable: in each case the individual observations are arranged according to groups, such as the different diseases, the different vocabulary items, the different animal species. There are, however, as may be expected, structural differences of quite an essential nature between the merely linguistic distribution on the one hand and the biological distribution on the other. The morbidity distribution, as will be shown, takes a medium position between the two, in accordance with its medium position between the distribution of code symbols and that of biological species. We shall use first its similarity with certain linguistic distributions in order to extend some of the properties of these distributions to morbidity statistics. This will lead to developing a general parameter for this type of distribution, i.e. of groups according to frequency of occurrence, which is essential for the relation to be treated in Section II.

1.2. We are interested in the frequency of occurrence of specific diseases (diagnoses). Suppose we arrange our diagnoses (say N in number) in ascending order according to frequency X; it is then possible to form a frequency distribution of diagnoses according to X, thus

Number of Occurrences X	*Number of Diseases*
1	f_1
2	f_2
3	f_3
4	f_4

The number of diseases that occur X times in the sample from a given population is called the frequency f_x of X (X being the variable), and the manner in which the frequencies are distributed over the scale of X, called the frequency distribution, is epitomized by the statistics:

the arithmetic mean

$$M_x = \Sigma f_x X/N$$

the standard deviation

$$\sigma_x = \sqrt{\{\Sigma f_x X^2/N - M_x^2\}}$$

and the coefficient of variation

$$v_x = \sigma_x/M_x.$$

Now this distribution is of a very special kind. It is based not upon a sample of $N = \Sigma f_x$, but upon a sample of $S_1 = \Sigma f_x X$, and the statistics like M_x, σ_x, v_x all vary with the sample size S_1. It is obvious why this must be so (Yule, 1944). As we go on extending the size of sample taken from a given population, the distribution will itself extend. If in 3,000 patients taken at random the most frequent diagnosis occurs 100 times, say, in 10,000 patients taken at random from the population it will probably occur about 300 times. Apparently everything about the distribution will alter as the sample size is increased: the mean number of occurrences of a disease will go up; any measure of scatter or dispersion will rise rapidly; but the ratio of disease number to the number of occurrences will decrease, since the code of diseases, however great, is necessarily limited, or finite.

1.3. We ask now, which is the factor responsible for the lack of stability of M_x, σ_x, v_x? It is important to realize that there are really two types of distribution involved: the distribution of patients according to specific diagnosis or disease items and the distribution of diseases according to the frequency of occurrence. The conventional parameters describe the latter distribution. In this distribution N represents formally the sample size, and statistics like the arithmetic mean and the standard deviation, and consequently the coefficient of variation, all contain N. But N is in a way a stranger in the basic distribution of patients, since sampling was done according to the occurrences, whose total represents the first moment S_1 of the former type of distribution. This suggests that a statistic which is independent of N might prove constant.

Such a statistic is $v_x/\sqrt{N}$, that is, the coefficient of variation divided by $\sqrt{N}$. To show this, we write v_x^2/N explicitly (Herdan, 1955, 1956) as

$$v_x^2/N = \frac{\sigma_x^2/N}{M_x^2} = \frac{\Sigma f_x X^2/N^2 - M_x^2/N}{M_x^2}$$

$$= \frac{\Sigma f_x X^2/N^2}{[\Sigma f_x X/N]^2} - \frac{1}{N} = \frac{\Sigma f_x X^2}{[\Sigma f_x X]^2} - \frac{1}{N} \quad . \quad . \quad . \quad . \quad (2)$$

Since N cancels out in the last but one term on the right and $1/N$ becomes for great N negligibly small, v_x^2/N can be regarded as sensibly independent of N. Expressing (2) by the 1st and 2nd moment of the variable about zero, $S_1 = \Sigma f_x X$ and $S_2 = \Sigma f_x X^2$, we write the supposedly constant Characteristic as

$$\frac{v_x^2}{N} = \frac{S_2}{S_1^2} - \frac{1}{N} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

and for great N as

$$K^* = \frac{S_2}{S_1^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3a)$$

which, but for the factor 10^4, which Yule used so as to avoid very small decimals, is Yule's Characteristic $K = 10^4 \, (S_2/S_1^2 - 1/S_1)$ for great samples when $1/S_1$ becomes negligibly small (Yule, 1944).

$\sqrt{K^*}$ when written in the form

$$\frac{\sigma_x/\sqrt{N}}{M_x}$$

is seen to represent the coefficient of variation of a mean, or the relative fluctuation of a mean as distinct from that of a single value σ_x/M_x. We shall distinguish it from the latter by the symbol v_m and understand by it the coefficient of variation of the sampling distribution of means.

Another explanation of the constancy of K without using the stochastic models of Yule and of Simon (1955) has been given by Good (1953). That paper suggests methods of estimating, among other things, various general population parameters measuring heterogeneity. One such parameter for which Good uses the symbol $\hat{c}_{2,\,0}$ and which is essentially Yule's Characteristic K, is an estimate of the probability that two words selected at random from the text under consideration will turn out to be the same word of the language. Hence it tends to be larger the more repetitive is the author's vocabulary, or more roughly the smaller is the author's vocabulary.

If the population probabilities of the distinct words are $p_1, p_2 \; . \; . \; .$ then

$$\hat{c}_{2,\,0} = \frac{1}{S_1(S_1 - 1)} \sum_x X(X-1) f_x = \frac{\Sigma X^2 f_x - \Sigma X f_x}{S_1(S_1 - 1)} \quad . \quad . \quad . \quad (4)$$

represents an unbiased estimate of $p_1^2 + p_2^2 + \; . \; . \; .$

Yule's Characteristic K may be shown to equal $10^4 \, \hat{c}_{2,\,0} \left(1 - \frac{1}{S_1}\right)$:

$$K = 10^4 \, \hat{c}_{2,\,0} \left(1 - \frac{1}{S_1}\right) = \frac{S_2 - S_1}{S_1(S_1 - 1)} \cdot \frac{S_1 - 1}{S_1} = \frac{S_2}{S_1^2} - \frac{1}{S_1}, \quad . \quad . \quad (5)$$

and thus tends to $10{,}000 \, (p_1^2 + p_2^2 + \; . \; . \; .)$ as $S_1 \to \infty$. This is the explanation for the "constancy" of K and thus also of $\frac{v_x}{\sqrt{N}}$.

We shall use in this paper v_m in addition to K, since it is interpretable in terms of such elementary statistics as the arithmetic mean, the standard deviation and the coefficient of variation.

1.4. Turning now to the data of Table 1, the distributions would seem to indicate quite a striking difference. The number of diagnoses made only once or twice seems much greater in the Regional Board hospitals. It thus looks as if rare diseases would come more often under observation in these hospitals. This impression, however, is obviously misleading and due to the difference in the size of sample from which the two distributions were obtained. A small sample implying a short time of observation, may show a greater number of diagnoses occurring only once. As the time of observation is increased, and the sample grows, such diagnoses will be conceivably repeated, and this leads with time to a relative reduction in the number of the diagnoses which were made once or twice only and to an increase of the number of diagnoses which were made more often.

We now apply the development of Sections 1.2, 1.3 to morbidity statistics, more particularly to the data of Table 1. Table 2 lists the relevant quantities.

TABLE 2

Teaching Hospitals

	Diagnoses	*Diseases*	S_1/N	σ_x	v_x	v_m	$v_m^{\ 2}$	K^*	$1/N$
Males . .	94,936	718	132·2	335·6	2·53	·095	·0090	·0104	·0014
Females .	119,228	747	159·6	382·1	2·39	·088	·0077	·0090	·0013

Regional Hospital Group

	Diagnoses	*Diseases*	S_1/N	σ_x	v_x	v_m	$v_m^{\ 2}$	K^*	$1/N$
Males . .	28,900	641	45·0	123·0	2·73	·108	·0116	·0131	·0015
Females .	35,889	655	54·7	130·4	2·38	·093	·0087	·0102	·0015

We note that whereas neither M_x, σ_x or v_x can be used as a characteristic since they vary with the number of diagnoses, and with the number of diseases, rising as these numbers increase, $v_m^{\ 2}$ remains sensibly constant as predicted and equal to K^*-1/N.

Such differences as there are between the values of v_m are evidently no longer dependent upon sample size, and that is why differences in v_m are useful for the interpretation of real—as against statistical—differences of diagnostic behaviour. Comparing the values of v_m in Table 6 we find that there is only a slight but consistent difference between the values for males and females within a given group of hospitals, the value for females being always somewhat smaller than that for males. On the other hand, there appears also a small difference between hospitals: for the Teaching Hospital both values of v_m are somewhat smaller than those for the Regional Hospital group, the difference being roughly of the magnitude of ·01. Since the differences in v_m are no longer dependent upon the sample size, we may interpret them as indicating differences in diagnostic behaviour and in the given case we may say that the number of diseases which were diagnosed with the same or a similar frequency is greater for females in each of the two types of hospital, and regardless of sex, greater in the Teaching Hospitals. No statistical significance test is applied and it may well be that the differences are too small to be significant (see section II, Discussion). This information about morbidity statistics could not have been obtained by any of the conventional methods of vital statistics and shows the value of v_m as a statistical constant.

SECTION II

We emphasize the importance of understanding that the morbidity distribution may be one of individual patients (1) or of diseases (2), that is of groups of patients suffering from the same disease. In Section I we regarded the distribution of diseases according to the number of individual diagnoses as the basic distribution. In this section we have to regard the distribution of individuals as our basic one. In fact, the randomization of the sample of morbidity statistics is, as a rule, by individual patients, and not by diseases. In general, samples taken from a population by those two different methods of randomization require different mathematical treatment (Williams, 1946). It should be remembered, however, that even in Section I, a parameter suitable for the description of morbidity statistics was obtained only by transforming the parameters used for the distribution (2) by eliminating the quantity N, and this was interpreted as a reversal to the original distribution of individuals.

So far, no assumption was made about the law which governs the distribution. In this respect, the similarity in form between the distribution of diseases according to the number of patients with that of animal species according to the number of individuals, suggests the logarithmic series as a possible distribution law for frequency of different diagnoses, since it was found to fit satisfactorily the distribution of species according to the number of individuals.

Writing ln for $\log_e$, the logarithmic series yields, for $|x| < 1$,

$$-(1/x)\ln(1-x) = 1 + x/2 + x^2/3 + x^3/4 + \ldots \qquad (6)$$

We now denote the frequencies of diseases being diagnosed once, twice, etc. by $n_1, n_2, \ldots$ Since the first term in the series is n_1, the number of diseases with single occurrences in the population, the logarithmic series applied to morbidity statistics becomes

$$n_1 + \frac{n_1 x}{2} + \frac{n_1 x^2}{3} + \frac{n_1 x^3}{4} + \ldots \qquad (7)$$

The sum of all terms to infinity, which is the total number of diseases, is given by

$$N = \frac{n_1}{x}\{-\ln(1-x)\} \qquad (8)$$

which is finite for $x < 1$; that is to say the series is convergent.

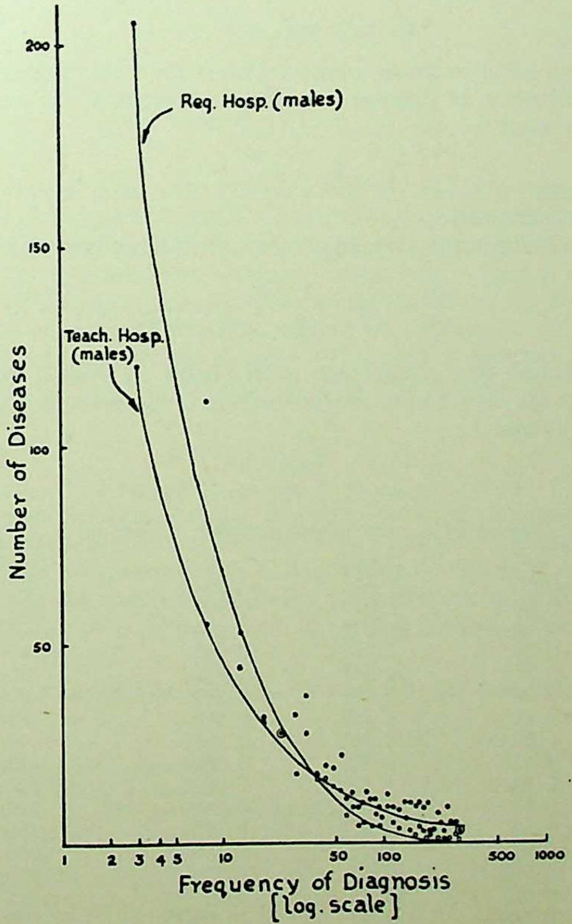

Fig. 1.

The $r + 1$st term is obtained from the r^{th} by multiplying by $rx/(r + 1)$; the second term is thus less than $\frac{1}{2}\, n_1$, the third term less than $\frac{1}{3}\, n_1$, and so on. In general

$$n_{r+1} = n_r \left(\frac{r\,x}{r + 1}\right)$$

$$(r + 1)\, n_{r+1} = rn_r + x \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

which, incidentally, enables one to verify Dr. Turing's smoothing formula as used by Good (1953) for this particular case.

Since as n approaches infinity, x approaches unity, the formula goes over into Turing's formula

$$r^* \simeq (r + 1)\, n_{r+1}/n_r$$

$$r^*\, n_r \simeq (r + 1)\, n_{r+1} \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

Fig. 1 shows the distribution of Table 1 as a graph similar to a hyperbola. The nearer x approaches unity, the closer the resemblance; when $x = 1$, the graph becomes a hyperbola.

The successive terms of the series are the number of groups (diseases) containing 1, 2, 3 . . . units (patients); it follows that the numbers of units in each successive term are,

$$n_1,\ 2\,\frac{n_1\,x}{2},\ 3\,\frac{n_1\,x^2}{3},\ 4\,\frac{n_1\,x^3}{4}\ \text{etc.}$$

or

$$n_1,\ n_1x,\ n_1x^2,\ n_1x^3\ \text{etc.} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

This is a geometric series and its sum to infinity which is the total number of units in the sample or, in terms of the distribution of diseases according to frequency, the total number of cases, is the first moment about zero:

$$S_1 = \frac{n_1}{1 - x} \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

This is finite if x is less than unity.

From (8) and (12) we obtain the average number of units per group as

$$\frac{S_1}{N} = \frac{x}{(1 - x)(-\log_e \overline{1 - x})} \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

The extraordinary length of the morbidity series (Table 1), from $X = 1$ to $X = 4{,}999$, made it necessary to condense the distribution progressively as X increases, and to work with a grouped distribution to obtain S_1 and S_2.

TABLE 3

(from C. B. Williams, 1947)

Values of x with Corresponding Values of S_1/N

x	S_1/N	x	S_1/N
0·50	1·443	0·992	25·68
0·60	1·637	0·993	28·58
0·70	1·938	0·994	32·38
0·80	2·483	0·995	37·48
0·85	2·987	0·996	45·11
0·90	3·909	0·997	57·21
0·91	4·198	0·998	80·33
0·92	4·551	0·9990	144·6
0·93	4·995	0·9992	175·1
0·94	5·567	0·9994	224·5
0·95	6·340	0·9996	319·4
0·96	7·458	0·9998	586·9
0·97	9·214	0·99990	1,086·0
0·980	12·53	0·99995	2,020
0·985	15·63	0·999990	8,696
0·990	21·47	0·999995	16,390
0·991	23·38	0·9999990	71,430

Thus, for any average number of units per group there is only one possible value of x. When this has been calculated, n_1 can be obtained by multiplying S_1 by $(1 - x)$. It follows that if the distribution of groups according to the number of units has the structure of the logarithmic series, and if the average number of units per group is known, it is possible to calculate x and n_1 and with it the whole series. This means that for any given number of units and of groups only one logarithmic series is possible. For the following development we shall need, apart from the first moment about zero, S_1, the second moment about zero, $S_2 = \Sigma f_x x^2$ which for the logarithmic series equals

$$\frac{N}{1-x} = \frac{n_1}{(1-x)^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

The ratio of the two quantities of the distribution n_1 and x is a constant which Williams has called α, that is

$$\frac{n_1}{x} = \alpha \text{ or } n_1 = \alpha x \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (15)$$

The logarithmic series can therefore be written as

$$\alpha\, x,\ \alpha \frac{x^2}{2},\ \alpha \frac{x^3}{3},\ \alpha \frac{x^4}{4} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (16)$$

In this case the total number of groups is

$$N = -\,\alpha \log_e (1 - x) \quad . \quad . \quad . \quad . \quad . \quad (17)$$

and the total number of units

$$S_1 = \alpha \frac{x}{1-x} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (18)$$

which formulae, are, of course, structurally identical with (8) and (12), respectively.

Both n_1 and x vary with sample size, but α is a constant for all samples from the same population taken by the same method of sampling. It is therefore a property of the population and was called by Williams the Index of Diversity (Williams 1946, 1947), since it is high in populations having a large number of groups relative to the number of individuals and low in populations having a small number of groups relative to the number of individuals. It may be regarded as a measure of the extent of the classification of units. The relation between the number of groups N and the number S_1 and the Index of Diversity is given by the formula

$$N = \alpha \log_e \left(1 + \frac{S_1}{\alpha}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (19)$$

Thus, if α is known for the population from any one sample it is only necessary to insert in formula (19) the new S_1 for a second sample of a different number of units to find the new N. It may also be read off from Fig. 5 of Williams, (1947).* The Index of Diversity α has been shown by Williams to be nothing but the reciprocal of Yule's Characteristic or rather to be reciprocal to $K/10^4$.† Yule's Characteristic $K = 10^4\,(S_2 - S_1)/S_1^2$ where S_1 and S_2 are the first and second

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* Attention is called to certain differences in notation as used in this paper and in the paper by Fisher *et al.* (1943):

	Fisher et al.	*This Paper*
Total number of units (diagnoses) . . .	N	S_1
Total number of groups (diseases) . . .	S	N
Average number of units per group (average incidence of disease)	N/S	S_1/N

† It may be noted that for the logarithmic distribution

$$v_x^2/N = 1/\alpha x - 1/N,$$

so that v_x^2/N is not invariant for small samples, as it involves x.

moments about zero. Since for the logarithmic series

$$S_1 = \frac{n_1}{1-x} \text{ and } S_2 = \frac{n_1}{(1-x)^2} \quad . \quad . \quad . \quad . \quad (12), (14)$$

it follows that

$$\frac{S_2 - S_1}{S_1^2} = \frac{x}{(1-x)\,S_1} = \frac{x}{n_1} = \frac{1}{\alpha} \quad . \quad . \quad . \quad . \quad . \quad (20)$$

Disregarding the coefficient 10^4 which Yule used only for avoiding very small quantities for the Characteristic and putting, therefore, $K' = \frac{1}{\alpha}$, formula (19) about the relation of N and S_1 and α could be put in a more convenient form:

$$N = 1/K' \log_e (1 + S_1 K') \quad . \quad . \quad . \quad . \quad . \quad (19a)$$

Since $S_1 K'$ is a big number, adding unity will make little difference to the relation and we write therefore

$$N K' = \log_e (S_1 K') \quad . \quad . \quad . \quad . \quad . \quad . \quad (21)$$

or, if we prefer not to neglect unity, we arrive at the inequality

$$N K' < \log_e (S_1 K') \quad . \quad . \quad . \quad . \quad . \quad . \quad (21a)$$

In words: the product: Characteristic × number of groups is sensibly equal to the natural logarithm of the product: Characteristic × first moment. We now apply these relations to the data of Table 1 and list the results in the following table:

TABLE 4

Teaching Hospital Group

	Diagnoses	*Diseases*	S_1/N	x	α	$K'=1/\alpha$	N *Estimated from Fig.* 5 (Williams, 1947)	$\text{Log}_e\, S_1K'$	$N K'$	N *Estimated by equation* (21)
Males . .	94,936	718	132·2	·9988	105·4	·0095	710	6·804	6·812	717
Females . .	119,228	747	159·6	·9991	106·4	·0094	730	7·022	7·017	747

Regional Hospital Group

	Diagnoses	*Diseases*	S_1/N	x	α	$K'=1/\alpha$	N *Estimated from Fig.* 5 (Williams, 1947)	$\text{Log}_e\, S_1K'$	$N K'$	N *Estimated by equation* (21)
Males . .	28,900	641	45·0	·996	116·4	·0086	630	5·514	5·506	642
Females . .	35,889	655	54·7	·997	114·0	·0088	650	5·752	5·744	656

DISCUSSION

We first compare the values of K^* obtained from the observed distributions and listed in Table 2 with the values of K' calculated under the assumption of a logarithmic series.

The values of $K' = 1/\alpha$ are all slightly smaller than those of K^* calculated from the observed data, but on the whole the agreement is satisfactory, and supports the assumption of the logarithmic series. The differences of K' between hospitals and between males and females are slightly smaller than the corresponding ones of K^*. For Teaching Hospitals the difference between K' for males and females is in the same direction as the difference in K^*; for the Regional Hospital Group the direction is reversed. Taking, however, into account that $K^* = K' + 1/S_1$, and that $1/S_1$ is smaller for females than for males, the difference between K^* when calculated from K' of Table 4 narrows down, and thus changes slightly in the direction of that of K^* of Table 2. The remaining difference between the expected values of K^* is so small as to be undoubtedly non-significant. We next compare the number of diseases calculated under the assumption of a logarithmic series with the actually observed numbers in the four distributions. Here again, there is good agreement between theory and observation. The same applies to the number of diseases calculated from the relation (21) between the logarithm of the product $S_1\, K'$ and $N\, K'$. All this speaks in favour of the assumption that the frequency distribution of diseases according to the number of occurrence is satisfactorily fitted by the logarithmic series.

No statistical significance test is applied, because the nature of the observations–diagnoses of diseases– is not of the sort for which it would seem sensible trying to establish the logarithmic series as a functional relationship between the numbers of diseases and the frequency of their diagnosis. The mere fact that a diagnosis may have to be changed, or that it may be a case of multiple diagnosis, must deter from trying to establish functional relationships of the type of the logarithmic series. All that is claimed is that the distribution is near enough to the logarithmic series for making it profitable to use the latter for arriving at certain conclusions which are of medical interest. One such conclusion refers to the number of diseases in a given morbid population or in the sample from such a population, and if the estimate of that number on the basis of the logarithmic series comes fairly close to the actual number of diseases, this would justify the use of the logarithmic series for our purpose.

SECTION III

The relation between the number of diseases and the number of patients in the population established under II enables us to take diagnostic habits into account as an important factor of morbidity statistics.

That the development of medical science will have changes in diagnosis in its wake goes without saying. Given, however, a certain development of medical science, say medical science of to-day, it is little realized how habits of diagnosing may affect what we call morbidity statistics. Such statistics are first of all statistics not of diseases, but of diagnoses, and it is therefore essential to be clear about the stability of diagnoses for a given syndrome. To make the matter quite clear: for a certain syndrome, the name given to it, that is the diagnosis, will differ, or may do so, if the syndrome is of a somewhat unusual type, from one general practitioner to another, and still more between general practitioners and hospitals, or between hospitals according to the facilities which are available for diagnostic purposes. This question of the variation of diagnoses has nothing to do with the ultimate correctness of the diagnosis. There is no way of ascertaining by any statistical procedure whether a given diagnosis is correct, that is, whether it corresponds to reality. The question which interests us here is of a more formal type, namely it is concerned with *the preference for either uniformity or diversity of diagnosis*, which attitude of mind may conceivably differ according to the clinician, and this aspect, as has been shown, is in some measure susceptible to statistical treatment. What is more, such statistical treatment enables us to show variation in diagnosis to be a factor in morbidity statistics.

The amount of professional vocabulary at the disposal of a doctor and his preference for certain types of diagnosis will determine the relation between the number of diseases he diagnoses and the number of patients. Conversely that relation as established from a sample when compared with what might be considered the standard of the ratio $\dfrac{\text{number of patients}}{\text{number of diseases}}$ in the population may afford a clue to the extent of professional vocabulary at the disposal of the doctor. For example, taking as the standard for diversity in diagnosis for females the expected value $K' = \cdot 009$ (see Table 4), any observed S_1 obtained by the same sampling method as used for the standard, multiplied by K' should give according to formula (21), $\log_e S_1\, K' = N\, K'$, an estimate of N, the number of different diseases in the sample. If N as observed is very different from the expected N, this is an indication that the doctor or doctors responsible for the diagnosis in the sample have used the International Code of Diseases differently from how it ought to be used or, at least, from how it is used in the hospitals providing the standard.

There are, of course, certain restrictions for making comparisons of this kind. One of the conditions for making the comparison a sensible procedure is that the doctor should use the nomenclature of the International Code of Diseases when making his diagnosis. The second condition is that there should be no selection, either with regard to the part of the population which comes under his observation or with regard to the type of diseases he is required to deal with. Given these conditions, however, the sample of diagnoses made by the doctor should yield sensibly the same Index of Diversity α or Characteristic K as the total morbid population, and the relation between the number of diseases and the number of patients should be that demanded by formulae (19), (21). If this is not the case, and if the number of diseases remains very much behind what might be expected on the basis of the standard, this would throw an interesting light on the amount of professional vocabulary actually at the particular doctor's disposal.

The Characteristic v_m thus becomes an index of what may well be called "diagnostic habits" if we understand by "habits" not only idiosyncracies but also the facilities for diagnosis which are available in a given case. It epitomizes the relation between the diseases of the International Code and the frequency of their diagnoses This makes it at once appear to be an interesting constant from the medical point of view. A comparatively small v_m implies that very many diseases are diagnosed with the same or very similar frequencies and that the number of diseases whose frequency of diagnosis differs greatly from the mean frequency is small; conversely, a high v_m implies that not too many diseases will be diagnosed with the same (mean) frequency and that the number of diseases whose frequency of diagnosis differs greatly from the mean frequency is great.

The writer is not so optimistic as to think that these views will be accepted without criticism, but considering that morbidity statistics have so far not got beyond the state of being raw, and sometimes very raw, material, it is even something in this branch of statistics to offer new views that can be criticized. Whatever the criticism, the point should not be missed that the method developed above enables us at least to *condense the information* provided by morbidity statistics in form of a *statistical constant*, and so provide a basis for comparison; further that it enables us to express in a sensible way the relation between the International Code of Diseases and the number of diagnoses, and in this way *characterizes the diagnostic habits*, as regards preference for uniformity or diversity in diagnosis.

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SENTENCE-LENGTH DISTRIBUTIONS OF GREEK AUTHORS

By WILLIAM C. WAKE

1. *Introduction*

THE suggestion of G. Udny Yule (1939) that sentence-length distributions could be used as a criterion of style and therefore of authorship has received but little attention. One of the reasons for this is the need to collect a large amount of data from works of known authorship and hitherto the only published figures are those of Yule himself, C. B. Williams (1939) and the writer, Wake (1948). A second reason may be Yule's mistaken belief that the variability observed in the known works of a single author was greater than the standard errors of the statistics he employed (*vide infra*).

Apart from the human interest occasioned by mysteries surrounding unknown works or mistaken attribution of authorship, historical research and literary criticism are sometimes handicapped by incomplete knowledge concerning certain works. Standard literary methods for dealing with these problems are mainly based on peculiarities of style and suffer from the disadvantage of depending largely, if not entirely, on subjective judgment. There is a need for a more objective "measurement" of some stylistic factor, a need appreciated but not realized by Lutoslawsky (1897). Nowhere is this need more apparent than when handling problems of classical authorship. Plato's *Seventh Letter*, the three ethical works ascribed to Aristotle, and the whole problem of the Corpus Hippocraticum illustrate the importance of this need in a study of philosophy and in the history of science. The present paper is a contribution to the task of establishing the study of sentence-length distributions of Greek classics on a firm foundation so that they may be used by scholars where appropriate, as part of their standard equipment for examining works of unknown or disputed authorship. The problems that arise and are dealt with in this paper concern the nature of sentence-length distributions, the definition of sentence length, the problem of punctuation peculiar to Greek texts, sampling, and the constancy of the sentence-length distribution of one author over a period of time.

2. *Nature of Sentence-Length Distributions*

If we leave for a moment the problems of defining, of punctuation, and of sampling, and, anticipating later sections of this paper, consider that acceptable solutions have been found, we may examine some typical distributions as, for example, Table 1 where two books of the *Hippocratic Corpus* are given both as linear and logarithmic distributions. The linear distributions, following Yule (1939) are grouped in fives and all sentences are reckoned as having the length of the mean of the group in which they fall. Values of statistics within group limits are obtained when required by linear interpolation. The logarithmic distributions are grouped with intervals of $0 \cdot 10$ and sentences ten words long falling on the dividing line between the groups $0 \cdot 90$–$1 \cdot 00$ and $1 \cdot 00$–$1 \cdot 10$ (means $0 \cdot 95$ and $1 \cdot 05$) are apportioned equally between the two groups.

It will be observed that the linear distributions are very skew and, since the variances are greater than the mean, a feature always apparent, the series of groups cannot be any form of simple binomial. Transference of the data to a logarithmic scale follows the suggestion of Williams (1939) that the distributions are log-normal (Gaddum, 1945). This transformation brings the distributions more nearly symmetrical and normal error curves can be fitted, as for the two examples;

$$y = 55 \cdot 12 \exp - 7 \cdot 598 \, (x - 1 \cdot 15)^2$$

and

$$y = 48 \cdot 55 \exp - 9 \cdot 046 \, (x - 1 \cdot 179)^2$$

TABLE 1

Sentence-Length Distributions from Hippocratic Corpus

(a) *Linear*

Sentence-Length (Words)	*Frequency* Regimen in Acute Diseases	*Frequency* Prognostic
1–5	23	9
6–10	84	63
11–15	91	79
16–20	59	54
21–25	45	31
26–30	22	20
31–35	9	9
36–40	7	8
41–45	6	5
46–50	3	2
51–55	2	2
56–60	—	2
61–65	1	—
66–70	—	—
71–75	1	1
76–80	—	—
81–85	1	1
86–90	—	—
91–95	1	—
Totals	355	286

(b) *Logarithmic*

Sentence-Length (Words)	*Mean Log Length*	*Frequency* Regimen in Acute Diseases	*Frequency* Prognostic
2	·35	1	—
3	·45	4	2
4	·55	6	2
5	·65	12	5
6	·75	15	12
7	·85	17	13
8–10	·95	41	31·5
10–12	1·05	50	39·5
13–15	1·15	52	46
16–19	1·25	48	48
20–25	1·35	56	37
26–31	1·45	24	22
32–39	1·55	12	14
40–50	1·65	11	8
51–63	1·75	2	4
64–79	1·85	2	1
80–100	1·95	2	1
Totals		355	286

The histograms, Fig. 1, show the observed frequencies (*full lines*) and the frequencies expected (*broken lines*) on the assumption that the distributions are Gaussian. The distribution for *Regimen in Acute Diseases* does not depart significantly from normality, but the other has an excess in the central values making the curve too tall and falling away to the tail values too quickly. The k-statistics are, $k_1 = 1\cdot179$, $k_2 = 0\cdot0649$, $k_3 = -0\cdot0006465$, $k_4 = 0\cdot00077$, $g_1 = -0\cdot0137$, which is negligible compared with its standard error of $0\cdot142$, and $g_2 = 3\cdot23$ which is significant (S.E. $0\cdot287$). In fact, the log-normal distribution is an approximation only and examples could have been chosen showing a greater departure from normality.

Some distributions are bimodal and this suggests the summation of two or more Poisson series, and one such distribution was fitted by the method of Schilling (1947) but the fit was not good and the computational labour great. In view of the apparently complex nature of the distributions (being presumably the sum of at least three Poisson series) it is obviously difficult to characterize them in any fundamental way and recourse must be had to some less efficient but easily calculated statistic. By approximating to a log-normal distribution the errors involved are small but there are objections to this procedure. It is recorded by Yule and is certainly true for some authors examined by the present writer, that the extreme upper end of the sentence-length distribution can be noticeably characteristic. Thus, in general, samples of 100 or 200 sentences from Aristotle contain in the upper groups of sentence length few sentences, the series coming to an abrupt end. With Plato however, the distributions tail away far more gradually and there is usually a whole series of groups containing one, or perhaps two, sentences. In addition, most samples of 200 sentences from Plato will contain one sentence one-quarter or even one-half as long again as the next longest, a characteristic never found with Aristotle. If these distributions are transformed to a logarithmic scale we find that those derived from Aristotle have their upper tails missing whereas for Plato they approximate more nearly to a normal curve in the tail region. The principal statistic usually calculated for normal curves of error, namely the mean, does not reflect with any degree of sensitivity the changes in the tails of the distribution. In addition, simple though the calculations are to the statistician, the method of handling skew linear distributions by quantiles is simpler, particularly for classical scholars who may wish to follow if not to repeat on other data the necessary computations.

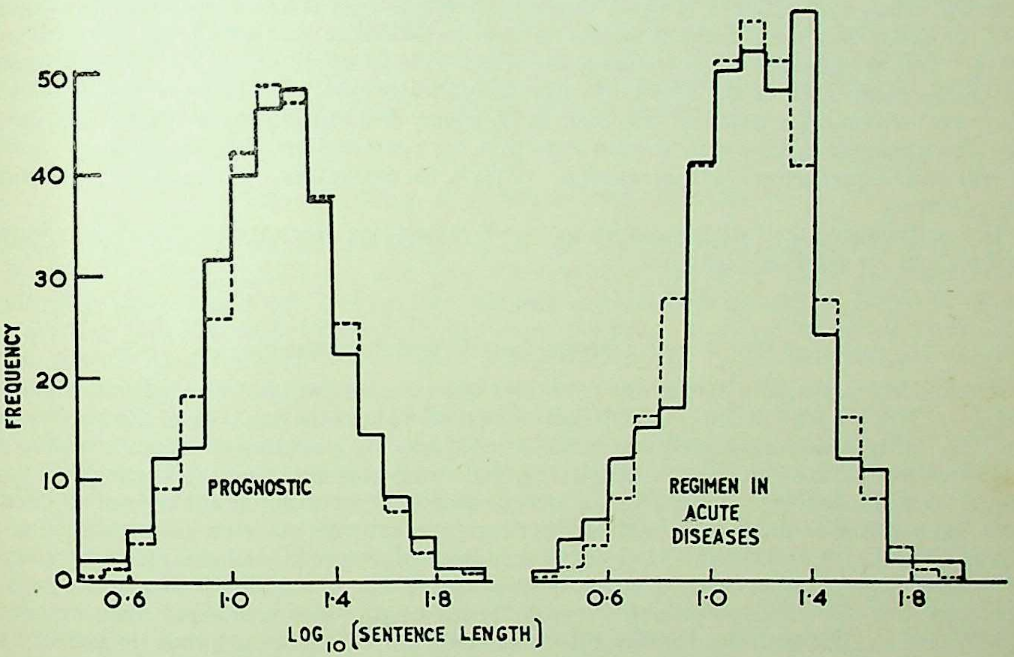

FIG. 1.—Log-normal distribution for Hippocrates.

Quantiles, or percentiles, are a special case of frequency constant more used in early work than now, and the probable errors of such constants were discussed by K. Pearson (1920) and are given by M. G. Kendall (1948). Their calculation requires a knowledge of the mean ordinate at the position of the quantiles (i.e., *not* the actual ordinate given in a single sample distribution) and this can only be obtained from a number of samples or, more conveniently, by setting up a model distribution in counters or random numbers and drawing samples from it.

We have here unfortunately to record a slip in the pioneer paper (Yule, 1939) on the subject. Although Yule states, "Given similar material and mode of treatment, an author's frequency distribution does remain constant within fairly narrow limits" he seems to have miscalculated the errors of his constants for he also states, "The differences between the upper quartiles and the ninth deciles of the two samples from Coleridge for example, are 10 or 11 times the standard errors . . ." The standard error of the third quartile is $\sqrt{3N}/4y$ where N is the number of sentences and y the mean ordinate corresponding to the third quartile. If the frequency at the position of the quartile is taken as a reasonable estimate of y it is, for his data from Coleridge, 14·5/5, the divisor being the group interval, and the standard error comes to about 2·0. The difference which Yule states is 10 or 11 times the standard error is 3·3. Similarly, the standard error for Coleridge's ninth decile is about 2·5 and the difference between the two samples given is 4·2. This mistake proved unfortunate for it led Yule to conclude. . . . "that differences between samples taken as we have taken them may greatly exceed the limits of *simple sampling* [his italics] without, for practical purposes, being of any real significance". This is not so and Yule's own samples and the present work show that, for many authors, *differences between samples are only those expected from random sampling*. That is, for one author, differences are not statistically significant.

Before discussing the variation shown by Greek writers it is necessary to discuss the nature of the data and the mode of collection.

3. *Greek Sentence Lengths and Punctuation*

The modern editor of a Greek text relies on three punctuation marks to indicate the end of periods. They are the full stop (.), the Greek colon placed above the line (·), and the interrogation mark (;). It is usually contended that punctuation marks are a relatively modern invention and even if modern editors agree as to their placing, there is no guarantee that this corresponds to the author's actual intentions. It must immediately be conceded that no autograph copy of any literary MSS. has survived to the present time and that copies extant were made many centuries after the date of composition of the text. How nearly then does modern punctuation indicate the author's intentions? Obviously in many if not in most cases the end of a period is determined without ambiguity, and, for statistical purposes, it is the period length which is required when we refer to a "sentence". Differentiation between the Greek colon and the full stop is then unnecessary and avoidance of this differentiation obviates many points of difference between modern editors. This does not however deal with places where ambiguity exists and where the author's intentions as to the ending or otherwise of the period require to be known in order to establish the meaning. Aristotle (*Rhetoric*) writing of composition states that the ending of a period should be clear from the text and that it should not be necessary for it to be marked by a *paragraphos*. However, period endings were marked by Aristotle's contemporaries either by a break in the line (Greek of the fourth century did not show breaks between the words) or by a break with the addition of the *paragraphos* (παράγραφος, or more rarely παραγραφή see Kenyon, 1899). The paragraphos was a short stroke below the first letters of the line in which a period ends. It may be taken therefore that the end of the period was either obvious to the scribe or editor, or was indicated by a break in the line emphasized sometimes by a paragraphos. Other breaks in the line only occurred when it was necessary to avoid ambiguity by emphasizing the end of one word and the beginning of another or to indicate rough breathings, again sometimes necessary to avoid ambiguity; e.g. among the relative pronouns. It should be assumed therefore that on the autograph copy the author's intentions were clear.

Most of the texts of ancient writings now in use as the primary sources of authority for the text belong to the tenth century A.D. or later. In these, words are shown separated and punctuation (also accentuation) is included. Confidence in their integrity can only rest on internal evidence and comparative studies such as the present investigation. If obvious interpolation and ambiguous passages are avoided there is no real reason for supposing that the remaining material, where continuous prose, is not substantially as the author left it, and that the lengths of the periods reflect those in the author's autograph copy. In the data collected here care has been taken to omit sentences where the text of several manuscripts disagree to an extent sufficient to alter the sentence length, the *apparatus criticus* being the guide accepted. Usually about 2 per cent. of the total sentences have to be omitted but where this percentage is much exceeded the text has been held to be too corrupt to permit the evaluation of a distribution.

Most modern texts are derived from standard editions usually established during the nineteenth century. These editions have formed the bases of subsequent ones, alterations being made where they are thought necessary by the editor either as a matter of academic judgment or to accommodate readings found in more recently discovered MSS. Many modern texts are derived in this way from the great edition of Bekker. Comparison of the punctuation of two modern texts, therefore, is not always a comparison of two modern independent editings of an original, but a comparison of two texts derived by minor amendment only from a single text. This objection does not apply to a comparison of the older established text of Aristotle's *Categories* with a new text that has recently become available (Minio-Paluello, 1949). This edition has been independently re-punctuated and comparison with the standard text shows clearly the order of modification which ambiguity in period length can make to a length distribution.

4. *Effect of Editing on Sentence-Length Distribution*

A count was made of the sentence lengths of the first eight chapters of Aristotle's *Categories* (the ninth chapter contains a lacuna and interpolations whilst the tenth chapter corresponds to the start of a section usually held to differ from the other nine) in both the Oxford (Minio-Paluello, 1949) and the Loeb Classical Library (Cooke, 1938) editions. Many of the sentences are, or contain, quotations, definitions and so forth; matter that is not within the strict class of continuous prose. Sections containing such sentences were omitted and only the remainder used giving two distributions of 312 and 326 sentences respectively from the two editions. About two-thirds of the sentences are common to both editions and the remainder differ either by the incorporation or omission of words or by punctuation, the former difference arising from different readings of the MSS. and the latter by different interpretation of the meaning or grammatical function of the same words. It is possible to separate these two types of difference and, when this is done, two distributions are obtained for the first type having, surprisingly, the same number of sentences and which show that the two editors, even though they disagree frequently on the inclusion of minor words and phrases, do not differ greatly, and the means of these two distributions are 14·89 and 14·81 Loeb and Oxford respectively, the ninth deciles, 25·2 and 25·1. Differences due to the incorporation or omission of words are, therefore, negligible.

Differences due to punctuation when differences of the first type have been eliminated have a greater significance in the present investigation and cause a change in the mean of the linear distribution from 15·33 for the older (Loeb) edition to 15·93 in the newer work. This is caused by the elimination of 14 sentences which the altered punctuation of the Oxford text incorporates into other sentences. It is, of course, obvious that a change of punctuation so that two contiguous sentences of x and y words become on re-punctuation $(x - a)$ and $(y + a)$ cannot influence the mean sentence length. The reviewer of the new edition (Rees, 1951) had noticed this change for he remarks; "But one of M's greatest services lies in his repunctuation; gone is Waitz's series of short, clipped sentences, and the sequence of thought is again and again rendered more clear thereby". As would be expected the changes affect the tails of the distributions most and in an opposite sense. The first quartile of the linear distribution rises from 13·3 to 14·1 whilst the ninth decile drops from 27·6 to 26·7, both Loeb and Oxford. It should be noted that none of the changes are significant compared with the sampling errors of the constants mentioned.

For readers who prefer to transform the variate, the log-normal distributions for the complete samples (Loeb and Oxford) are given in Table 2, the distributions being grouped as for Table 1. Table 3 gives the corresponding k statistics. Tests fail to show any significance in the difference between the two means or the two variances but the Loeb distribution is symmetrical whereas the Oxford distribution has a value for g_1 of $-\,0{\cdot}2716$ compared with its standard error of $0{\cdot}138$, giving a ratio of $1{\cdot}97$ which approaches significance. The two distributions are illustrated in Fig. 2.

TABLE 2

Sentence-Length Distributions from Aristotle's Categories

Sentence-Length (Words)	*Mean Log Length*	*Frequency*	
		Oxford	*Loeb*
4	0·55	2	3
5	0·65	7	12
6	0·75	15	15
7	0·85	21	20
8–10	0·95	41	51
10–12	1·05	35	49
13–15	1·15	52	51
16–19	1·25	48	42
20–25	1·35	55	41
26–31	1·45	23	26
32–39	1·55	11	12
40–50	1·65	1	1
51–63	1·75	1	1
64–79	1·85	—	2
Totals . . .		312	326

TABLE 3

k-*Statistics Illustrating Editorial Differences*

Statistic	*Oxford Edition*	*Loeb Edition*
k_1 (mean) . . .	1·149	1·127
k_2 (variance) . . .	0·0510	0·0571
k_3	−0·003128	0·0000907
k_4	0·001114	−0·000539

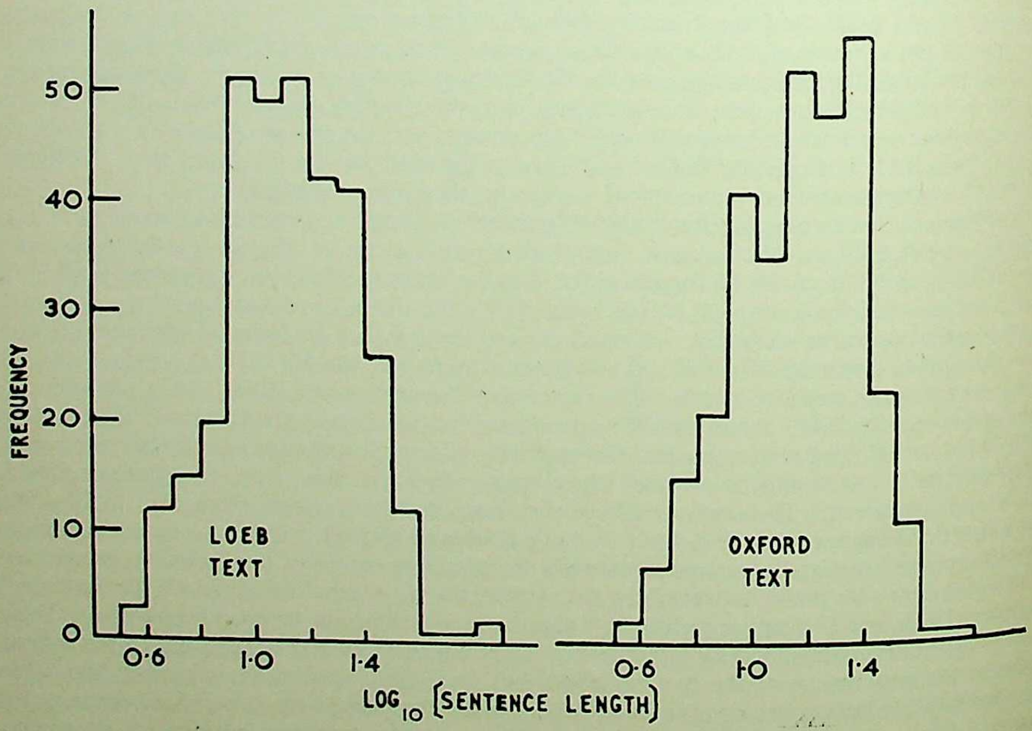

FIG. 2.—Log-normal distribution for Aristotle *Categories*.

This crucial example demonstrates that editorial modification alters the sentence-length distribution but that the alteration is small compared with the variation expected from random sampling and requires fine techniques (normalization and the use of the fourth moment) before it is demonstrably significant in the statistical sense.

5. *Sampling*

Greek works are short, usually 300 to 500 sentences and frequently the sample from which the distribution is constructed is necessarily the whole work. However, sometimes it is possible or desirable to use only a proportion of the work and it is then necessary to decide how the proportion—the sample—will be chosen. This arises if it is desired to make a rapid survey of an author's works when, if the results are of sufficient interest to warrant it, a return can later be made to extend the sample to the whole work. Also, if two or more samples can be obtained from one work, such samples can be used to compare the variability within works with that between works. The two most convenient methods of sampling are *Block Sampling* and *Spread Sampling*. In the former a block of sentences is chosen from the work, and in the latter a number of blocks are chosen spread through the work; e.g., alternate pages of the text. *Random Sampling* is unacceptable because of the labour involved since the sentences would first be required to be numbered serially, and because when one sample has been so chosen a second sample independent of the first cannot be obtained. In fact, authors do not use sentences randomly but short and long sentences tend to occur grouped together. If the sample is of reasonable size sampling will average these groups and hence minimize differences in distributions when sampling books of finite length not much greater than the sample size If two samples are to be obtained from one work whose total length is not much greater than the sum of the two samples then, by random sampling, two estimates are obtained for the constants of the distribution for the entire work rather than estimates of the constants of two independent distributions.

When spread sampling is used it should be noted that the beginnng and ending of a work are best avoided since textual corruption tends to be greater than in the body of the work, and also if a further sample is later required it must be obtained by sampling further into the work and not by sampling from between the blocks used in obtaining the first spread sample.

It would appear from the foregoing sections that sentence-length distributions represent fairly clearly the author's distribution of period lengths, surviving the hazard of a punctuation added in full at a later date and the further hazard of modern editorial change. It should be clear of course that these remarks apply only where a good MS. tradition exists and cannot be true when an obviously mutilated text is considered. It is now necessary to establish for the Greek texts what Yule established for English prose, namely, that the sentence-length distribution of an author remains constant, within sampling error, from work to work. To establish this two authors are discussed in the next section, Xenophon and Aristotle, and the distributions obtained from groups of genuine works are examined for constancy and also compared with works ascribed to them in early times but now known to be spurious.

6. *Distributions from Works of Known Authorship*

Xenophon.—The works used for this demonstration were chosen by their chance occurrence on the writer's shelves and in each case the complete work formed the block sample for the distribution except for the *Cynegeticus* where the work is sufficiently long for it to form two equal blocks. The linear distributions are given in Table 4 and the constants derived therefrom in Table 5. The *Cynegeticus* is universally accepted as spurious and the differences of the sentence-length distributions are obvious to simple inspection. Table 6 gives the means for the constants of the genuine works together with their standard errors (calculated from the between samples variability and therefore possessing 5 degrees of freedom) and the means of the constants for the *Cynegeticus*. The figures quoted for the S.E.'s assume the samples to be of equal size and they can be shown to be of the same magnitude as would be expected from random sampling from the same population.

Aristotle.—The form in which Aristotle's writings have come down to us has been a matter of frequent discussion and controversy. The *Poetics*, for example, cannot be said to be prose in any ordinary sense of the term and may be the notebook jottings of one of his students or lecture headings prepared by himself or even by someone else lecturing on this work. Fortunately, these uncertainties are largely absent from the zoological works which are treatises written in continuous prose and written down fairly fully by Aristotle himself since he says so in *De Partibus* (Aristotle, *a*). The fact that these works are written in a definite prose style and not in abbreviated form suits them admirably to our purpose. There are many cross-references between them and there is little doubt that of the ones named below a large section is the work of Aristotle himself. The works which have been examined are: *De Generatione*, *De Partibus*, *De Incessu Animalium*, *De Motu Animalium*, *Historia Animalium*.

TABLE 4

Sentence-Length Distributions from Xenophon

Sentence-Length (*Words*)	Hero	Agesilaus	Ways and Means	Hippar-chicus	Spartan Rep.	Horses	Cynegeticus	
							I	II
1–5 . .	13	6	8	4	8	8	22	37
6–10 . .	36	56	37	54	64	64	80	74
11–15 . .	49	98	50	67	68	96	89	67
16–20 . .	30	62	40	63	52	67	58	54
21–25 . .	26	53	22	28	36	46	25	29
26–30 . .	11	26	14	30	25	31	8	15
31–35 . .	16	17	11	16	8	16	7	4
36–40 . .	5	13	6	8	5	9	2	7
41–45 . .	3	6	5	6	1	4	4	2
46–50 . .	1	7	1	6	1	1	1	—
51–55 . .	6	2	1	1	1	2	—	—
56–60 . .	1	3	—	1	2	1	—	1
61–65 . .	—	1	1	2	2	2	—	1
66–70 . .	1	—	1	1	—	..	—	..
71–75 . .	..	1	—	..	1	..	1	..
76–80 . .	..	—	1	..	..	..	1	..
81–85 . .	..	—	—	..	..	..	—	..
86–90 . .	..	—	1	..	..	..	—	..
	..	1	..	..	..	..	1	..
	..	(106–110)	..	..	..	..	(96–100)	..
	..	1	..	..	..	..	1	..
	..	(121–125)	..	..	..	..	(126–130)	..
Totals . .	198	353	199	287	274	347	300	291

TABLE 5

Constants of Distributions of Table 4

Work	*Mean*	*Median*	*First Quartile*	*Third Quartile*	*Ninth Decile*
Hero	18·9	15·7	10·5	24·4	34·6
Agesilaus	20·0	16·8	11·3	24·5	35·4
Ways and Means . . .	19·2	14·9	11·0	23·7	34·2
Hipparchicus	19·6	17·0	11·5	25·4	34·3
Spartan Republic . . .	17·5	15·3	10·2	22·4	29·2
On Horses	18·1	16·1	11·3	23·2	30·6
Cynegeticus: Sample I . .	15·4	13·2	8·8	18·4	24·7
II . .	14·6	13·1	7·9	19·2	25·8

TABLE 6

Comparison of Typical Constants for Genuine and Spurious Works of Xenophon

Constant	*Mean Value for Genuine Works*	*Standard Error*	*Value for* Cynegeticus
Mean	18·9	0·4	15·0
First quartile . . .	11·0	0·2	8·4
Ninth decile . . .	33·1	1·0	25·3

The genuineness of the first three of the above list is accepted universally, that of *De Incessu* never having been questioned (Forster, 1937). *De Motu* has been the subject of claims and counter claims but the concensus of more recent opinion is in favour of Aristotelian authorship. The *Historia* differs from the preceding four inasmuch as it is accepted as being by a number of different hands (e.g., Jaeger, 1934, and Peck, 1937). It would be anticipated therefore that the majority of these works including *De Incessu* will have sentence-length distributions drawn from one population and that the *Historia* will probably differ from this majority.

In obtaining distributions for the Books of *De Generatione* it was observed that after Book I the quality of the text deteriorates. In recording the sentence length data whenever there is some doubt about the length of a sentence due to the supposed existence of a gloss or the actual existence of an obvious scribal error, it has been customary to record SM (sentence missing) instead of the number of words. It is felt that this results in less biasing of the distribution than the recording of a spurious length provided that the number of SM's recorded can be disregarded in relation to the total number of sentences counted. With most works examined the number is small and of the samples taken from other works of Aristotle about 1 to 2 per cent. are missed. With Books II to V of *De Generatione*, however, 6 to 7 per cent. were marked SM, the uncertain sentences several times occurring together as short paragraphs. These data have therefore been discarded and in the following analysis only Book I of *De Gen.* has been used. Table 7 gives the complete distributions which were obtained from the locations given in the list of references (Aristotle, *b* to *r*). The constants of the distributions and the size of the samples are given in Table 8. The mean of the constants for all the works except the *Historia* are given in Table 9 together with the standard errors calculated from the between samples variation. Also quoted in Table 9 are the expected standard errors obtained by Monte Carlo methods and adjusted to a sample size of 250, slightly larger than some of the samples taken. The evidence is conclusive that the observed variation is no greater than would be expected from random sampling of a single population. Table 10 compared the means of the constants for the *Historia* with the means quoted in Table 9 and their standard errors and it is obvious that a different population is involved.

TABLE 7

Sentence-Length Distributions from Zoological Works of Aristotle

Sentence-Length (*Words*)	De Gen	De Partibus				De Motu	De Incessu	Historia									
		Book						*Book*									
	Bk. I	*I*	*II*	*III*	*IV*			*I*	*II*	*III*	*IV*	*V*	*VI*	*VII*	*VIII*	*IX*	*X*
1–5	14	15	9	11	5	16	13	14	11	4	13	8	4	5	19	12	3
6–10	61	51	41	59	42	49	65	72	62	25	66	20	36	24	33	44	25
11–15	60	50	44	63	60	54	85	59	58	27	66	28	12	24	27	22	22
16–20	43	43	46	52	36	42	65	29	40	19	47	17	21	21	17	13	21
21–25	38	31	26	24	19	21	34	20	17	10	22	13	7	16	6	10	18
26–30	20	21	10	16	20	18	24	9	13	16	14	7	7	8	6	1	8
31–35	10	13	8	12	10	8	17	6	6	6	7	6	5	4	—	5	6
36–40	28	11	5	6	5	6	7	2	2	2	5	1	—	5	—	1	3
41–45	6	7	1	3	2	6	8	6	—	1	1	—	1	—	2	—	1
46–50	2	1	6	1	—	5	1	—	4	—	2	—	2	—	1	1	1
51–55	5	5	4	3	1	2	3	—	..	1	1	—	—	1	—	..	..
56–60	1	—	—	2	..	—	2	2	..	2	..	—	—	..	1	..	..
61–65	1	—	2	—	..	1	1	..	..	..	..	—	1	..	1	..	..
66–70	1	1	—	—	..	1	2	..	..	..	..	1	..	..	..	..	..
71–75	—	—	1	1	..	..	—	..	..	..	..	..	..	..	..	..	..
76–80	—	—	..	..	..	..	—	..	..	..	..	..	..	..	..	..	..
81–85	—	—	..	..	..	..	—	..	..	..	..	..	..	..	..	..	..
86–90	—	—	..	..	..	..	1	..	..	..	..	..	..	..	..	..	..
	1	1	..	..	..	..	1	..	..	..	..	..	..	..	..	..	..
	(101–5)	(91–5)	..	..	..	..	(121–5)	..	..	..	..	..	..	..	..	..	..
	..	..	..	..	..	..	1	..	..	..	..	..	..	..	..	..	..
	..	..	..	..	..	..	(131–5)	..	..	..	..	..	..	..	..	..	..
Totals	271	250	203	253	200	229	330	219	213	113	244	101	96	108	113	109	108

TABLE 8

Constants of Distributions of Table 7

Work	*Mean*	*Median*	*First Quartile*	*Third Quartile*	*Ninth Decile*	*Sample Size*
De Gen . . .	18·7	15·5	10·7	23·8	34·4	271
De Part: Bk. I .	19·2	16·5	10·1	25·0	35·0	250
II .	18·9	16·3	10·6	22·9	34·7	203
III .	17·4	15·0	9·9	21·5	31·7	253
IV .	17·2	14·9	10·8	22·3	30·0	200
De Motu . . .	18·0	15·1	10·7	23·0	34·3	229
De Incessu . .	19·2	15·5	10·8	23·4	33·7	330
Historia Bk. I . .	14·9	12·5	8·6	18·8	27·2	219
II . .	15·2	13·5	9·0	19·0	26·8	213
III .	18·3	15·6	10·4	25·4	31·1	113
IV .	15·5	13·7	9·3	19·6	27·5	244
V . .	16·4	14·5	9·8	21·6	29·0	101
VI .	16·2	13·8	8·3	20·3	30·1	96
VII .	17·3	15·3	10·1	22·7	30·0	108
VIII .	13·6	11·3	6·9	17·2	25·3	113
IX .	13·0	9·2	7·2	16·9	24·0	109
X . .	17·7	16·4	10·3	23·3	30·7	108

TABLE 9

Variation of Constants and Expected Sampling Error

Constant	*Average Value*	*Observed Standard Error*	*Expected Standard Error*
Mean	18·4	0·8	0·95
Median	15·5	0·6	0·7
First quartile . . .	10·5	0·4	0·6
Third quartile . . .	23·1	1·1	1·25
Ninth decile . . .	33·4	1·85	3·7

TABLE 10

Constants for Historia *Compared with Other Works*

Constant	*Average Value* Historia	*Average Value Remainder*	*Standard Error* Historia *Observed*	
			Observed	*Expected*
Mean	15·7	18·4	1·6	1·5
Median	13·6	15·5	2·2	1·0
First quartile . . .	9·0	10·5	1·2	0·95
Third quartile . . .	20·5	23·1	2·75	2·0
Ninth decile . . .	28·2	33·4	2·4	5·8

Substantial agreement exists therefore between the conclusions of classical scholars and deductions from sentence-length distributions and it may be accepted that the first four zoological works are the work of Aristotle himself whereas the *Historia* is by another hand or, at least a composite document. The time of composition of the four works is not known and they probably all belong to the later period of Aristotle's life. Even so, the time occupied in writing them must have spread over several years and we have evidence therefore of the constancy of his sentence-length distribution over the unknown but considerable period of time represented by their composition.

Plato.—Plato lived from about 429 to 347 B.C. and is generally held to have written as his first surviving work, *The Apology*, some few years after the death of Socrates in 399 B.C. *Laws* are regarded by all scholars as a work of his old age, so that the period of his literary production as given by his extant writings spreads over some 45 to 50 years.

Plato provides a severe test for any method of stylometry. His dialogues extend from short works in which the cut and thrust of sharp debate predominates, to long philosophical discussions which are dialogues in title only. These wide limits are yet insufficient, for he frequently varies the dialogue with reported speech or allows a principal character to frame rhetorically his own questions and answers. It is obvious that such a range of style is bound to influence considerably the distributions of sentence lengths, and the sampling of 200 sentences from a majority of the accepted works shows a variation in mean length from 9 to 28 words. This variation is only slightly reduced by the omission of sentences of one or two words which occur frequently as replies to questions in dialogues usually regarded as early. It is obvious that the dialogue form imposes limits on the sentence length if each of several characters in it are to play their part. It is obvious also that many short interjections will occur which weight the distribution of sentence lengths. Detailed examination of the early Platonic dialogues showed that they are, in fact, unsuited to examination by this method since the distributions obtained depend on the relative frequency of long and short speeches, and on the number of interjections made in the course of the argument. However, as mentioned above, some of Plato's dialogues are dialogues in name only and contain long speeches from a principal character with only an occasional remark from one of the others. Such long speeches, if the interrupting remarks be omitted, can be treated as continuous prose, and it is to continuous prose that constancy of sentence-length distribution has been attributed and has so far been demonstrated.

The samples taken therefore were those from the longer speeches only, omitting the interruptions, and this limited the works from which they could be taken to those listed in Table 11. All works were included which gave a sample of at least 200 (actually 194) sentences and the works were split into their constituent books provided that the sample length still exceeded 150 sentences.

TABLE 11

Sentence-Length Distributions from Plato

Sentence-Length (Words)	Gorgia	Phaedo	Phaedrus	Timaeus				Laws							Letter
				A	*B*	*C*	*Total*	*V*	*VI*	*VII*	*VIII*	*IX*	*XI*	*XII*	*VII*
1–5	23	10	10	2	8	5	15	7	1	10	4	5	12	4	10
6–10	38	26	28	25	24	21	70	26	29	23	19	14	24	21	24
11–15	37	27	56	32	46	26	104	34	53	34	24	24	31	27	30
16–20	33	29	42	29	42	31	102	38	52	28	24	19	33	23	31
21–25	18	12	31	29	27	17	73	28	47	34	16	19	26	19	22
26–30	22	23	23	26	18	24	68	21	33	12	13	17	20	28	21
31–35	16	13	23	16	12	17	45	11	25	16	9	14	13	20	15
36–40	5	20	15	10	11	20	41	16	20	12	9	11	14	15	14
41–45	4	11	11	9	4	10	23	11	19	14	9	6	10	9	9
46–50	4	6	8	7	4	7	18	4	8	8	7	8	8	7	5
51–55	5	5	5	5	3	5	13	3	8	4	4	5	6	7	6
56–60	4	2	3	4	3	7	14	1	4	4	8	6	5	7	—
61–65	—	1	3	3	4	3	10	1	5	4	1	3	4	6	2
66–70	3	3	1	3	1	2	6	—	1	3	4	1	5	—	3
71–75	1	2	1	3	2	3	8	3	3	—	3	—	2	2	2
76–80	2	1	1	1	—	1	2	1	2	4	1	—	1	2	2
81–85	2	—	—	1	1	1	3	1	—	1	..	—	3	—	1
86–90	—	2	1	—	1	—	1	1	—	—	..	—	1	1	4
91–95	1	—	1	1	1	1	3	1	1	1	..	—	..	1	3
96–100	..	—	..	—	1	1	2	1	1	1	..	2	..	1	—
101–105	..	—	..	—	..	—	—	—	—	2	..	1	..	1	—
106–110	..	—	..	1	..	—	1	—	—	1	..	..	..	—	—
111–115	..	—	..	1	..	—	—	1	—	—	..	..	..	—	—
116–120	..	—	..	..	..	1	1	..	—	—	..	..	..	—	1
	..	1	..	..	..	1	1	..	1	1	..	..	..	1	1
	..	(126–130)	..	..	..	(206)	(206)	..	(121–125)	(162)	..	..	..	(169)	(153)
Totals	218	194	263	208	213	204	625	210	313	217	155	155	218	202	206

TABLE 12

Constants of Distributions from Table 11

Work	*Mean*	*Median*	*First Quartile*	*Third Quartile*	*Ninth Decile*	*Sample Size*
Gorgias . . .	21·7	17·2	9·6	28·8	45·7	218
Phaedo . . .	26·6	22·2	12·8	36·9	48·5	194
Phaedrus . . .	24·2	20·0	13·0	32·1	44·5	263
Timaeus: A . .	27·7	23·3	14·4	34·6	52·7	208
B . .	23·6	18·9	12·9	29·0	45·1	213
C . .	30·0	25·9	15·3	38·5	55·9	204
Laws: Bk. V .	25·1	20·5	13·4	32·1	44·1	210
VI .	26·8	22·8	15·1	33·9	45·4	313
VII .	28·6	22·5	12·2	37·9	55·9	217
VIII .	27·8	22·5	13·8	39·5	56·4	155
IX .	28·3	24·6	14·6	37·5	52·0	
XI .	27·1	22·2	13·5	37·1	54·8	
XII .	30·3	26·8	15·2	38·7	56·8	

TABLE 13

Variability of Samples from Laws

Constant	*Estimated Variance*	*Expected Variance*	*Probability*
Mean	3·70	1·7	..
Median	5·4	2·1	<0·05
First quartile . . .	1·2	1·1	..
Third quartile . . .	6·0	6·0	..
Ninth decile . . .	22·0	9·9	<0·05

TABLE 14

Possible Trend in Distributions

Work	*Mean*	*Ninth Decile*
Gorgias . . .	21·7	45·7
Phaedo . . .	26·6	48·5
Phaedrus . . .	24·2	44·5
Timaeus . . .	27·1	51·9
Laws	27·7	52·2

The edition used was that of J. Burnet (1899 and 1905) and a long speech was defined as one extending for at least one of Burnet's pages. The constants calculated from the distributions are given in Table 12. The sizes of the seven samples obtained from *Laws* vary somewhat, but if the ranges covered by the constants of the distributions are considered, estimates (though not very efficient ones) can be obtained (Lindley and Miller, 1953) of their variances which are given in Table 13 together with the expected values obtained by Monte Carlo method. It is seen that the range of the constants is greater than would be expected to arise at the 0·05 probability level and since the constants of the samples of all works given in Table 11 cover a greater range than those from *Laws* only, it follows *a fortiori* that the variability of Plato's sentence-length distribution is greater than would be expected to arise from the random sampling of a single population. In Plato we have an author unlike Aristotle or Xenophon. To state this is to state a truism for, as was noted earlier, Plato was above all a stylist and the form used by him for expressing his ideas is one expected to give rise to large fluctuations.

Since the distributions obtained are not to be regarded as random samples from a single population, can we demonstrate the existence of any regular trend? This does in fact appear to exist in Table 14 where average values for the samples from *Timaeus* and *Laws* have been inserted in place of the values for the separate books. The order of the works in the table is that usually accepted and it is tempting to speculate that the order given for *Phaedo* and *Phaedrus* is incorrect. However that may be, they are both works of his middle period whereas *Gorgias* is relatively early and *Timaeus* and *Laws* are known to be late works, *Laws* being a work of his old age the completion of which must have spread over some considerable time. To what extent do these results with Plato invalidate the statement made in section 2 that differences between samples (from the same author) are only those expected from random sampling? The present writer feels that the data collected from Plato's works are exceptional for the reasons already given but summarized below, and have an interest of their own.

(1) The dialogue form makes it difficult to obtain samples that can reasonably be called continuous prose.

(2) The samples that it has been possible to obtain are known to be shorter than desirable.

(3) The variability outside that from random sampling is demonstrable within a single work (*Timaeus*).

(4) The trend observed is associated with a noticeable change of style from one in which a dialogue still predominates (*Gorgias*) to one in which the prose is, or is almost continuous.

7. *Works of Uncertain Authorship*

(*a*) *Plato's* Seventh Letter

The length distribution of 206 sentences from this work is given in Table 11 and the question to be answered is whether there is any reason to suppose the distribution to be other than a sample from the general population of sentences written by Plato. Comparison of the constants of the distribution which are given in Table 15 together with those from *Laws* shows an agreement so good that statistical proofs of the insignificance of the differences are superfluous. It might also be noticed that, given the reality of the trend discussed above, not only would there be no reason for believing the letter to be other than one of Plato's works, there would be strong indications that it belongs to the latter part of his life as, of course, is usually agreed by those who support its authenticity.

TABLE 15

Comparison of Constants for Distribution from Seventh Letter *with* Laws

Constant	Seventh Letter	*Average of* Laws
Mean	28·1	27·7
Median	22·3	23·1
First quartile . . .	13·1	13·5
Third quartile . . .	36·0	36·7
Ninth decile . . .	54·2	52·2

(*b*) *The* Ethics *of Aristotle*

The problem now to be presented is a *cause célèbre*. The *Ethics* of Aristotle present an interesting problem in that three works have survived with his name attached, two of them dealing entirely with the same subject matter and even possessing three books in common, whilst the third, the *Magna Moralia* is regarded as a later pastiche although Sarton (1953) has raised the possibility of Aristotelian authorship.

Of the two important works, the *Nicomachean Ethics* is now usually taken to be the work of Aristotle, and the *Eudemian Ethics* the work of a pupil. Some critics have held the common material to be integral with the *Nicomachean Ethics*, a few the reverse, and some that it has been incorporated from a work by one of his pupils. The question as to whether the common portion of the two *Ethics* is integral with either of them is ideally a problem for statistical examination. *If the common portion is integral with one* we may expect that its sentence-length distribution will be *distinguishable from one but not from both of its environments*. The genuine zoological works of Aristotle have already been examined in section 6 so that, provided his style does not change in going from zoological to philosophical works, it should further be possible to identify the Aristotelian portion.

Burnet's edition (1900) was used for the text of the *Nicomachean* and the Teubner edition (Susemihl, 1882) for the *Eudemian* which Burnet does not give. A check in randomly chosen places showed that for the *Nicomachean Ethics* the two editions were substantially the same text and the punctuation agreed between the editors. The full linear distributions are given in Table 16 and the constants derived therefrom in Table 17.

TABLE 16

Sentence-Length Distributions from Aristotle's Ethics

	Frequency														
Sentence-Length (Words)	Common Books			Nicomachean Ethics							Eudemian Ethics				
	A	*B*	*C*	*I*	*II*	*III*	*IV*	*VIII*	*IX*	*X*	*I*	*II*	*III*	*VII*	*VIII*
1–5 . .	9	4	8	11	6	8	15	12	11	9	5	14	14	16	16
6–10 . .	28	26	23	28	52	34	43	44	25	26	16	31	30	46	22
11–15 . .	18	32	21	23	59	32	15	25	30	25	22	19	18	31	24
16–20 . .	18	13	21	19	49	17	9	12	15	29	19	15	16	21	17
21–25 . .	14	5	10	10	24	10	7	3	12	3	11	7	8	6	10
26–30 . .	7	9	5	5	21	6	4	2	6	5	6	3	1	6	5
31–35 . .	4	4	6	3	13	2	2	2	4	4	4	5	4	4	3
36–40 . .	1	2	3	—	3	2	3	1	2	1	9	2	3	1	2
41–45 . .	1	3	1	2	6	—	1	1	1	..	2	2	3	1	1
46–50 . .	2	—	—	2	—	1	—	..	..	..	3	—	1	2	1
51–55 . .	1	—	—	1	—	..	—	..	..	..	—	1	—	..	..
56–60 . .	1	1	1	1	—	..	—	..	..	..	—	—	1	..	..
61–65 . .	1	—	—	..	1	..	—	..	..	..	—	—	1	..	..
66–70 . .	..	—	—	..	—	..	—	..	..	..	2	1	..	..	..
71–75 . .	..	1	—	..	1	..	1	..	..	..	1	..	..	..	..
76–80 . .	..	..	—	..	..	..	..	..	..	..	1	..	..	..	..
	..	..	1	..	..	..	..	..	..	..	1	..	..	..	..
	..	..	(111–115)	..	..	..	..	..	..	..	(91–95)	..	..	..	..
											1				
											(101–105)				
Totals .	105	100	100	105	235	102	100	102	106	102	103	100	100	134	101

TABLE 17

Constants of Distributions of Table 16

Book		*Mean*	*Median*	*First Quartile*	*Third Quartile*	*Ninth Decile*
Common Books:	A	17·2	14·8	8·6	22·6	31·1
	B	17·0	13·6	9·5	20·5	32·0
	C	17·5	15·0	9·2	21·5	32·2
Nicomachean:	I	15·8	13·5	8·2	19·8	29·0
	II	17·7	15·5	10·6	22·6	30·7
	III	14·5	12·6	8·1	19·2	26·3
	IV	13·2	9·6	6·7	16·6	26·8
	VIII	11·8	9·9	7·0	14·6	20·5
	IX	15·0	13·4	8·6	20·0	27·5
	X	14·3	13·7	8·7	18·3	25·2
Eudemian:	I	22·6	17·7	11·6	29·0	42·2
	II	15·0	11·8	7·3	19·2	31·5
	III	15·6	12·2	7·4	20·2	34·2
	VII	13·5	12·9	7·4	17·3	26·3
	VIII	14·6	13·1	7·6	19·5	27·4

It is immediately apparent that the expectation that the common books would differ from one but not both environments is not realized. The common books appear uniform amongst themselves but the remainder of the *Nicomachean Ethics* is extremely variable, and, if the work of one writer, then this writer is much more variable in his sentence lengths than is the author of Books V to VII. It may be, however, that more than one writer is involved, in which case it should be noted that Book II is statistically indistinguishable from the shared books. Similarly the *Eudemian Ethics* is heterogenous since Book I differs greatly from the remaining books (II, III, VII, VIII) which themselves differ from the common books (in this case appearing as Books IV to VI). Books II, III, VII, and VIII, are less variable amongst themselves and could be random samples from one population and hence, could be the work of a single author.

Returning to the shared books, it will have been noticed that the constants in Table 17 are remarkably similar to those of Table 9 which are from the four Zoological works. These four works have indistinguishable distributions and they provide a criterion for the work of a single author who is certainly Aristotle himself. The shared books of the two *Ethics* are distinguishable in sentence-length distribution from the remainder of both works and approach very nearly the distribution of the four zoological works. If this statistical evidence is acceptable to other critical arguments it points to an Aristotelian authorship only for the shared books, the remaining material being substantially the work of other hands. As far as is known to the present writer, this suggestion of Aristotelian authorship for the common books but not for the remainder, is new.

8. *Conclusion*

The markedly skew distributions of sentence lengths of passages of continuous prose seem to display a constancy which enables them to be used as objective criteria of authorship style. The distributions only vary from sample to sample in a manner expected for samples drawn randomly from a fixed population of sentence lengths. This seems to be true for Greek as well as English authors and the exception that Plato's works provide can be accounted for by the literary form adopted. In any case they are characteristic even though their variability is greater than would be obtained by random sampling of a single population for there seems to be a trend with age superposed.

Provided textual uncertainties are avoided and that such avoidance does not involve more than, say, 2 *per cent*. bias, the method could become of importance to classical scholars to supplement the traditional methods and to provide a foil against excessive subjectivity of judgment.

The finding that the books common to the two *Ethics* alone qualify for strict Aristotelian authorship is important for, of the various possibilities hitherto explored, this seems to have been overlooked. Of course it does not imply that Aristotle had nothing to do with the composition of the remainder, but it does imply that the two sets of books outside the common books have an equal status or, rather, an equal lack of status.

9. *Acknowledgments*

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WHOLESALE PRICES IN 1956

BY THE EDITOR OF "THE STATIST"

Method of Construction of the Index Numbers

The following table illustrates the method of construction of the index numbers. The index numbers here given are based on the average prices for the eleven years 1867–1877. Take, for instance, the *Gazette* price of English wheat:

	s.	*d.*	
Average, 1867–77 . .	54	6,	average point.
,, 1914 . . .	35	0,	64 or 36 per cent. *below* the average point.
,, 1930 . . .	80	7,	148 ,, 48 ,, *above* ,, ,,
,, 1936 . . .	53	3,	98 ,, 2 ,, *below* ,, ,,

The individual index numbers, therefore, represent simple percentages of the average point. The 45 articles are grouped in six categories. They are shown in detail in Table 5.

The general average is drawn from all 45 descriptions, which are treated as of equal value, and is the simple arithmetic mean.

TABLE 1

Construction of Index Number for 1956

1867–77 = 100

	Number of Commodities in Index	*Total Numbers*		*Index for 1956*
		1867–77	1956	
General average	45	4,500	17,320	385
Food	19	1,900	6,187	326
Vegetable food	8	800	2,550	319
Animal food	7	700	2,507	358
Sugar, coffee and tea . . .	4	400	1,130	282
Materials	26	2,600	11,133	428
Minerals	7	700	4,064	581
Textiles	8	800	3,197	400
Sundry materials . . .	11	1,100	3,872	352

TABLE 2

*Annual and Monthly Index Numbers**

1867–77 = 100

	Year	*Jan.*	*Feb.*	*Mar.*	*April*	*May*	*June*	*July*	*Aug.*	*Sept.*	*Oct.*	*Nov.*	*Dec.*
1902–1911 .	74	73·9	74·0	74·2	74·5	74·9	74·5	74·5	74·3	74·3	74·5	74·4	74·8
1912–1921 .	148	145·2	146·3	146·9	147·7	147·9	147·2	147·9	149·2	149·0	149·3	149·1	149·2
1922–1931 .	120	122·7	123·0	122·5	122·0	121·0	119·3	119·0	118·0	118·0	118·6	119·2	118·5
1932–1941 .	97	96·0	96·5	96·5	96·8	97·3	96·9	97·5	97·8	98·7	98·8	99·4	101·4
1942–1951 .	231	221·4	224·1	224·6	227·3	228·0	227·1	226·7	227·3	231·2	235·3	238·5	241·2
1943 . .	155	153·3	153·2	154·0	154·9	155·6	155·4	156·0	154·4	154·6	153·7	153·9	153·9
1944 . .	160	154·9	155·1	155·7	157·9	159·5	160·0	161·9	161·5	160·1	159·6	160·3	160·7
1945 . .	164	161·4	161·5	162·1	162·4	164·0	166·7	165·2	163·2	162·7	162·7	162·7	163·2
1946 . .	186	165·6	166·2	166·6	168·4	169·3	169·7	174·4	175·7	180·5	196·2	198·1	200·5
1947 . .	230	206·7	209·6	212·1	215·9	216·5	218·1	223·4	225·9	228·9	236·0	240·8	246·5
1948 . .	260	250·5	253·4	256·0	256·4	260·0	263·9	260·4	260·1	258·1	261·5	263·8	266·0
1949 . .	274	267·6	266·8	265·4	272·0	269·2	267·2	263·3	262·5	285·7	291·5	293·7	297·4
1950 . .	324	300·2	301·0	301·7	305·2	308·3	310·3	317·7	332·1	335·7	349·2	363·1	372·3
1951 . .	402	404·8	420·5	419·3	425·7	420·6	405·2	394·5	387·9	395·9	392·0	396·9	399·7
1952 . .	380	403·5	394·8	384·4	377·5	372·2	379·5	374·4	370·4	369·9	368·9	369·4	372·9
1953 . .	366	374·5	375·2	374·9	370·3	368·1	367·5	361·3	357·9	357·0	357·3	577·3	360·0
1954 . .	361	359·8	361·2	366·1	366·5	363·9	361·2	355·5	353·5	353·7	356·8	362·9	371·7
1955 . .	370	378·0	376·5	375·8	370·2	370·1	370·1	373.1	363.9	366·8	372·4	370·9	374.1
1956 . .	385	372·6	379·2	379·8	385·9	383·8	379·7	374·2	373·4	376·2	384·1	393·1	402·9

* The average of the twelve monthly figures of each year does not necessarily coincide with the annual figures, as the latter are calculated mostly from the average of 52 weekly quotations, while the former are based on end-of-the-month prices.

TABLE 3

Annual and Quarterly Index Numbers for Groups

1867–1877 = 100

	All Groups	*Food*				*Materials*			
		Total	*Vegetable Food*	*Animal Food*	*Sugar, Coffee and tea*	*Total*	*Minerals*	*Textiles*	*Sundry Materials*
1902–1911 . .	74	71	66	88	49	77	90	70	74
1912–1921 . .	148	146	136	167	100	154	166	151	148
1922–1931 . .	120	115	101	150	80	122	137	128	110
1932–1941 . .	97	88	84	116	49	103	131	94	91
1942–1951 . .	231	172	201	169	121	273	300	306	232
1943 . . .	155	138	156	156	72	167	187	166	156
1944 . . .	160	137	152	156	73	178	197	182	161
1945 . . .	164	139	155	156	78	182	209	189	159
1946 . . .	186	140	155	154	88	219	239	231	198
1947 . . .	230	156	191	149	100	284	304	295	263
1948 . . .	260	171	217	155	107	324	368	348	279
1949 . . .	274	201	240	185	151	328	382	356	273
1950 . . .	324	235	268	204	224	390	416	491	299
1951 . . .	402	265	305	227	249	502	516	639	394
1952 . . .	380	294	336	285	227	443	532	475	363
1953 . . .	366	325	365	317	261	399	473	430	329
1954 . . .	361	326	330	325	320	386	476	429	298
1955 . . .	370	329	319	367	281	400	529	410	310
1956 . . .	385	326	319	358	282	428	581	400	352
1955 1st Quarter .	376·8	339·8	333·7	365·8	306·2	407·2	517·1	437·6	307·2
2nd " .	370·1	330·6	323·9	375·0	266·1	399·1	510·6	417·0	315·1
3rd " .	367·9	324·2	312·8	383·6	243·2	400·0	542·3	401·3	308·5
4th " .	372·5	330·8	310·2	389·2	269·7	402·9	558·8	390·6	312·6
1956 1st Quarter .	377·2	324·7	323·2	361·5	263.4	415.7	579·4	392·5	328·5
2nd " .	383·1	335·5	342.9	362·6	267·7	418·8	565·6	391·4	342·9
3rd " .	374·6	312·2	300·3	351·1	268·1	420·2	578·7	390·0	338·8
4th " .	393·4	332·3	309·8	360·8	328·1	438·1	595·7	419·5	351·3

TABLE 4

Monthly Index Numbers for Groups

1867–1877 = 100

	Year	*Jan.*	*Feb.*	*Mar.*	*April*	*May*	*June*	*July*	*Aug.*	*Sept.*	*Oct.*	*Nov.*	*Dec.*
1955													
Food:													
Total . . .	329	341·7	340·0	337·7	332·2	329·3	330·2	329·3	315·0	328·3	335·6	328·5	328·2
Vegetable food .	319	335·9	334·0	331·2	320·5	317·2	334·0	327·3	304·1	307·0	314·8	308·4	307·5
Animal food . .	367	360·7	367·7	369·0	371·8	378·9	374·3	385·6	373·5	391·8	390·9	387·7	388·9
Sugar, tea and coffee .	281	320·3	302·5	295·9	286·1	266·8	245·3	234·9	234·6	260·0	280·4	265·1	263·6
Materials:													
Total . . .	400	404·6	403·2	403·7	398·0	400·0	399·3	405·3	399·7	395·0	399·3	401·7	407·7
Minerals . . .	529	515·0	513·8	522·4	509·1	512·0	510·6	542·7	542·0	542·4	546·8	555·5	574·2
Textiles . . .	410	442·9	439·4	430·4	422·2	415·6	413·1	410·9	402·7	390·2	392·6	392·4	386·7
Sundry . . .	310	306·4	306·6	308·7	309·7	317·3	318·4	313·7	307·0	304·7	310·3	310·6	317·0
1956													
Food:													
Total . . .	326	323·2	325·5	325·4	344·1	335·4	326·4	311·7	309·7	315·3	318·6	330·3	348·1
Vegetable food .	319	313·3	320·1	336·1	358·7	344·5	325·5	307·2	300·1	293·5	298·7	313·5	316·1
Animal food . .	358	367·6	361·9	354·9	365·3	363·7	358·8	345·5	349·1	358·7	345·6	349·9	386·9
Sugar, tea and coffee .	282	265·3	272·5	252·3	263·7	267·8	271·7	261·8	259·8	282·8	311·0	329·5	343·7
Materials:													
Total . . .	428	408·8	418·8	419·6	418·6	419·1	418·6	419·9	419·9	420·7	432·0	439·1	443·1
Minerals . . .	581	569·6	591·8	576·7	565·3	565·7	565·6	575·6	580·2	580·3	603·5	589·8	593·8
Textiles . . .	400	393·0	390·9	393·7	392·0	394·5	397·8	393·1	394·7	392·3	403·8	427·6	427·0
Sundry . . .	352	317·9	329·0	338·6	344·7	343·7	340·3	340·3	336·3	339·0	343·4	351·5	359·0

TABLE 5

Average Prices of Each of the 45 Commodities

1951 to 1955

			Average Prices					*Index Numbers* (1866–77 = 100)				
			1952	1953	1954	1955	1956	1952	1953	1954	1955	1956
	Vegetable Food											
1	Wheat, English Gazette	*s.* and *d.* per qr.	129 0	138 0	121 0	98 7	109 4	237	253	222	181	201
2	Man. No. 1 Vanc.	,, ,, ,,	108 11	112 5	124 7	127 6	130 9	194	200	222	227	233
3	Flour, G.R.	*s.* per sack (280 lb.)	80¾	102⅓	95¼	97¼	103⅓	175	222	207	211	225
4	Barley, English Gazette	*s.* and *d.* per qr.	116 4	102 4	91 11	92 11	91 8	300	275	236	238	235
5	Oats, ,, ,,	,, ,, ,,	74 5	68 0	62 10	73 1	68 9	286	262	242	281	264
6	Maize, American mixed (*a*)	*s.* per qr.	132¼	122⅓	114 9/16	114⅜	119 5/12	407	376	353	352	368
7	Potatoes, good English†	,, ton	295	300	304	374⅝	454½	252	257	260	320	388
8	Rice, Burma. No. 2	*s.* and *d.* per cwt.	83 6	107 9	90 0	174 3	63 6	835	1077	900	743	636
	Animal Food											
9	Beef, prime	*d.* per 8 lb.	158	176	187	234	200	268	300	317	397	340
10	,, middling	,, ,,	140	150	170	222	184	280	300	340	444	368
11	Mutton, prime	,, ,,	190	208	196½	214	205¼	301	330	312	340	326
12	,, middling	,, ,,	111	113	139	182	181	202	206	253	331	330
13	Pork, large and small, average	,, ,,	190	202	196	206	218½	365	389	377	396	420
14	Bacon	*s.* per cwt.	281½	339	296	270⅝	298	380	458	400	366	403
15	Butter	,, ,,	250	292½	346	369½	399⅝	200	234	277	296	320
	Sugar, Coffee and Tea											
16	Sugar, Java floating cargoes (*b*)	,, ,,	33	26⅝	26⅛	26	28¾	116	94	92	91	101
17	,, Continental whites	,, ,,	,,	36¼	35 3/16	37 1/16	42½	,,	157	153	161	185
18	Coffee, Ceylon plantation, low middling (*c*)	,, ,,	471½	448⅔	609	438½	491	542	516	700	504	564
	Rio, good	,, ,,	380	387	481	312	321⅓	594	604	752	488	502
19	Tea, average import price	*d.* per lb.	38·36	40·03	54·65	64·61	53·62	222	234	310	375	311
	Minerals											
20	Iron, Scottish pig	*s.* and *d.* per ton	291 10	328 9	344 8	372 4	428·2	} 424	468	500	540	603
	Cleveland (Middlesborough) pig	,, ,, ,,	255 8	274 10	296 3	324 7	350·5					
21	bars, common	£ per ton	26⅕	28⅜	31¼	38⅞	43⅜	325	344	379	471	526
22	Copper, Standard	,, ,,	260	254⅕	248⅞	351⅕	328¾	347	340	332	468	438
23	Tin, Straits	,, ,,	964 9/16	721 7/16	707 15/16	740⅜	787 7/16	919	687	670	705	750
24	Lead, English pig	,, ,,	135 9/16	91⅗	96⅘	106 1/16	116¼	661	447	472	520	567
25	Coal, Best Yorkshire House*	*s.* per ton	64 11/12	69½	62½	66⅝	74¼	296	316	284	303	337
26	average export price	,, ,,	94·38	89·03	86·75	86·96	105·41	755	712	694	696	843

TABLE 5—*cont.*

			Average Prices					*Index Numbers* (1866–77 = 100)				
			1952	1953	1954	1955	1956	1952	1953	1954	1955	1956
	Textiles											
27	Cotton, Middling American	*d.* per lb.	38·92	31·83	32·72	31·70	26·95	432	354	363	352	299
28	East Indian	” ”	36·93	28·73	32·81	31·61	27·58	547	426	486	468	409
29	Flax, Medium grade continental retted	£ per ton	380½	321½	318⅚	312½	282⅓	737	628	635	592	543
	Belgian import price	” ”	312·3	269·9	279·4	243·7	228·4					
30	Hemp, Manila for roping	” ”	130 15/16	125	92	92⅜	96	527	474	396	470	474
	Italian S.B.	” ”	270¼	244¾	216¾	274⅛	273½					
31	Jute, L.J.A. Firsts	” ”	99⅞	79¼	94	84⅝	89½	526	417	495	445	471
32	Wool, Merino, Port Philip, average fleece	*d.* per lb.	120	126¼	111¼	99⅜	102 7/12	621	665	579	493	546
	Merino, Adelaide, average greasy	” ”	73	80¾	69⅛	54	67⅓					
33	English Lincoln half hogs	” ”	49½	60	63¼	61¾	61⅞	251	304	320	313	313
34	Silk, Japanese	*s.* per lb.	36⅔	40	36⅔	34¼	32 7/12	160	174	160	149	142
	Sundry Materials											
35	Hides, River Plate, dry‡	*d.* per lb.	25¼	22 15/16	19⅜	17 5/16	21¼	336	309	281	245	253
	River Plate, salted	” ”	20 9/16	20¼	19	16 1/16	15¾					
	average import price	” ”	30·96	27·47	25·02	21·71	20·95					
36	Leather, dressing hides	” ”	62½	57	55⅞	49¼	49	388	380	390	387	373
	average import price	” ”	72½	75¼	79¾	85¼	80½					
	Tallow, Town	*s.* per cwt.	86½	66¾	68¼	73⅜	73⅜	192	148	152	163	163
	Oil, palm	£ per ton	106⅛	83	71¼	84⅛	97	272	213	183	216	249
	olive	” ”	328¾	305⅜	263 13/16	260	416	657	611	528	520	832
	linseed	” ”	185	180	80¼	102⅝	137½	508	443	292	345	411
	Seeds, linseed	*s.* per qr.	272	209¼	182 15/16	207½	232½					
41	Petroleum, motor spirit (c.i.f.)	*d.* per imp. gall.	13·30	12·32	12·08	11·53	11·81	84	77	76	73	74
	Kerosene (burning oil) c.i.f.	” ” ”	12·58	11·36	10·87	10·86	11·74	199	180	172	172	186
	gas oil, c.i.f.	” ” ”	11·84	10·76	10·32	10·70	11·44	297	270	259	269	288
42	Soda, crystals	*s.* per ton	190	190	190	212½	222½	206	206	206	231	242
43	Sodium nitrate	*s.* per cwt.	30	29¾	27	27¼	28	214	212	193	195	200
44	Indigo, Bengal, good consuming	*s.* per lb.	14	12½	12½	12½	13¾	193	172	172	172	190
45	Timber, hewn, average import price	*s.* per load	376·00	374·64	338·12	338·44	342·64	829	758	717	762	777
	sawn, or split, average import price	” ”	569·24	489·49	479·04	530·16	543·02					

(*a*) La Plata from 1924–40; Argentine (Feeding Stuffs) from 1941–52; Yellow American subsequently. (*b*) Raw centrifugals. 96 per cent. Pol., from 1924. (*c*) East India, good middling from 1908–1947; Kenya from 1948. * Owing to change in selling methods the quotation for house coal alone, no longer ascertainable; from 1954 the quotations relate to the average pit price of all grades of Yorkshire coal. †Average of weekly quotations during the eight months, January to April and September to December. ‡Nigerian Butcher Dry in 1956; Dry Bahias not quoted.

TABLE 6

Index of Silver Prices

The base of the Index Numbers given below is 60·84*d.* per standard ounce = 100, this being a parity of 1 fine oz. of gold to $15\frac{1}{2}$ standard ozs. of silver.

	Price per oz. Standard (d.)		*Index Number*	
	Average for year	*At end of December*	*Year*	*At end of December*
1942 . . .	$23\frac{1}{2}$	$23\frac{1}{2}$	18·1	18·1
1943 . . .	$23\frac{1}{2}$	$23\frac{1}{2}$	18·1	18·1
1944 . . .	$23\frac{1}{2}$	$23\frac{1}{2}$	18·1	18·1
1945 . . .	$30\frac{1}{2}$	44	23·1	31·1
1946 . . .	$48\frac{5}{8}$	$55\frac{1}{2}$	36·7	41·6
1947 . . .	$44\frac{7}{16}$	45	33·3	33·8
1948 . . .	45	$42\frac{1}{2}$	34·1	31·9
1949 . . .	$49\frac{1}{5}$	64	32·8	33·3
1950 . . .	$64\frac{1}{5}$	70	33·7	36·4
1951 . . .	$77\frac{27}{32}$	77	40·5	40·1
1952 . . .	$74\frac{3}{8}$	$72\frac{3}{4}$	38·7	37·9
1953 . . .	$73\frac{15}{16}$	$73\frac{3}{4}$	38·5	38·5
1954 . . .	$73\frac{1}{2}$	$74\frac{1}{4}$	38·4	38·6
1955 . . .	$77\frac{1}{2}$	$78\frac{3}{4}$	40.0	40.8
1956 . . .	$79\frac{1}{8}$	$79\frac{1}{2}$	40.8	40·9

TABLE 7

Gold and Silver: Production

	Gold Value of output (£ *thousand*)	*Silver* (*million ounces*)				
		United States	*Mexico*	*Canada*	*Australia*	*Central and South America*
1942	289,153	54·5	84·9	21·8	14·2	31·8
1943	226,397	44·8	71·2	18·6	10·3	29·3
1944	209,031	37·4	63·0	14·8	9·4	29·7
1945	199,116	29·3	61·1	14·0	8·1	27·4
1946	202,435	21·4	48·3	13·6	9·0	26·1
1947	206,434	36·1	49·2	13·5	9·5	23·6
1948	214,647	36·1	45·8	17·0	10·1	23·0
1949	320,887	34·6	49·5	17·6	9·8	24·0
1950	329,580	42·0	49·0	23·2	10·7	28·0
1951	322,623	39·9	43·8	23·1	10·8	28·7
1952	329,381	39·8	50·4	25·2	11·4	32·6
1953	325,500	37·7	47·9	28·3	12·4	35·3
1954	344,865	35·6	39·9	30·7	13·8	33·0
1955	340,732	36·5	48·0	27·7	14·0	35·3
1956* . . .	351,600†	37·5	44·0	27·5	14·0	35·5

* Estimate. † Excluding U.S.S.R.

REVIEWS OF STATISTICAL AND ECONOMIC BOOKS

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1.—*Statistical Analysis of Stationary Time-Series*. By U. Grenander and Murray Rosenblatt. New York, Wiley, 1957. Stockholm, Almqvist and Wiksell, London, Chapman and Hall. 300 pp. 9″. 88*s*.

The statistical analysis of stationary time-series is a topic in which recent developments in stochastic process theory have caused fundamental re-assessments and revisions of older techniques. The present monograph, devoted entirely to this topic, will be a valuable reference book for some of the most important research contributions, including many from the authors themselves. It might, however, be remarked that the book is more a research monograph than a comprehensive treatise and should not be regarded in the latter sense, not only because the authors have tended to concentrate on problems of spectral analysis in which they are interested, but because the subject is one in which further development is not yet over, so that further results and procedures have appeared after this book, which has been somewhat late in appearing in print, was written.

The first three chapters are largely introductory, dealing with the relevant theory of stationary stochastic processes (including interpolation, extrapolation and filtering) and, in Chapter 3, estimation and goodness of fit procedures previously developed for what the authors term "finite parameter models". In Chapter 4 the authors consider estimation of the spectrum from a wider non-parametric viewpoint, though in confining their attention to (absolutely) continuous spectra they dismiss the practical possibility of discrete spectra in curiously cursory fashion. They correctly point out that if the spectrum has jumps one cannot in general estimate it consistently from a single realization, because of the possibility of the amplitude of the jump being a random variable. It is nevertheless an important practical problem to estimate a discrete spectrum when it has a *fixed* amplitude for each jump at an unknown frequency. In the case of a uniform continuous spectrum, this is the classical periodogram problem, and the mixed case, when the continuous spectrum is not uniform, was raised by Whittle in 1952.

The antagonistic behaviour of the variance and bias for different spectral estimates leads the authors to consider the mean square error, which includes both these errors, and the minimization of the mean square error is discussed. The mathematical treatment here and elsewhere aims on the whole at complete rigour, though the necessary detail for this is sometimes rather heavy and leaves less space for discussion of the practical properties of the methods arrived at. (Note that in the first problem to Chapter 4, on vector processes (p. 283), the summation limits in the expressions for the smoothed cross-spectral functions should include negative as well as positive values of the indices.)

The development in Chapter 6 of methods of constructing the confidence region for the entire distribution function is important. Further discussion would, however, have been useful of the power of the recommended procedure, which is based on equidistant boundaries, in discriminating against erroneous functions in different regions of the frequency λ. As the fluctuations of the periodogram sum increase with increasing λ, the power is presumably least for small λ. If a test is required sensitive in the region of small λ it might consequently be better to reverse the summation to one from π to 0. (Some other modifications proposed by the reviewer are noted in the problems on this Chapter.) The extension of Theorem 4 to comparing two sample spectral distributions (Theorem 6) is also worthy of note.

Chapter 7 deals with the accuracy of methods of estimating systematic terms linear in the unknown coefficients. Chapter 8 ends the book, apart from the problems, an Appendix on complex variable theory, and the bibliography, with a somewhat miscellaneous group of topics. An interesting feature to many statisticians will be the references, mostly contained in Chapter 5, to physical applications. These include optics, ocean waves, turbulence and noise; bibliographical references to relevant literature in these fields are also valuable.

To include a full treatment of any general branch of stochastic processes within a deliberately restricted space, so that it is satisfactory both to theoreticians and to practising statisticians, is a rather impossible task. The present monograph has a broad "spectrum" of approach, but perhaps rather a particular concentration at the extremes of mathematical detail on the one side and physical applications at the other. Numerical examples are comparatively few; and general topics little discussed include multivariate (vector) and multidimensional time-series, mixed spectra, and point processes (which have spectral functions in an extended sense). Those interested in this book will also constitute a broad "spectrum", so that the type of information gleaned by readers will have a wide and probably more uniform distribution. Mathematical statisticians should find most of the material of value; econometricians much less, because spectral analysis is not so useful with time-series of rather limited length. M. S. BARTLETT.

2.—*The Mathematical Theory of Sampling*. By Walter A. Hendricks. New Brunswick, N.J.: Scarecrow Press, 1956 (London, Bailey Bros. & Swinfen, 1957). vii, 364 pp. 9″. *67s. 6d.*

This is a sound statistical text-book of roughly intermediate level, with special emphasis on sampling techniques. The introductory portions of the book define the meaning of sampling. There follows a definition of "expected value" as "average of all possible values"; this definition precedes that of probability. Probability is defined as the "expected relative frequency of occurrence of an event in repeated trials". There is a rather unfortunate attempt, on p. 61, to compare this with the "intensity of belief" approach to probability. The sentence "It is certain that (the probability) P lies between zero and unity" is quoted as an example of an "intensity of belief" statement, although it could quite well be interpreted as referring to the fact that all calculated values of P would satisfy the inequality $0 \leqslant P \leqslant 1$.

There follows a fairly thorough discussion of random sampling. The specialist subjects of the book—techniques of drawing samples from real populations and analysing the data so obtained—are then developed in more or less conventional order. There is first a discussion of stratified sampling, followed, in order, by analysis of variance, multistage sampling, choice of sampling units, sampling with unequal probabilities of selection, ratio and regression estimates and control of errors.

The treatment of these techniques is unexceptionable, but the book is unlikely to displace the text-books at present established in this field, at any rate in the United Kingdom. The price is rather high, in view of the unusual method of reproduction which seems to be by some sort of photography of manuscript print. The reviewer was able to find few errors in the text, of which of the appearance of 0·1 instead of 1·0, on p. 265, for a sum of probabilities, was the most noticeable. N. L. JOHNSON.

3.—*Mathematical Economics*. By R. G. D. Allen. London, Macmillan, 1956. xvi, 768 pp. 8¾″. 63*s.*

A single reading of a book like this is hardly an adequate basis for a review. A just appraisal would require a period of months or even years over which Professor Allen's book had been subject to frequent consultation. There can, however, be little doubt that Professor Allen has made an extremely valuable contribution to the literature of economic theory and one that will have an important and lasting effect. He has not tried to pursue new paths of his own, but where there was before only a steep and rough track he has driven—if not a road for everyone—at least a route which the alert and vigorous as well as the expert may follow.

He has accomplished with considerable skill and elegance a work of consolidation and integration which should enable those of his readers with a relatively modest mathematical equipment to appreciate more fully the import and relevance of certain recent advances in economic theory. In executing such a formidable task Professor Allen has made full use of his gift for clear thinking and exposition, coupled with a feeling for mathematical elegance, imperturbable common sense and a sympathetic understanding of the difficulties of the generality of his readers. His efforts will ensure that the new methods of analysis reach a wider public in a balanced presentation which neither underestimates their originality nor exaggerates their importance.

Professor Allen's "aim is to treat in mathematical form those economic problems which students of economics are required to handle and which have some bearing on the facts of economic life". Despite the length of the book and the conciseness of the presentation, the discussion is, broadly speaking, confined to two main classes of problem; the dynamic behaviour of an economy and those decision problems that require a fairly detailed knowledge of linear algebra. In both cases the method of attack is the same. First an explanation is given in mathematical terms, supported by an illuminating economic commentary, of the nature of the problems under consideration. The mathematical tools required for such problems are then developed, special emphasis being attached to the underlying logical unity of superficially dissimilar approaches. Thus equipped the reader is introduced to the simpler economic applications. Gradually further complications on the road to economic realism are examined until the use of a given mathematical technique has been fully exploited.

Three chapters of discussion of simple dynamic models of the Keynesian type are followed by a clear account of complex numbers, leading on to linear differential and difference equations. A knowledge of the mathematical treatment and the economic consequences of lags and delayed response enables the reader to appreciate more fully the implications of the various modern forms of Trade Cycle theory. After that he is ready to explore the relevance of the theory of control systems to problems of economic stabilization.

The second theme is introduced by a discussion of general equilibrium and inter-industry relationships. In the three chapters on linear algebra that follow, Professor Allen is rather less successful in holding the interest of the reader, but this deficiency is largely due to the nature of the subject. Tools in hand once more, with Professor Allen's guidance we grapple with the Theory of Games and the intricacies of linear programming. The author successfully demonstrates the fruitful results which stem from the recognition of the duality existing between these two apparently diverse types of problem. Even in Professor Allen's hands the Theory of Games still appears more an absorbing intellectual toy than a helpful system of thought applicable to realistic economic problems. However, there is a convincing account of the wide class of problems for which the linear programming approach is appropriate. Professor Allen is at pains to emphasize that this method of attack supplements rather than supplants the more conventional economic attitude towards maximization problems.

The final chapter on the aggregation problem provides an indication of the link between the two main themes and in it the difficulty of moving from the specification of individual behaviour to relationships between aggregative variables is clearly demonstrated. Two useful appendices round off the book; in the first there is a brief account of the algebra of operators, while the second consists of hints on the solution of the many exercises with which the main text is liberally besprinkled.

There is no difficulty in arguing that other topics are sufficiently important to have required Professor Allen's attention. In most cases, however, reflection suggests that the frontier of knowledge has hardly yet progressed far enough to leave a settled tract which at the moment merits the intensive cultivation that the author's chosen topics so clearly deserve.

What are the broad general impressions which a reading of this book imprints upon the mind? If economic theorists wish to handle effectively problems of a realistic degree of complexity, they must not shrink from sinking sufficient intellectual capital in learning to use the weapons, which mathematics alone can provide and with which such problems can best be tackled. As Professor Allen emphasizes, the intervening steps of a mathematical argument, as opposed to its premises and conclusions, cannot in general be made intelligible in a purely literary form. It is, however, precisely these parts of an analysis that enable an economist to obtain the requisite insight into the real nature of his problem. Furthermore, the tendency to argue on the basis of implicit and often obscure assumptions leads to much sterile controversy. A mathematical supplement to a literary exposition is more likely to ensure that arguments about the logic of an analysis are not confused and intermingled with attacks on the plausibility of the basic assumptions.

It is also salutary for an economist to realize that the more realistic is the treatment of the facts of economic life, the less possible is the statement of heroic economic generalizations based on theoretical considerations alone. The need for knowledge about orders of magnitude and hence for statistical estimation becomes of ever-increasing importance.

In one respect Professor Allen's generally agreeable restraint is to be regretted. He confines his explanation to theory as it stands without indicating the directions of desirable future progress. It is arguable that both in economic dynamics and in decision theory as expounded in this book the element of chance or uncertainty is largely lacking. Recognition of the inherent uncertainty of the future might well lead to different morals about stability and about the desirability of Government intervention, however benign or well intentioned. As far as individual or institutional behaviour in face of an uncertain future is concerned, there is a clear need for an integration of the economist's and the mathematical statistician's so far independent approaches to the same class of problem. The theorist should recognize that the facts of economic life include varying degrees of unpredictability, and when he has accepted this position he will be able to guide the economic statistician by indicating more clearly the way in which the latter's observations have been generated.

A. D. Roy.

4.—*Chapters from the Classical Theory of Probability.* By C. Jordan. (Hungarian text.) Budapest, Akadémiai Kiadó, 1956. xii, 616 pp. 9¼″.

To readers in Western countries Charles Jordan is known through his *Statistique Mathématique* (Paris, 1927) and through numerous contributions to scientific journals in French, English and German. He is one of those scholars of high achievement, hailing from Hungary, whose number is so high in relation to the small number of their compatriots. The present book is a survey of results of 50 years of research and 30 years of teaching; the manuscript was written 10 years ago. Its status in the world of to-day is precisely expressed in the Preface: "We retain the old definitions and we treat probability from the old point of view, which is now called the classical calculus of probability". This rather apologetic passage is also a reflection of the social and philosophical background in his country.

The book contains 10 chapters in just over 600 pages, the last two being headed "Theory of Errors" and "Gastheory". Thirty-five years ago such a compendium would have been a classic, containing everything the student of probability ought to know. To-day, it is still a classic, but the theory has progressed, and our interest has shifted. This is particularly noticeable where the author confesses to lay stress on applications, and it is precisely these that have changed with the times. Applications to lotteries, poker, and trente-et-quarante, or to probabilities of statements of witnesses, sound rather old-fashioned, and we look for quality control and other industrial applications. Fisher's concept of maximum likelihood is introduced, but its importance is not appreciated. It is also typical, we think, that the only table in the book is one of binomial coefficients.

Little would be gained by describing here in detail the author's skill of exposition, since our readers are not likely to turn to this Hungarian text, however authoritative it be. But an impartial reviewer should mention one aspect which is characteristic of present-day Hungarian publications. They are prepared for publication by a team and readers, editors, referees etc. are all mentioned together with those responsible for printing, and for the quality of the paper. It is unfortunate that such publicity is compatible with shoddy performance. For instance, Galilei and Zermelo are misspelt in the index, there are entries referring, indiscriminately, to Arne and to R. A. Fischer [*sic*], the entries are neither complete nor are the page references always correct. On p. 430 the title of Pólya's well-known paper on random walk in two and three dimensions is misquoted.

Charles Jordan has deserved better from the younger generation, many of whom must have learnt their theory from his lectures, or from his published work.

S. Vajda.

5.—*Statistical Methods in Quality Control.* By Dudley J. Cowden. Englewood Cliffs (N.J.), Prentice-Hall, 1957. xxiv, 727 pp. 9″. $12.

The author points out in his preface that the title of this book is not "Quality Control" but "Statistical Methods in Quality Control", and that administrative organization for quality control is therefore not covered. Nevertheless the book is a fat one and, within its chosen field, it is comprehensive.

The book starts with a number of theoretical chapters on summarization of data; probability; binomial, multinomial and normal distributions; estimation; tests of hypotheses and confidence limits; analysis of variance; and tests of homogeneity. Further theoretical chapters, dealing with the Poisson and hypergeometric distributions; frequency curves; regression and covariance; and sequential methods, are deferred until later in the book, each appearing when the methods are needed.

The control chart (apart from a brief reference in chapter 1) first appears in chapter 12 and is then considered in detail, but always as a means to an end rather than, as sometimes occurs, as an end in itself. Charts are considered from the point of view of analysis of historical data as well as during production. The customary forms of chart, i.e. for mean and standard deviation, for mean and range, for defectives and for number of defects, are all discussed as well as many less usual forms e.g. for coefficient of variation, for extreme values, for t and for χ^2. Control charts for non-normal distributions, sloping control lines, and the economics of control charts each receive a chapter.

The final section of the book is devoted to acceptance sampling techniques using both "attributes" and "variables" plans and explaining in detail the calculations involved in the derivation and use of single, double, multiple and sequential sampling.

The mathematical ability demanded of the reader is not heavy; the understanding of the meaning of an integral sign is the most that is necessary for the main text. A few more mathematical sections form short appendices at the ends of the appropriate chapters, but the book can be read with profit even if these appendices are omitted.

Frequent practical and numerical examples, and a good selection of diagrams, are given. Some of the diagrams appear more ingenious than genuinely helpful (e.g. Fig. 2.4—a geometric interpretation of the standard deviation), others, which appear complicated at first, become more and more enlightening as they are studied (e.g. Fig. 18.7—O.C. surface for control chart for means).

Not everyone will agree with all the terminology used. "Process Control" is generally accepted as a term covering the field of control chart techniques, but to use "Product Control" to cover modified limit charts and acceptance sampling seems less acceptable. Also to be regretted is the use of the word "variation", until now available to imply the general concept without specifying any particular way of measuring it, to mean "sum of squares".

A curious feature is that for testing for homogeneity of variances the author gives the Neyman and Pearson likelihood ratio criterion, as well as Bartlett's test which has, surely, replaced it. Furthermore, in the rest of the book he always uses the older test in practical examples.

The approach to the logic of acceptance sampling seems unnecessarily complicated. It is possible to examine criteria for acceptance or rejection and determine how they will function when the submitted batches have any given distribution of quality, and having chosen a criterion in this way to use it in practice so that determination of the sampling results leads straight to acceptance or rejection. Professor Cowden inserts a third step in making the sampling results lead to an inference on the quality of the batch which in turn leads to acceptance or rejection. Thus he always insists that he is accepting or rejecting an hypothesis as well as accepting or rejecting the batch. This not only complicates the chain of reasoning, but also introduces the logical difficulties about inference from which this subject could be free. Even Sir Ronald Fisher casts "no contempt on acceptance procedures" and is content that batches of material should be accepted, even if hypotheses should not.

Apart from rather a large number of misprints, some of which could be misleading, the standard of accuracy is quite high. There are, however, a few major errors, for example (i) the addition theorem of probabilities (p. 29) is muddled by writing "the probability of . . . an item with a major defect" when what is meant is "the probability of . . . an item with *only* a major defect"; (ii) two separate formulae are given for $\Gamma(X)$ (p. 63), the first to be used if X is an even integer, the second if X is an odd integer. This should be the first if X is an integer, the second if $X + \frac{1}{2}$ is an integer; (iii) on pages 94 and 95 a mistake is made analogous to the adding of likelihoods; (iv) in writing of making a logarithmic transformation of a non-normal distribution and plotting control charts of the logarithms instead of the original observations (p. 331) the statement is made "Although the range in units of the original data cannot be negative, it is possible to have a negative range of logs. As an extreme example, if the range of a sample in units of the original data is zero, the range of the logarithms is minus infinity." This, of course, is false; no range can ever be negative. The range of the logs is not the same thing as the log of the range!

These criticisms are few compared with the merits of the book, and with the skill and good sense which are apparent throughout. There is a useful range of tables at the end, and a glossary of symbols.

I. D. HILL.

6.—*Electronic Data Processing for Business and Industry*. By Richard G. Canning. New York, Wiley, 1956 (London, Chapman & Hall Ltd.). xii, 332 pp. 9″. 50*s*.

This book must be severely criticized for the inordinate degree of repetition by which 60 pages of material are expanded into a book of 300 pages. Having said this, the reader must not think that the book has no value. Once the discursive style ceases to irritate one the book does give a sound and sensible approach to the data processing problem. The author's fundamental thesis is that the problem of data processing is the problem of production control, of the flow of *information* rather than is usual, the flow of product. He says the first stage in the introduction of machinery is the investigation to determine what the actual flow of information is in the present situation and at what points this could be improved in quantity, quality and timing, and he discusses the ideas of central processing as opposed to de-centralized processing and the relative merits of delayed central processing against real time processing (terms which do not need the long discussion given to understand the difference). The final conclusion, as to which form of processing to use, is shown very clearly to depend upon the type of equipment that it is reasonable to instal, and this hinges on the problem of sorting. For real time processing an instantaneous access store is the ideal requirement to give individual random sorting facilities, but with delayed central processing techniques systematic sorting of very large batches of information can be used, which eases the storage and sorting problems simultaneously.

A chapter is devoted to a general description of the current available data processing machines, which is reproduced later with further detailed descriptions of the equipment characteristics (another example of duplication of material).

This book is written for managers and so it is perfectly appropriate that a chapter is devoted to showing how typical simple clerical operations are programmed for these machines. This is followed by further consideration of how the information flow system is studied, and how the interaction of the requirements of this system with the available equipment influences a preliminary design of an information control process. This is followed by a further chapter in which this preliminary design is taken, for every typical instance, into more detail. There then follows a chapter on the role of Operational Research in this problem. Mr. Canning does not believe that Operational Research has any part to play in the installation of data processing equipment. This is due to a too narrow interpretation of the term Operational Research, since the initial problem of studying and understanding the information flow system is itself a piece of Operational Research. The installation of equipment without studying the system is, as Canning emphasized, a very dangerous step, as there is on many occasions a very delicate balance between the return obtainable in efficiency by increasing the size and capabilities of the equipment installed.

The book emphasizes that with the increased capacity to handle information, there is a great danger that more information will be handled than at present, and this increased quantity may make managerial decisions harder rather than easier. The two purposes of this control of information flow are (i) to aid the production control of the real product or products and (ii) to give managers a simple overall picture of how production control is functioning. They really wish to know when the system is not functioning properly and special attention should be paid and is paid in this book to arranging that exceptional situations and only these are brought out of the data processing system for the attention of managers.

The book attempts to show how much more complicated the application of data processing machines to business and industry is than the application of computers to scientific activity, but having a little knowledge in both fields the reviewer remains unconvinced.

K. D. TOCHER.

7.—*Introduction to Operations Research.* By C. West Churchman, Russell L. Ackoff and E. Leonard Arnoff. New York, Wiley, 1957 (London, Chapman & Hall). x, 645 pp. 9″. 96*s*.

This book has grown out of lecture material for the "Short Course in Operations Research" offered by Case Institute of Technology. The initial versions of many of the chapters were prepared by other people (to whom acknowledgment is made in the preface) but Churchman, Ackoff and Arnoff have accepted the responsibility of revising this material in adding it to their own writing in an effort to obtain a connected and consistent text. In this aim they have been astonishingly successful; no one reading the book without first reading the preface would guess that it had been prepared in this way.

There is a division into ten parts, each containing one, two, or three chapters. The parts are headed Introduction; The Problem; The Model; Inventory Models; Allocation Models; Waiting-Time Models; Replacement Models; Competitive Models; Testing, Control, and Implementation; and Administration of Operations Research. These main headings include all the well-known techniques, for example, linear programming comes within allocation models, queueing theory within waiting-time models, theory of games within competitive models.

The discussion is entirely in terms of industrial and civilian applications. Military applications are not considered.

An important feature is the generous amount of space devoted to case histories, many of which are reported in considerable detail. These case histories are well chosen to illustrate the nature of the subject. Arithmetical examples are worked out fully and lucidly, and the diagrams are clear and helpful. There is a comprehensive bibliography at the end of each chapter.

The level of mathematical ability required is not excessive, but varies a little from chapter to chapter. This is one of the few points where the text seems to be slightly inconsistent; is it, for example, necessary to explain the use of the summation symbol Σ on pages 302 and 303, after using it many times earlier in the book?

The authors' preface states that "It should be emphasized that this book is an introduction. It is not intended to be a reference work for experienced practitioners". However, there will be few practitioners who will not wish to have a copy available.

I. D. HILL.

8.—*A Dictionary of Statistical Terms*, 1957. By M. G. Kendall and W. R. Buckland. Prepared for the International Statistical Institute. Edinburgh and London, Oliver & Boyd, 1957. ix, 493 pp. 8¾″. 25*s*.

It is a solemn thought that statistics is so developed that a dictionary of over 300 pages is necessary to define and describe the special terms used, but such is the situation. The dictionary under review was prepared because the International Statistical Institute felt the need for something that would define and standardize statistical terms used in many languages. It was decided to have prepared a dictionary with definitions and explanations in one language—English—and to publish with it a glossary of equivalent terms in several languages—English, French, German, Italian and Spanish. English-speaking people are lucky that the result of this international project is so immediately and directly useful to them. The dictionary has been prepared by Professor Kendall and Dr. Buckland with the acknowledged help of many colleagues in the statistical world, and the glossaries have been provided by statisticians in countries speaking the various languages. The result is a very fine production, well arranged and convenient to use, handsomely printed, and extraordinarily low in price.

Professor Kendall, in a paper given at the Rome conference of the International Statistical Institute in 1953, discussed some of the issues that had to be decided in preparing the dictionary; and impressed the reviewer with the reasonableness of his decisions. In order to prevent the dictionary from growing inordinately, the authors have been fairly strict in selecting terms for inclusion and they have omitted those belonging more to some subject of application than to statistics. Thus, *national income*, *cost-of-living*, and *bio-assay* are omitted (but *tolerance limits*, which is an engineering term that happens to be used in quality control, is included—doubtless there is room for argument over borderline cases). Terms and usages are included only when they are in common use, or are backed by the authority of a distinguished writer, or seem to the authors to fill a gap and likely to come into common use. Terms and usages that have recently become obsolete, or are becoming so, are included with a note to that effect—*probable error*, for example, is "an older measure of sampling variability now almost superseded . . . ". The authors have allowed themselves to express opinions, although with restraint—the use of the word *aleatory* is noted, with the remark "There is no need for this", and *rational trend* is "a term which is, or ought to be, obsolescent" (so, perhaps, is the term *rational sub-group*, which occurs in the literature of quality control but finds no place in the dictionary). In according the authors the right to express such opinions, one applauds their liberalmindedness in not exercising any censorship over terms of which they disapprove. For them to have exercised such censorship, as was suggested to them, would have been quite wrong.

The definitions and descriptions are brief, but understandable by people who are reasonably knowledgeable in the subject. Where terms are used only vaguely (e.g. *variable* and *variate*) or there are several usages (e.g. of *Monte-Carlo Method*) the situation is set out in full. The authors have also done what they could towards attributing terms and ideas to the people who first used them, but they do not claim to be entirely accurate or comprehensive. All this is reasonable and admirable.

But this dictionary does not represent the authors' last word and doubtless they will modify it in due time in the light of experience. I feel, for example, that the entries related to quality control could be improved. The term *quality control* tends to be used in the U.S.A. to cover almost every kind of application of statistical methods to manufacturing industry; in the U.K. the meaning is usually restricted to the statistical use of inspection data, and *statistical quality control* is sometimes regarded as the antithesis of 100 per cent. inspection. The variety of these usages is not disclosed in the dictionary entry for *quality control*. *Modified control limits* is missing, and terms like *consumer's risk* are described as arising in quality control whereas they would be better described as arising more specifically in acceptance sampling, especially in view of the restricted definition of quality control adopted. These matters are arguable—they are mentioned here merely to show that statisticians will probably find in this dictionary much to argue about. But lexicography must start somewhere and at some time. This dictionary has been well considered and carefully done, and provides an excellent start. It will hold the field for a very long time to come.

L. H. C. TIPPETT.

9.—*A Revision of Demand Theory*. By J. R. Hicks. Oxford, Clarendon Press, 1956. ix, 196 pp. $7\frac{1}{2}''$. 18*s*.

In this book, a clear and thorough exposition is given of the static theory of consumers' choice. The theory presented is a completely deterministic one, of the kind often used in the discussion of index number problems. The method of presentation is intended to suit the needs of economists, so that although the development is almost entirely deductive, few symbols are used, and no mathematical technique is presupposed.

Professor Hicks early emphasizes that he has been influenced by the "econometric approach". By this he seems to mean the approach to economic theory in which the stress is on verifiability, on the "objective consequences of economic events". In the preface it is made clear that the book will be concerned with "the theory itself" and not with "the statistical application of the theory".

In Part I, the foundations of the theory are laid. The hypothesis of cardinal utility is briefly considered, but is rejected as being unsuitable for the author's particular purposes. Then the basic postulates are introduced. The consumer is supposed to choose between two bundles of goods. He will either always choose one, or remain indifferent. By comparing different pairs of bundles in this way an ordering of the bundles is set up. In order to obtain consistency, a transitivity postulate is introduced, and it is also assumed that larger bundles of good are preferred to smaller ones. If there is never indifference between any pair then the ordering is called "strong". If there is sometimes indifference the ordering is "weak". The author attaches considerable importance to the distinction between these two types of ordering.

In a typical economic case the consumer is faced with a whole range of possibilities, only limited by his income and relative prices. His choice is consistent with his ordering. By comparing situations in which the consumer has different incomes, and where relative prices differ also, and determining the bundle of commodities chosen in each situation, the quantity of any particular commodity chosen can be determined as a function of income and prices: in other words the demand function is determined for that commodity.

In Part II of the book the demand for a single commodity is discussed for the case in which its own price can vary, but the prices of all the others are fixed. The author emphasizes that this is an expository device. Then the division of the effect of a price change into an income effect and a substitution effect is defined, and the existence of inferior goods is mentioned. The (income compensated) demand curve can be approached from two symmetrical points of view. From one, it gives the quantity of the goods which a consumer will buy when faced with a particular price. From the other, roughly speaking, it gives the maximum price he will pay if he can buy a given quantity. This duality is developed systematically, and is one of the best parts of the exposition.

The "general theory of demand" is reserved for Part III, and now the prices of several goods are allowed to vary. In this part there is an extensive discussion of substitutability and complementarity between goods. Mostly this discussion is definitional, complements and substitutes being defined from the properties of the demand functions only, but in chapter 17 an interesting alternative point of view is mentioned. Complements (tyres and petrol for example) arise from the joint technical need for them in obtaining a particular end. In postulating such an end, one is looking beyond the preference ordering to the determinants of such an ordering. This approach surely corresponds rather more to the way in which statisticians think of complements in deciding what variables to try out in a demand equation. The more usual theory gives little help in such a selection. Also, in this part of the book, the "index number theorem" on the relation of the Lasperne and Paasche indices (L and P) is developed, and an exceptional but quite possible case for which $P > L$ is given, even supposing there is no change in tastes. From the "general law of demand" an instructive example is given for which the price and quantity indices of a group of commodities would move in the same direction.

The book contains some difficult reading, but those who manage to master it will have a considerable insight into the static theory of demand. On the other hand such static theory has not proved flexible enough to explain much time series data. If one starts with the demand equation direct, instead of going back to the preference order hypothesis, one has much greater flexibility, and much more ease of generalization. Professor Hicks acknowledges (on page 16) the necessity for bringing in more than current prices and incomes to explain the demand for a commodity. He divides the useful explanatory variables into three classes. These are, briefly, "non-economic" explanations (e.g. age), current price and income explanations, and thirdly, lagged price explanations. He claims that his approach "separates off" the current price and income effects from the others. But variations in the other variables will usually change the preference ordering, so that price and income effects cannot be isolated in this way, especially if the remaining variables differ for each period of observation.

There are some technical slips in the discussion of ordering. The difference between weak and strong ordering has nothing to do with the mathematical continuity of the quantities of commodities. Both types of ordering can occur in either the discrete or continuous case. In the case of weak ordering, as Wold points out in his book "Demand Analysis", one requires an extra postulate to ensure that each indifference curve is sufficiently extensive, so that the arguments about equivalent real income can be carried through. Moreover it is often possible to define a perfectly adequate notion of "almost" equivalent real income, even in the case of strong ordering, if mathematical continuity is somewhere assumed. There are cases, discussed by Georgescu-Roegen, where this is not possible, and the most useful set of postulates would be those designed to exclude these cases. Weak ordering, with the addition of Wold's extension postulate, can easily be converted to the type of strong ordering where they are excluded. This can be done by inserting an additional ordering on each indifference curve, while the ordering of the indifference curves themselves is kept unchanged. Correction of such slips, though altering the emphasis of the chapters on ordering, would not affect the argument elsewhere in the book.

C. B. WINSTEN.

10.—*Bibliography of Index Numbers.* The Hague, International Statistical Institute, 1956. 40 pp. 11¼″. 4*s.* 8*d.*

In his masterly introduction to this Bibliography, Professor R. G. D. Allen remarks "it would seem that the theory of economic aggregation has fallen between two stools; the economist has left it to the statistician and the statistician is not enough of an economist to appreciate the real needs". He also makes the point that there is "no adequate text-book (of recent date) in existence, in any well-known language, on the theory and practice of index numbers". It is evident, however, that whatever may be the deficiencies of the literature on index numbers, there is no lack of it. The names of some 340 authors, with the titles of their 600 or so publications, in eleven languages, are shown in chronological order from the year 1707 to 1954 inclusive—in a form (with an index) which will certainly be helpful to teachers of economics and statistics throughout the world.

There can be little doubt that few people have a better claim to have directed this research into the Bibliography of Index Numbers than Professor Allen. He has made several contributions to the literature, including three since the war on *The Economic Theory of Index Numbers* (1949), *Index Numbers of Retail Prices* (1952) and *International Trade Statistics* (jointly with J. E. Eley in 1953). Nor could any reviewer of this Bibliography—short of reading scores of the publications listed—do better than draw liberally on Professor Allen's Introduction—particularly his attempted division of the six hundred entries into the following four significant periods of appearance:

First (ignoring some of the very early publications) there was the *second half of the nineteenth century*—when "the literature was dominated by a few famous names and was much more economic than statistical". The main theme in that period was the measurement of the "general level of prices" and the "value of the currency". The period ends "very appropriately, and at the turn of the century, with Walsh's book on *The Measurement of General Exchange Value* (1901)".

Secondly, there was *the period* 1902–24—"when statisticians as well as economists began to interest themselves in index numbers". The interest was, however, mainly mathematical, and its most extreme manifestation was Irving Fisher's *The Making of Index Numbers* (1922). "This book, with its concentration on formal tests and on a single-minded search for the 'ideal' index, marked the end of an epoch. Never again was anyone to make such a dogmatic statement as: 'The purpose to which an index number is put does not affect the choice of formula' (Irving Fisher, chapter heading, page 229)".

Thirdly, there was *the period* 1925–44—"when the particular purposes, uses and interpretations of index numbers were the main emphasis . . . Instead of an index of prices in general—in which the weighting was incidental—attention was directed to the price component in the changes of a particular aggregate, e.g. to the changing cost of a given 'basket' of goods and services": . . . "The period ends, again quite appropriate, with a path-breaking and theoretical—empirical paper by Geary, concerned with the economic basis of an index of the movement of the net volume of output (1944)".

Finally, there was the very fruitful *post-war period from* 1945 *onwards*—"when index numbers in great variety were designed and discussed. . . . During this decade there were as many entries as in the whole of the preceding century". Among them were a large number in the English language, including *The Measurement of Production Movements* by Carter, Reddaway and Stone (Cambridge University Press 1948); two papers on aspects of comparative "Productivity" by the late L. Rostas (N.I.S.E.R. occasional papers 11 and 13, 1948); "Movements in the Real Product of the United Kingdom" by W. B. Reddaway (*J.R.Statist. Soc.* 1950); "Indices of Wholesale Prices" by J. Stafford (*J.R.Statist. Soc.* 1951); "System of Aggregate Index Numbers and Their Compatibility" by R. Stone and S. J. Prais (*Econ. J.* 1952) and *Price Indices and Quality Changes* by E. von Hofsten (Stockholm, 1952). Many other papers, articles and books, appeared in this post-war decade, and among the authors were: Adams, Barna, Bowley, Cobb, Humes, Hodgman, Leser, Nicholson, Peterson, Roy, Rutherford, Tippett, and last but not least Winkler, who produced, in 1954, an historical survey of certain developments in the field and can boast the last entry in the Bibliography, which was completed in 1955 and printed in 1956.

Even the briefest inspection of the list of subjects in this Bibliography emphasizes the very many different interests and angles of approach of the authors. Index numbers should be regarded as tools in the hands of economists and statisticians and it is clear that many have been designed in recent years for a wide variety of purposes in vastly different conditions—from country to country—sometimes with very inadequate base-data. It is against this many-sided background that one must judge the introductory suggestion that there has been a "disturbing . . . lack of integration of index number theory and practice into the general body of economic doctrine or statistical technique". Even though it be admitted that the different purposes and problems of index number compilers are legion—and that the quality of their data is very variable—it can also surely be admitted that very often so-called new problems and *ad hoc* solutions would be better recognized as old ones (dressed-up) if only there were accessible some integrated authoritative text-book on the basic doctrines and techniques. How true this suggestion is may be arguable, but certainly the historical stage reached seems to call for some synthesis. Perhaps one may express the hope, nevertheless, that this will not mean the production of a Frankenstein-like volume (as frightful in its content as in its size) purporting to set out the final "truth" about this hydra-headed index number monster; such a volume might deter ordinary folk from stepping out with little monographs of immediate value like so many listed in this Bibliography.

Looking through the main recent works in this Bibliography it is certain that contributions have in the main been made by economists. So much so that Professor Allen is driven to remark that it is "remarkable and regrettable, how few distinguished statisticians are to be found—in a decade of unprecadented developments in statistical techniques—contributing to the advancement of the theory and practice of index numbers". I am not at all sure that it is so regrettable. It seems undesirable that any statistician—*qua* statistician—should write a book about the theory and practice of index numbers, since the practice is closely connected with all the real problems of economic life with which the figures are associated. Surely it is the joint fusion of statistical training and experience with scientific economic thought, applied to practical problems, that has made possible all the more useful contributions to this subject. The more fusions of this kind that take place, the better for the literature. The broad mantle of "economics" certainly needs to be recognized as embracing the relevant applied statistics and it is bound to gain in the process.

We are indebted to Dr. William R. Buckland of the London School of Economics for what must have been an extremely arduous task in compiling this comprehensive Bibliography; it cannot be suggested that the price (4*s.* 8*d.*) is excessive. R. H. S. PHILLIPS.

11.—*The Interpretation of Accounts.* By F. Sewell Bray. London, Oxford University Press, 1957. x, 215 pp. 8½". 30*s.*

This book brings together the lectures given by the author as Stamp-Martin Professor of Accounting from his inaugural lecture in 1953 up to the end of 1955. Essentially the lectures are designed to so emphasize the main purposes of accounting and the orderly system of presentation best adapted to these purposes, that accounts constructed on the basis of the author's precepts shall in fact be capable of proper and complete interpretation.

One of the advantages to a reader of the lecture form of presentation is the fact that phraseology designed for oral delivery is often better adapted to the average speed of mental assimilation. At least good lectures are so adapted and by this test the Stamp-Martin lectures are good. They are also pleasant to read, as they were doubtless pleasant to listen to, because Professor Bray has a marked regard for humanity as well as for ciphers and has a healthy disrespect for traditional practices that cannot be defended on better grounds than their antiquity.

For the statistician the interest of these lectures lies in that broad area of common ground represented by the need for the sufficient analysis of causal factors in economic measurements and the need for logical presentation of numerical data of economic activity. Both the statistician and the accountant depend upon precise classification; and both face the inevitable truth that even the best techniques cannot improve poor data.

The first part of the book deals with the formal principles of public company accounting and to what Professor Bray calls "Accounting dynamics"—the measurement of changes in time in the economics of individual organizations and their aggregates. The author searches economic theory critically for his basic concepts, i.e., the structure, form and measurement of economic activity, and above all, the discernible relationships in economic behaviour. The short period operating accounts designed for the study of change relationships bring the accountant and the industrial statistician close together, because in their interpretation some assessment has to be made of that part of the experience which is repetitive, otherwise there is no basis for prediction and no assistance to decision.

The second part of the book contains lectures on the nature and purpose of direct taxation, auditing theory and social accounting. The latter is the substance of a paper read to the Liverpool Economic and Statistical Society in 1955 and is a lucid account of the information, in which we all surely have a common interest, required to indicate what is happening and is likely to happen to our national prosperity. B. BENJAMIN.

12.—*Consumer Instalment Credit*. By the Board of Governors of the Federal Reserve System in association with the National Bureau of Economic Research and George D. Bailey. Parts I–IV (6 Vols.). 1957. Washington, United States Government Printing Office.

At the direction of the President of the United States of America, the Board of Governors of the Federal Reserve System was requested to undertake a broad study of the role of consumer instalment credit in a growing economy and to review the arguments for and against the renewal in some form of governmental authority to regulate credit in this field. The Federal Reserve System set to work on the project, using its own research staff and the services of a special consultant and employing the National Bureau of Economic Research to mobilize the knowledge and experience of the universities and research institutions in all parts of the country. The result of these activities is a report in six volumes presenting the government with a vast analytical and factual background to guide it in its political decisions.

The first two volumes which together constitute Part I of the study contain the contributions of the research staff of the Federal Reserve Board in the form of essays, each of which deals with a special aspect of consumer instalment credit. Apart from the purely descriptive studies which are mostly supported by generous amounts of statistical data and cover a lot of ground in considerable detail, there are some interesting analyses of the effects of consumer credit on aggregate demand and on economic stability. A concluding chapter summarizes some of the main arguments for and against governmental regulation.

Volumes three and four, constituting Part II of the study, contain the contributions mobilized by the National Bureau which itself has previously sponsored a good deal of work in this field. In volume three the several papers cover such theoretical topics as the relationship between consumer credit and economic fluctuations and between consumer credit and long run economic growth. Since consumer credit cannot be regarded as a completely independent entity, several papers examine its connection with the rest of the financial system with the particular object of discovering whether changes in general monetary policy have or have not a strong and immediate impact on consumer credit. On the basis of small sample surveys a paper dealing with reasons for personal saving and attitudes towards consumer debt represents an interesting contribution on the psychological aspect of the subject. The fourth volume is a record of vigorous controversy about the desirability and effectiveness of the traditional controls and the possibility of certain untried alternative policies for dealing with the special problems created by consumer credit.

The role of the Federal Reserve Board's special consultant, Mr. G. D. Bailey, was to mobilize the views of businessmen having direct commercial interests in consumer credit and his report is contained in the fifth volume constituting Part III in the series. A majority opinion clearly emerges; most manufacturers, dealers and financiers concerned are not in favour of the regulation of instalment credit in peace time. Among the minority favouring some form of control motives are mixed, but the desire to have protection against the sharper practices of competitors is certainly one of them. One or two representative consumer groups were also consulted. Their replies included comment on the need for legislation against bad practices and abuses in the credit agencies, but on the main issue the consumers join with the majority of commercial interests in advocating freedom from consumer credit regulation.

The sixth volume (Part IV) dealing with the financing of new car purchases based on a sample survey of lenders, credit buyers and cash buyers is not available at the time of writing this review.

The arguments for and against standby or actual controls over consumer credit range very widely; can the government know the exact amount of pressure which should be put on in any given situation; can it correctly time its actions; can it enforce sufficient observance of its regulations to make its actions effective? These are only a few of the questions discussed. A wider range of discussion centres round the problem of discrimination. The restrictive regulation of consumer credit as an isolated action in the sphere of monetary policy clearly discriminates against certain classes of consumers (lower income groups and younger married couples), certain classes of traders and the manufacturers of consumers' durables. As the duty of the government as guardian of the economic health of the country is to protect the overall economic situation, supporters of the discrimination argument claim that more general measures should be employed. Some argue that general credit policies would be adequate for this purpose. Others argue that they would not be adequate, or alternatively that where inflationary pressures are mainly caused by the increased purchase of consumers' durables on credit, some restraint on the further expansion of consumer credit is justified and that furthermore it might obviate the need for a more drastic use of general credit restriction at a later date. One contributer points out that some discrimination is the inevitable consequence of any form of control while another seems to think that the hardship arising out of discriminatory practices tends to be overestimated.

The study does not recommend any course of action but is devoted entirely to bringing together the information available in a purely objective manner, thus keeping within the terms of reference. While this may meet perfectly the requirements of the President's Council of Economic Advisers to whom the study is addressed, it may not be altogether satisfying to the general reader, who may feel that objectivity has been carried too far and that the report lacks breadth commensurate with its length. The first five volumes amount to just over 1,600 pages, equalling the number of pages contained in Professor Samuelson's *Economics* and Marshall's *Principles* taken together. In a work of this bulk the reader feels the need for a few signposts. It is, for example, difficult to distinguish what is new from what is old, what is more important from what is less important and which facts are more soundly based (statistically and otherwise) and which are less soundly based. In presenting fairly and impartially all the evidence and all the opinions and in leaving all exercise of critical discrimination to the reader, the weight of knowledge behind the compilation of the essays and the wealth of experience gained in collating them is partly wasted. For the general reader, at any rate, either a shorter or a more positive study might have been more satisfying.

T. M. Ridley.

13.—*Problems in the International Comparison of Economic Accounts.* N.B.E.R. Studies in Income and Wealth, Vol. 20. By the Conference on Research in Income and Wealth, Princeton, University Press, 1957 (London, Oxford University Press). x, 404 pp. 9″. $8.

This book contains six papers read at a session of the American Conference of Research in Income and Wealth held in October 1954, together with comments on the papers by leading experts who participated in the conference. A useful summary of some of the main points of interest brought out by the various authors and commentators is given in an introduction by John W. Kendrick.

The first and longest paper is by Morris A. Copeland, who sets out in some detail an "extremely tentative sketch" of a general purpose system of national accounts which gives adequate emphasis to input-output, money flows and balance sheet accounts as well as to the normal social accounts. His starting point is the United Nations report *A System of National Accounts and Supporting Tables* (Studies in methods series F, No. 2, 1953) which he modifies in a number of ways, partly to make it easier for non-industrialized countries to produce. One of his suggestions is that as a short-term objective the items "national income" and "personal saving" should be dropped in favour of "gross domestic product" and "net personal lending", as too much attention might be placed by some countries on obtaining these totals at the expense of developing a full set of national accounts on the lines he proposes.

The chapter is quite complicated in parts and will probably be of little interest to anyone except the expert on national income who is familiar with the O.E.E.C. and the United Nations standardized systems. The reviewer sympathises with George Jaszi who tentatively concludes that he sees no overwhelming advantage in Copeland's system over the United Nations system. The much simpler British system set out in the National Income Blue Books appears to be preferable.

Two papers by Gerhard Colm and Marilyn Young discuss certain conceptual problems in the Government sector and examine and explain the differences between the United States Budget figures of revenue and expenditure on the one hand and the national income statistics of Government revenue and expenditure on the other.

Among many conceptual problems discussed are whether the services rendered by government-owned assets, like roads and buildings, should be reckoned in estimating government expenditure and the national income; the treatment of indirect taxes in the calculation of the national income; the classification of government expenditure into current and capital and by purpose. It would appear that the United Kingdom National Income Blue Book provides much more detailed information about the character of Government expenditure than does the corresponding United States publication.

The fourth paper in the book is one by Herbert B. Woolley, who is engaged in compiling a matrix of international transactions on a from-whom-to-whom basis and has prepared a "trial run" matrix for merchandise and transportation transactions for the year 1951. In his paper

Woolley discusses the usefulness of constructing such a matrix and the difficulties encountered.

The fifth paper is rather a complicated one written by Dorothy S. Brady and Abner Hurwitz which deals with problems concerned with measuring the comparative purchasing power of national currencies. Among the subjects discussed are attempts to break away from the traditional "basket" method of weighting retail price indices. The authors make a number of suggestions for calculating purchasing power equivalents without having to make price observations at all. For example, similarity in the pattern of consumers' expenditure is given as one test.

In the final chapter, Irving B. Kravis attempts to find a concept of economic activity that would be useful in comparing national incomes in countries with widely differing social and economic institutions. He finds that it is not adequate for this purpose to base national income estimates on a common range of products by including non-market products in the non-industrialized countries that have market counterparts in the industrialized countries, because there is also an impressive list of non-market activities in the industrialized countries. He rejects Frankel's thesis and concludes that significant income comparisons can be made between two very different types of economies providing economic activity in both countries is sufficiently widely defined.

L. S. BERMAN.

14.—*The Measurement and Behavior of Unemployment*. N.B.E.R. Special Conference, Series 8. Princeton, University Press. 1957. London, Oxford University Press. x, 605 pp. 9″. 60*s*.

This is a report of a Conference of the Universities-National Bureau Committee for Economic Research held at Princeton in 1954. It contains eleven papers in nine of which a comprehensive exploration is made of the statistics of unemployment in the U.S.A. These are followed by one relating to the Soviet Union and another reviewing international comparisons of unemployment statistics. The series opens with a paper on the meaning and measurement of full employment. The author examines various meanings which may be attached to full employment but fails to find one best definition. He then discusses various types of measurements, with illustrations from the figures of the U.S.A. and Great Britain. He examines, for example, the effects of assuming that full employment exists when unemployment is at a minimum determined historically. He argues the desirability or otherwise of regarding as unemployment time not worked by part-timers and of time lost through temporary stoppages. The possibility of distinguishing between frictional and other types of unemployment is discussed. The paper ends with an examination of the relationship between full employment and inflation. The second paper relates to the current unemployment statistics of the Census Bureau. Its main purpose is to ascertain to what extent they are adequate and how far the concepts and definitions are reasonable. The statistics record the number of unemployed persons who did no work for pay or profit in a specified week. To these are added certain "inactive workers". This concept is examined and criticized. The process of measurement by sample is described and its reliability is considered. In conclusion the author recommends some changes in the statistics, to include a measure of those who worked less than full time. A paper on Unemployment Data from the Employment Security Program compares these figures with those described in the preceding paper. The author concludes that the chief advantage of the Security data is that they provide figures for local labour market areas. On the other hand they have disadvantages in that they have a limited coverage which varies in different States and it is not possible to define the figures in terms which have economic significance. A paper on the Meaning and Measurement of Partial and Disguised Unemployment discusses various definitions and examines the problems in the U.S.A., particularly in agriculture. In agriculture, owing to seasonable variations in activity, the normal measures of unemployment in a specified week are not adequate and other methods are considered.

The next paper in the report deals with the Secondary Labour Force and the Measure of Unemployment. Its thesis is that differentiation in labour force statistics between those who are continuing or regular members and those who are temporary or secondary members may be feasible. Such differentiation would contribute to the understanding of employment and unemployment fluctuations. The extent to which it is possible is discussed at some length and available statistics are summarized. The sixth paper presents a new series of annual estimates of unemployment in the U.S.A. from 1900 to 1954. The series is compared with partial series previously available. In general the different series show the same broad tendencies though the actual measurements vary, owing largely to differences in the categories of the persons included. The subject of the next paper is Differential Unemployment and Characteristics of the Unemployed in the U.S.A., 1940 to 1954. It includes an examination of analyses by age, sex, colour, marital status, industry and occupation. A longer and more detailed analysis of unemployment by industry is dealt with in another paper. The main conclusion of the author is that the two most common measures of unemployment by industry are the industrial distribution of the numbers unemployed and comparisons of the rates of unemployment in different industries. He concludes that the former is not of primary importance but that the latter is useful for comparison over time. A further section of the paper examines the movement of labour between the main industry groups between 1949 and 1952 and concludes that unemployment played a minor role in the movement of labour between the construction, manufacturing and trade groups.. Unemployment by Locality and Industry is the next subject for treatment. Much of the early part of the paper is devoted to a discussion of the meaning of unemployment in relation to local and industrial figures. The area technique is to count those covered by unemployment insurance and to derive estimates of the unemployed who are not covered by benefit. Analyses are compiled by industry and by occupation. It is pointed out that local rates of unemployment may differ widely from national rates owing to wide differences in the industrial distribution of the workers.

The purpose of a paper on Soviet Union Labor Force Materials is to discuss the impact of Soviet planning and economic expansion on the supply of labour and to examine trends in the percentage of full time participation in the labour force. In the official view no unemployment exists. The review starts with details revealed by the 1926 census. when 87·3 per cent. of the civilian labour force, mainly in agriculture, were self-employed or unpaid family workers. From scanty information since that date it is found that the number of wage and salary workers had increased by about 1930 and the number of self-employed in agriculture had declined owing to collectivization. Subsequent information is too meagre to trace the position further. In the final paper the material available as to unemployment from 1900 to 1950 in ten countries is assembled. Differences in definition and in the basis of the figures render comparisons difficult and the paper is largely devoted to these differences. In addition to a general review there are appendices quoting and describing the figures for the period in each of nine of the countries.

The whole report presents a useful summary of the possible concepts and definitions of employment and unemployment and of the extent to which they can be measured in the U.S.A. from available material. The paper on international comparisons is a concise factual record of the figures over long periods in many countries. The authors include representatives of Universities, the Bureau of the Census, the Department of Agriculture, the Bureau of the Budget, and the Bureau of Employment Security. R. B. AINSWORTH.

15.—*Rural Depopulation in England and Wales:* 1851–1951. By John Saville. Dartington Hall Studies in Rural Sociology. London, Routledge, 1957. xvi, 253 pp. 8½″, 28*s*.

Comparatively little is known on the subject of internal migration in this country. Mr. Saville has therefore performed a valuable service in bringing together and analyzing the material relating to the exodus from rural districts which has made England one of the most urbanized countries in the world.

Mr. Saville is an economic historian, not a demographer or statistician. His work does not pretend to be an original statistical analysis of the inadequate data from which internal migration in this country may be estimated. He is concerned with an explanation of the movement, and as far as statistics are concerned, he is content with the analysis of the census material and with giving a synoptic view of previous work on the subject. He has no difficulty in showing that migration to the towns consisted primarily of the younger members of the population and that women outnumbered men among the migrants.

The author argues that the attractions of the towns, both economic and social, and the shrinkage of employment opportunities in rural areas account for the considerable movement that took place within the last hundred years and impressively documents his case both with reference to conditions in the country as a whole and by a case study of a group of villages in Devon. Notwithstanding the nostalgia of a number of town dwellers for life in the country Mr. Saville believes that the process of rural depopulation is bound to continue and that the small village is no longer a viable economic and social unit. His pioneer study of a rather neglected problem is much to be welcomed and will, it is hoped, stimulate further researches on this important topic.

E. GREBENIK.

16.—*Geographical Differentials of Agricultural Wages in the United States.* By W. D. Weatherford, Jr. Harvard University Press, 1957. (London, Oxford University Press). 99 pp. 9″. 10*s*.

In view of the increasing mechanization of agriculture in the United States, involving larger output from farm labour, a series of studies has been undertaken by Harvard University, with the assistance of the Ferguson Foundation, relating to analyses of the integration of labour and the use of machines and of the functioning of agriculture in the total economy. The study under review, which is based on work done by the author in 1947 and 1948, develops certain theories with regard to geographical differentiation in agricultural wages. He estimates that nine million Americans earn their living in agriculture, the majority being farmers and their relatives with two to three million hired workers of whom the number varies seasonally. Nevertheless he regards the wages of hired workers as an important area of study since they are mainly employed on the larger farms and in the aggregate provide the manpower to produce one-third of the total agricultural output. His examination covers general farm wage rates, irrespective of kinds and varieties of workers in the area, and particular wage rates, i.e. averages for various types of farm jobs.

His first conclusion as to the general distribution pattern of farm wages is that the lowest are found in South Carolina, central Georgia and Alabama, the "black belt". They rise gradually in concentric circles towards the north-west with the highest in Washington State. This pattern has persisted since the Civil War and, with one exception, cyclical and seasonal variations have not fundamentally affected the distribution. The one exception was in the New England States, a highly industrialized area, where during the twenties and thirties first industrial prosperity drew workers from the land and later the reverse process occurred. The author compares this pattern with that of productivity on farms and demonstrates that the two patterns in the main coincide. He discusses the reasons for this and presents figures in support of his conclusions. He argues that wage rates are determined by the demand for the supply of labour but that demand is a function of productivity. In other words the wage level determines for individual farms the level of productivity at which it pays the farmer to utilize labour. Consequently he concludes that the supply of labour is the most important factor in the determination of the geographical pattern of farm wages. The availability of labour depends on the density of the farm population in different areas and this in turn depends on the birth rate. It is also affected by the extent to which alternative employment, which is liable to draw off farm workers, exists in the area. The labour supply may again be affected by inter-regional migration. On this point the author refers to the fact that such migration has never been sufficient to equalize the ratio of men to resources and movement out of the Southern States has not eradicated the damming up of labour on farms in that area, with consequently low wages.

In conclusion the study includes policy proposals designed to remove some of the unfavourable features of American agriculture which it reveals, including suggestions for raising the general level of farm wages in relation to those in other industries and for removing or reducing the comparatively wide variation in farm wages and productivity in different States.

Not the least valuable part of the publication, especially from the point of view of research workers, is the very full annotation showing the sources of available information.

R. B. AINSWORTH.

17.—*New Technology and the Supply of Petroleum: the treatment of uncertainty in resources planning*. By Miller B. Spangler. New York, the Author, 1956. Printed by the Chicago University Press. xiii, 299 pp. 9″. $3.50.

The basic idea of this book is that the best forecast that can be made of a future situation is to be obtained by considering all the factors that are expected to have a bearing on the situation, weighting these factors with subjective numerical probability estimates obtained from experts in the related fields, and integrating-out the factors to provide a final subjective probability distribution for the future situation. The author takes the future supply of petroleum in the U.S.A. as a case study and poses the question, "Can future technological advances in the supply of petroleum ensure a continued high level of domestic supply through 1975 at prices not substantially higher than those at present?" Although a number of different factors bearing on an answer to this question are listed, Dr. Spangler concerns himself solely with the part to be played by technological change. He does however suggest a model "whereby the wisdom of various experts can be aggregated". This he calls a "probability map" which consists of subjective probability curves arranged one behind the other in depth, showing the probabilities assigned by experts in various fields to values of the variable which, for example, may be the amount of oil obtainable in 1975 at current prices. The probability estimates for any one factor are assumed to be independent of those given for any other. The aggregated subjective probability density $p(X)$ is equal to the multiple integral of the product $\Pi_i\, p_i(X_i)\, dX_i$ over the whole range of the variables X_i subject to the condition $\Sigma_i\, X_i = X$. With regard to the effect of technological change, three lengthy chapters (pp. 133–279) are devoted to the presentation of facts and data as found in the literature dealing respectively with technological progress in the recovery of petroleum, in oil-field development, and in discovery technology. The final chapter is concerned with the integration of these separate factors governing technological change and is but 13 pp. long. It is revealing to see that the author finds it impossible to make a proper use of his own model; in fact in the whole book only one subjective probability curve is drawn and the aggregation of the factors bearing on the contribution to the problem from technological change does not produce a subjective probability distribution. If there is any merit in the proposed model, it is felt that a stronger case than this will have to be made before it is appreciated.

G. HORSNELL.

STATISTICAL NOTES

(1) British Official Statistics

The index of retail prices compiled by the Ministry of Labour and National Service which was 104 (January 17th, 1956 = 100) in February remained at that figure in March and April and rose to 105 in May. The rise, however, though it affected the nearest whole number was only fractional, from a little under 104·5 to 104·6. Between mid-April and mid-May there were increases in the prices of some food items, including eggs, bacon and vegetables, but these were largely offset by reductions in coal prices and in road passenger transport fares in some areas. The detailed figures of the weights used in calculating the index and the indices for the different commodity groups were as follows:

	Food	*Alcoholic Drinks*	*Tobacco*	*Housing*	*Fuel and Light*	*Durable Household Goods*	*Clothing and Footwear*	*Transport and Vehicles*	*Miscellaneous*	*Services*	*All Items*
Weights:	350	71	80	87	55	66	106	68	59	58	1,000
Feb. 12th, 1957	103·5	102·6	105·3	105·2	106·3	101·9	101·6	109·8	105·9	105·9	104·3
March 12th, 1957	102·1	103·6	105·3	105·4	106·3	101·9	101·8	111·3	106·0	107·0	104·1
April 16th, 1957	102·7	103·9	105·3	108·9	106·3	100·7	101·9	109·6	106·4	107·3	104·5
May 14th, 1957	103·9	103·9	105·3	109·4	101·8	100·5	102·0	108·7	106·5	107·7	104·6

The Ministry of Labour index of weekly wage rates calculated on the basis of January 31st, 1956 = 100, showed that in February 1957 the level was 107. It remained at this figure in March and rose to 108 in April and 110 in May. For manufacturing industries alone the figures were 107 in February, March and April and 111 in May. The principal increases in April affected employees of British Railways and the London Transport Executive and workpeople in baking, retail meat distribution and chemical manufacture. In May there were increases for workers in engineering, building and civil engineering, coal mining, local authority services and retail food, drapery and furniture distribution.

The total working population and the numbers in civil employment in the four months ended April 1957 were as follows:—

(*Thousands*)

	Total Working Population			*Numbers in Civil Employment*		
Date	*Males*	*Females*	*Total*	*Males*	*Females*	*Total*
Jan., 1957 . .	16,212	7,873	24,085	15,231	7,748	22,979
Feb., 1957 . .	16,167	7,857	24,024	15,213	7,735	22,948
March, 1957 .	16,136	7,825	23,961	15,200	7,708	22,908
April, 1957 .	16,138	7,851	23,989	15,221	7,740	22,961

The number of persons on the unemployment registers of the Employment Exchanges fell by 17,000 in March, 21,000 in April and 29,000 in May. The total for May represented 1·5 per cent. of the total number of employees in Great Britain. The percentages in the different Regions ranged from 0·9 in the East and West Ridings, 1·0 in London and South Eastern and the North Midlands to 2·5 in Wales and 2·6 in Scotland.

The following is a sex analysis of the figures:

Number of Unemployed Persons on the Registers of Employment Exchanges

	Men and Boys	*Women and Girls*	*Total*
Feb., 11th 1957 . .	262,351	117,709	380,060
Mar. 11th, 1957 . .	251,171	111,774	362,945
Apr. 15th, 1957 . .	236,511	105,748	342,259
May 13th, 1957 . .	214,978	98,494	313,472

Of the total of 313,472 in May, 72,265 had been enemployed for not more than two weeks, 75,352 for two to eight weeks, and 146,326 for more than eight weeks, while 19,529 were temporarily stopped. In the four weeks ended May 8th 169,203 vacancies were filled by the Employment Exchanges. The number of vacancies unfilled at this date was 283,025.

The numbers of insured persons absent from work owing to sickness, including self-employed as well as employed, were 948,900 in February, 969,800 in March, 890,000 in April and 859,400 in May. The numbers of employed persons absent owing to industrial injuries were 61,700, 62,900 58,800 and 56,500.

The *Ministry of Labour Gazette* for June 1957, contains an age analysis of the employee population in Great Britain at May 1956. Of the total of 14,100,000 males 199,000 were 70 and over and 590,000 were 65 and over. Amongst 7,600,000 females, 47,000 were 70 and over and 152,000 were 65 and over. Figures relating to married women showed a total of 3,723,000 or 49 per cent. of the total number of female employees. The figure showed an increase if 153,000 over the previous year The review also includes regional and industrial analyses of the figures and some estimates of inter-regional migration.

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(2) OTHER STATISTICS

Systems of Social Security, Great Britain (International Labour Office, 1957. 73 pp. 3*s*. 6*d*.) is the third of a series of national monographs on the subject. It was drafted by the Ministries of Pensions and National Insurance and of Health, in accordance with a plan by the International Labour Office.

It describes the five main schemes under the headings of legal basis, scope of protection, provision of benefits, organization, and financing of social security. The last chapter contains a few statistical tables on rates of contribution, receipts and payments of the National Insurance Fund 1953–54 etc., but no data are given on the number of persons covered by the schemes, nor number of beneficiaries which would have added to the value of the report. Such data are given, however, by the International Labour Office in its Year Book of Labour Statistics.

The Economic Survey of Europe 1956 (Economic Commission for Europe, Geneva, 1957, 18*s*.) —the tenth of the series—contains the usual analysis of current developments in eastern Europe and the U.S.S.R. (chapter 1) and in western Europe (chapter 2). The economies of both these regions continued to expand in 1956, though there was a slowing down compared with 1955, and a speeding up of wage increases in 1956 in western Europe.

Chapters are added this year on the long-term plans of the east European countries, and on the possible implications of the proposals for a Common Market and a Free Trade Area and special chapters are devoted to a study of European transport including the co-ordination of road and rail transport; to one on income distribution in five West European countries, including the size distribution of personal incomes and the changes in recent years in the distribution of income among the various factors of production.

A very full list of references is given this year at the end of the volume instead of being scattered throughout the volume as in previous years.

The Department of Statistics of the New Zealand Government has issued as a special supplement to the Monthly Abstract of Statistics for February 1957 a *Report on the Inter-Industry Study of the New Zealand Economy in* 1952–53. Input-output tables showing the interdependence of productive activities are presented with a full discussion of techniques, mathematical theory and conceptual problems. A brief history of inter-industry research in other countries is also given.

CURRENT NOTES

The University of Aberdeen has established a postgraduate Diploma in Statistics, teaching for which begins in October 1957. In character, scope, and standard, the Diploma will be similar to those already in existence at some other British Universities.

The number of students that can be taken is severely limited, especially for the first two or three years, and prospective students should make application with as little delay as possible.

It is now possible for suitably qualified persons in the London area to read, at Birkbeck College, (part-time or full-time) for the degrees of Ph.D. and M.Sc. in Statistics. Further particulars and forms of application may be obtained from the Registrar, Birkbeck College, Malet Street, London, W.C.1.

STATISTICAL AND ECONOMIC ARTICLES IN RECENT PERIODICALS

UNITED KINGDOM—

Accountants' Magazine—

May 1957—Capital investment and output in the United Kingdom and United States.

Annals of Human Genetics—

March 1957—Counting methods in genetical statistics: *C. A. B. Smith.*

Biometrika—

June 1957—Restricted sequential procedures: *P. Armitage.* On theoretical models for competitive and predatory biological systems: *M. S. Bartlett.* The consistency and adequacy of the Poisson-Markoff model for density fluctuations: *V. T. Patil.* Testing for serial correlation in least squares regression: *E. J. Hannan.* On the analysis of multiple regression in k categories: *S. Kullback* and *H. M. Rosenblatt.* Bivariate structural relation: *R. L. Brown.* An analysis of paired comparison designs with incomplete repetitions: *J. W. Wilkinson.* Non-null ranking models. I: *C. L. Mallows.* The generalization of probit analysis to the case of multiple responses: *J. Aitchison* and *S. D. Silvey.* Experimenting with organisms as blocks: *S. C. Pearce.* The use of a concomitant variable in selecting an experimental design: *D. R. Cox.* Approximate confidence limits for components of variance: *M. G. Bulmer.* Multiple runs: *D. E. Barton* and *F. N. David.* Binomial sampling schemes and the concept of information: *D. V. Lindley.* A statistical paradox: *D. V. Lindley.* Stochastic cross-infection between two otherwise isolated groups: *H. W. Haskey.* Some statistics associated with the random disorientation of cubes: *J. K. Mackenzie* and *M. J. Thomson.* The difference between consecutive members of a series of random variables arranged in order of size: *J. H. Darwin.* Relation between the distributions of non-central t and of a transformed correlation coefficient: *B. I. Harley.* On the solution of estimating equations for truncated and censored samples from normal populations: *A. C. Cohen, Jr.* Upper percentage points of the generalized beta distribution. I: *F. G. Foster* and *D. H. Rees.* On problems in which a change in a parameter occurs at an unknown point: *E. S. Page.* Testing for departure from the exponential distribution: *D. J. Bartholomew.* The distribution of range in normal samples with $n = 200$: *B. I. Harley* and *E. S. Pearson.* Studies in the history of probability and statistics. V. A note on playing cards: *M. G. Kendall.* A singularity in the estimation of binomial variance: *A. Stuart.* Student's distribution and Riemann's elliptic geometry: *A. Wintner.* Some interrelations among compound and generalized distributions: *J. Gurland.* A note on tests of significance for linear functional relationships: *M. S. Bartlett.* The moments of the Leipnik distribution: *M. G. Kendall.* The effect of transformations of variables upon their correlation coefficients: *M. H. Quenouille.* Further properties of an angular transformation of the correlation coefficient: *B. I. Harley.* Heterogeneity of error variances in a randomized block design: *F. A. Graybill* and *J. L. Folks.* An extension property of a class of balanced incomplete block designs: *G. P. Sillitto.* Sequentially determined confidence intervals: *N. L. Johnson.* Estimation of means of normal populations from observed minima: *H. A. David.* The power of the Poisson index of dispersion: *J. H. Darwin.* Some properties of the bivariate normal distribution considered in the form of a contingency table: *H. O. Lancaster.*

British Journal of Preventive and Social Medicine—

January 1957—Control of tuberculosis: importance of heredity and environment: *A. W. Anderson, B. Benjamin, R. Grenville-Mathers* and *H. J. Trenchard.* Housing and tuberculosis in a mass radiography survey: *G. Z. Brett* and *B. Benjamin.* Contribution of cigarette smoking to respiratory disease: *J. H. Edwards.* A survey of handicapped persons: *A. L. Banks, R. G. Carpenter, J. A. Hislop* and *G. Nisbet.* Vagaries of local mortality rates under the 1953–54 rules for transfer of deaths: *D. Hewitt.*

British Journal of Sociology—

March 1957—Social class and the ranking of occupations: *C. J. Adcock* and *L. B. Brown.* Intelligence tests, social class and selection for secondary schools: *J. Floud* and *A. H. Halsey.* A. F. Bentley's inquiries into the behavioural sciences and the theory of scientific inquiry: *S. Ratner.*

British Journal of Statistical Psychology—

November 1956—"Spearman's rho" and the matching problem: *D. E. Barton* and *F. N. David.* The statistical analysis of the learning process. II. Stochastic processes and learning behaviour: *R. J. Audley* and *A. R. Jonckheere.*

May 1957—A study of the performance of 2,000 children on four vocabulary tests. II. Norms, with some observations on the relative variability of boys and girls: *M. I. Dunsdon* and *J. A. Fraser Roberts.* Psychological factors: their number, nature, and identification: *G. Bernyer.* The comparison of frequencies in matched samples: *A. Stuart.* Heredity and intelligence: a reply to criticisms: *C. Burt* and *M. Howard.*

Economica—

May 1957—A theory of second-hand markets: *A. H. Fox.* The relationship between the size of joint stock companies and the yield of their shares: *E. V. Morgan* and *C. Taylor.* Changes in the distribution of higher incomes: *R. G. D. Allen.*

Economic Journal—

March 1957—The size of trade unions: a study in the laws of aggregation: *P. E. Hart* and *E. H. Phelps Brown.* The scope of automation: *S. Moos.* Devaluation, a marriage of the elasticity and the absorption approaches: *H. Brems.* Trade balances and statutory marketing in primary exporting economies: *A. Hazlewood.* Estimating the Soviet national income: *M. C. Kaser.*

Faculty of Actuaries, Transactions—

Vol. 25, Part 2—Population mortality and assured lives' mortality in Great Britain: a comparison of trends: *H. A. R. Barnett.*

Incorporated Statistician—

April 1957—Cost accounting and statistics: *J. A. Reece.* The design of experiments. Part II. The nature of measurements: *M. J. Moroney.* The generation of income distributions: some illustrative models: *D. G. Champernowne.* Diagrams for rapid estimation of partial correlation co-efficients: *F. D. K. Liddell.*

Institute of Actuaries Students' Society, Journal—

April 1957—Sampling techniques and some applications: *P. G. Moore.* Social insurance overseas: *R. C. Gilder.* Bias of the census method: *W. L. Stevens.* Population data of the classical world: *P. J. H. Green.*

Journal of Agricultural Economics—

February 1957—Some thoughts on agricultural policy: *E. M. H. Lloyd.* The social environment of farm work: *G. P. Hirsch.* Some aspects of Commonwealth agricultural development: *G. S. Dunnett.* Farm classification in Britain—an appraisal: *R. Bennett Jones.* Note on the measurement of demand for fatstock: *J. R. Raeburn.*

Operational Research Quarterly—

March 1957—The name and nature of OR: *J. F. McCloskey* and *A. W. Swan.* A scrutiny of the British potato crop: *D. A. Boyd.* Estimation of the number of holes and charge per round in tunnel drivage: *A. Mercer.* Inspection in the paper industry: *H. F. Rance* and *I. F. Hendry.*

Oxford University Institute of Statistics, Bulletin—

February 1957—The free trade proposals: a symposium. Changes in the Soviet population: *A. Nove* and *J. A. Newth.* Sampling errors in the savings surveys: *L. R. Klein* and *P. Vandome.*

Population Studies—

March 1957—A fertility survey in the Maltese Islands: *D. Seers*. The population of Tikopia, 1929 and 1952: *W. D. Borrie, R. Firth* and *J. Spillius*. A survey of population in an area of Worcestershire from 1660–1850 on the basis of parish records: *D. E. C. Eversley*. Increases in recent mortality rates at high ages: *S. W. Caffin*.

AUSTRALIA—

Statistical Society of New South Wales Bulletin—

November 1956—Some statistical anniversaries: *F. B. Horner*. A new analysis of variance test for models with fixed effects and interaction: *H. Weiler*. The maximization of a linear function subject to linear restrictions: *R. W. Rutledge*.

INDIA—

Calcutta Statistical Association Bulletin—

March 1957—Enumerational errors in surveys: *B. Ghosh*. On a characterization of gamma distributions: *K. C. Seal*. A note on the asymptotic distribution of likelihood ratio: *K. P. Roy*. A note on the total relative loss of information in any design: *A. M. Kshirsagar*. An integral which occurs in statistics: *K. V. Ramachandran* and *C. G. Khatri*.

Maharaja Sayajirao University of Baroda, Journal—

October 1956—A method of determining a quantile from grouped data: *N. M. Bhatt*.

Sankhyā—

December 1956—Almost unbiassed estimates of functions of frequencies: *J. B. S. Haldane*. Sufficient statistics in elementary distribution theory: *R. V. Hogg* and *A. T. Craig*. Sufficient statistics and orthogonal parameters: *V. S. Huzurbazar*. A note on the multivariate extension of some theorems related to the univariate normal distribution: *D. Basu*. The Survey Research Centre of the Central Bureau of Statistics, Sweden: *T. Dalenius*. Ragnar Frisch on the factor cost concept of national income: *H. Ezekiel*. A rejoinder: *R. Frisch*. Food statistics: *P. C. Bansil*.

UNITED STATES—

American Economic Review—

March 1957—The simple analytics of welfare maximization: *F. M. Bator*. Interest rates and manufacturers' fixed investment: *F. Gehrels* and *S. Wiggins*. Growth in capacity and Canada's balance of payments: *J. C. Ingram*. Input-output coefficients as measures of product quality: *H. Brems*.

American Statistical Association, Journal—

March 1957—Statistical frontiers: *G. M. Cox*. A short-cut graphic method for fitting the best straight line to a series of points according to the criterion of least squares: *S. I. Askovitz*. Rapid analysis of 2 × 2 tables: *I. D. J. Bross* and *E. L. Kasten*. A note on the effects of non-response on surveys: *K. A. Brownlee*. A modification of Kendall's tau for the case of arbitrary ties in both rankings: *L. McK. Adler*. Two confidence intervals for the ratio of two probabilities and some measures of effectiveness: *G. E. Noether*. A systematic method of finding defining contrasts: *C. Holloway, Jr*. On the independence of tests of randomness and other hypotheses: *I. R. Savage*. Tables for best linear estimates by order statistics of the parameters of single exponential distributions from singly and doubly censored samples: *A. E. Sarhan* and *B. G. Greenberg*. Tables for tolerance limits for a normal population based on sample mean and range or mean range: *S. K. Mitra*.

American Statistician—

April 1957—Organizational problems related to large scale statistical computations at Bureau of Census: *J. F. Daly*. Basic instructions in statistical computations: *P. S. Dwyer*. Improvement of federal statistics: *R. T. Bowman*.

Annals of Mathematical Statistics—

March 1957—A theory of some multiple decision problems. I: *E. L. Lehmann.* Estimates for global central limit theorems: *R. P. Agnew.* Variances of variance components. II. The unbalanced single classification: *J. W. Tukey.* Some properties of generalized sequential probability ratio tests: *J. Kiefer* and *L. Weiss.* The minimum distance method: *J. Wolfowitz.* Statistical inference about Markov chains: *T. W. Anderson* and *L. A. Goodman.* Multistage statistical decision procedures: *M. A. Girschik, S. Karlin* and *H. L. Royden.* On a characterization of the normal distribution from properties of suitable linear statistics: *R. G. Laha.* On the estimation of autocorrelation in time series: *Z. A. Lomnicki* and *S. K. Zaremba.* Approximations to the power of rank tests: *C. K. Tsao.* Some uses of quasi-ranges: *J. T. Chu.* Modified randomization tests for nonparametric hypotheses: *M. Dwass.* On certain two-sample nonparametric tests for variances: *B. V. Sukhatme.* Multi-factor experimental designs for exploring response surfaces: *G. E. P. Box* and *J. S. Hunter.* Consistency of certain two-sample tests: *J. R. Blum* and *L. Weiss.* A note on truncation and sufficient statistics: *W. L. Smith.* A central limit theorem for multilinear stochastic processes: *E. Parzen.* On the enumeration of decision patterns involving *n* means: *R. L. Wine* and *J. E. Freund.* Percentiles of the ω_n statistic: *B. Sherman.* On the serial test for random sequences: *I. J. Good.* On the specification error in regression analysis: *H. Wold* and *P. Faxér.* Sets of measures not admitting necessity and sufficient statistics or subfields: *T. S. Pitcher.*

Bell System Technical Journal—

March 1957—Reading rates and the information rate of a human channel: *J. R. Pierce* and *J. E. Karlin.* Binary block coding: *S. P. Lloyd.* Selecting the best one of several binomial populations: *M. Sobel* and *M. J. Huyett.*

Biometrics—

March 1957—Incomplete block designs with row balance and recovery of inter-block information: *W. B. Taylor.* Extension of multiple range tests to group correlated adjusted means: *C. Y. Kramer.* An application of the Kronecker product of matrices in multiple regression: *E. A. Cornish.* Tables for the maximum likelihood estimate of the logistic function: *J. Berkson.* Bioassay from a parabola: *C. I. Bliss.* An evaluation of some statistical techniques used in the analysis of paired comparison data: *J. E. Jackson* and *M. Fleckenstein.* The distribution of European corn borer larvae, *Pyrausta nubilalis* (Hbn.), in field corn: *J. U. McGuire, T. A. Brindley* and *T. A. Bancroft.* An application of stochastic processes to experimental studies on flour beetles: *C. L. Chiang.* The use of affinity in chromosome mapping: *M. E. Wallace.* The heritable variance of $F_1 \times F_2$ progeny means: *J. H. van der Veen.*

Econometrica—

January 1957—Economic fluctuations and growth: *A. Smithies.* Assignment problems and the location of economic activities: *T. C. Koopmans* and *M. Beckman.* A generalized classical method of linear estimation of coefficients in a structural equation: *R. L. Basmann.* On the solution of discrete programming problems: *H. M. Markowitz* and *A. S. Manne.* Linear aggregation in input-output analysis: *H. Theil.* A new index number formula: *G. Stuvel.* Nonlinear power of adjacent extreme point methods in linear programming: *A. Charnes* and *W. W. Cooper.* Power series inversion of the Leontief matrix: *W. J. Berger* and *E. Saibel.* Note on the rational selection of decision functions: *H. Uzawa.*

Harvard Business Review—

November–December 1956—Operational approach to creativity: *W. J. J. Gordon.* Total quality control: *A. V. Feigenbaum.* Capital budgeting and game theory: *E. G. Bennion.*

Industrial Quality Control—

February 1957—Average sample number under curtailed or truncated sampling: *I. W. Burr.* Simplified computations for multiple regression: *C. Y. Kramer.* The principles of experimental design: *K. A. Brownlee.*

March 1957—The simplex method for solving linear programming problems: *C. E. Noble.* The Ampex quality rating system: *R. L. Pappas.* An application of the Bendix vendor quality rating system to a small plant: *G. Georgis.*

April 1957—Use of edge-notched cards in the analysis of variance: *L. S. Nelson.* Attribute sampling when defects are not randomly distributed: *I. W. Burr, F. G. Norris* and *J. W. W. Sullivan.* Some remarks on the problem of tolerance limits: *B. Ostle.*

May 1957—The challenge of total quality control: *A. V. Feigenbaum.* Initiating a quality control group in an existing organization: *L. E. Simon.* Top management decisions made easier through the effective use of quality control: *D. W. Alexander.* The relation between the engineer and the statistician: *J. Mandelson.* Make it right the first time: *L. Bass.* Controlling the reliability of soldered connections: *H. Kellogg.* Practical chemical process control: *R. S. Bingham, Jr.* Binomial and Poisson confidence limits: *F. E. Satterthwaite.*

Journal of Experimental Education—

December 1956—A study of the relationships of certain environmental factors to teaching success: *W. B. Knox.* Analysis of certain aspects of teaching ability: *M. L. Jones.*

Journal of Political Economy—

August 1956—Some foundations of economic development in the Upper East Tennessee Valley, 1850–1900. I: *W. H. Nicholls.* Prices of basic industrial products in the U.S.S.R., 1928–50: *A. Bergson, R. Bernaut* and *L. Turgeon.*

February 1957—The effectiveness of monetary policy: recent British experience: *W. L. Smith* and *R. F. Mikesell.* Convex indifference curves and diminishing marginal utility: *W. M. Gorman.* On measuring economic growth: *G. W. Nutter.* A statistical illusion in judging Keynesian models: *M. Friedman* and *G. S. Becker.* Multipliers and elasticities in foreign-trade adjustments: *F. Gehrels.*

Management Science—

January 1957—The Institute of Management Sciences: progress report: *G. H. Symonds.* Thoughts on linear programming and automation: *G. B. Dantzig.* Mathematical programming and service scheduling: *W. Karush* and *A. Vazsonyi.* Application of linear programming to investments in the electric power industry: *P. Massé* and *R. Gibrat.* Shorthand for defense planning: a proposal for comparing risks of war and capabilities: *C. L. Hosmer.* Programming resource use and capital investment in agriculture: *R. A. King, C. E. Bishop* and *J. G. Sutherland.* Statistical sampling techniques in the aging of accounts receivable in a department store: *R. M. Cyert* and *R. M. Trueblood.*

Milbank Memorial Fund Quarterly—

April 1957—The influence of war and post-war conditions on the teeth of Norwegian school children. II. Caries in the permanent teeth of children aged 7–8 and 12–13 years: *G. Toverud.* An estimate of the expectation of life in the United States in 1850: *P. H. Jacobson.* Are sex mortality differentials biologically caused? *F. C. Madigan.*

Monthly Labor Review—

May 1957—Experience with development areas in Great Britain: *J. A. Flexner* and *A. S. Ritter.*

Operations Research—

February 1957—The role of operational gaming in operations research: *C. J. Thomas* and *W. L. Deemer, Jr.* Economic-lot-size formulas in manufacturing: *A. Vazsonyi.* Bibliography on linear programming: *F. V. Rohde.* The solution of a certain two-person zero-sum game: *R. H. Brown.* The effect of departures from assumption when manufacturing to a specification: *E. S. Page* and *S. Muris.* Bombing problems—a statistical approach: *A. G. Laurent.* Some factors tending to limit the utility of the social scientist in military systems analysis: *D. N. Michael.* On a problem of production scheduling: *G. T. Bishop.*

April 1957—Résumé of useful formulas in queuing theory: *T. L. Saaty.* Some industrial applications of military operations research methods: *G. E. Kimball.* The distribution of the time required to reduce to some preassigned level a single-channel queue characterized by a time-dependent Poisson-distributed arrival rate and a general class of holding times: *G. Luchak.* The application of design of experiments and modeling to complex weapons systems: *E. Biser* and *M. Meyerson.* Salvo hit probabilities for offset circular targets: *A. D. Groves* and *E. S. Smith.* Optimum component redundancy for maximum system reliability: *R. Gordon.* Symposium on modern techniques for extremum problems. Operations research at Case Institute of Technology: *E. L. Arnoff.*

Psychometrika—

March 1957—New analysis of variance formulas for treating data from mutually paired subjects: *J. Lev* and *E. F. Kinder*. On the ranking problem: *I. Blumen*. Estimation of error variances of test scores: *J. A. Keats*. The detailed method of optimal regions: *P. S. Dwyer*. The development of hierarchical factor solutions: *J. Schmid* and *J. M. Leiman*. A theory of pattern analysis for the prediction of a quantitative criterion: *A. Lubin* and *H. G. Osburn*. The expected variance of the sampling errors for a set of item-criterion correlations: *H. E. Brogden*. A necessary and sufficient formula for matric factoring: *L. Guttman*. Exact probabilities for contingency tables using binomial coefficients: *J. M. Sakoda* and *B. H. Cohen*. A stochastic model for rote serial learning: *R. C. Atkinson*. A computational procedure for tau correlation: *D. S. Cartwright*. Iterative inverse factor analysis—a rapid method for clustering persons: *B. M. Bass*.

Quarterly Journal of Economics—

February 1957—On capital productivity, input allocation and growth: *F. M. Bator*.

Review of Economics and Statistics—

February 1957—The redistributional effects of inflation: *G. L. Bach* and *A. Ando*. Federal Reserve Board Committee reports on consumer expectations and savings statistics: *G. Katona*. Analysis of life insurance premiums: *M. E. Kreinin*, *J. B. Lansing* and *J. N. Morgan*. British exports to the United States, 1948–1955: *R. Hutt*. Price-competition between India, Japan, and the U.K. in the Indian cotton textile market during the nineteen-thirties: *F. A. Mehta*. Turnover and growth of the largest industrial firms, 1906–1950: *S. Friedland*.

BELGIUM—

Bulletin de l'Institut de Recherches Economiques et Sociales—

March 1957—Les limites physiques de la production de subsistance: *P. Leurquin*. Le crédit immobilier en Belgique de 1802 à 1954: *P. Têcheur*.

CZECHOSLOVAKIA—

Aplikace Matematiky—

Vol. 2, No. 1—Některé analogie necentrálního t-testu: *F. Zítek*.

Czechoslovak Mathematical Journal—

Vol. 7 (82), No. 1—Transformation ou σ-algèbre suffisante et minimum de la probabilité d'erreur: *A. Pérez*. A note on a linear programming problem: *J. Abrham*. The asymptotic behaviour of branching stochastic processes: *M. Jiřina* (Russian text, English summary).

EGYPT—

L'Egypte Contemporaine—

April 1957—The middle classes in Egypt: *H. El-Saaty*. Measures of efficiency in the organization and use of farm labour: *A. A. El Tonbary*.

FRANCE—

Revue d'Économie Politique—

January–February 1957—L'économie globale et l'étalon des valeurs: *J. Rueff*. De l'ambiguité des critères de distinction à la prétendue opposition de la micro et de la macro-économie: *R. Courtin*. Le problème de l'agrégation: *J.-R. Boudeville*.
March–April 1957—Les programmes linéaires stochastiques: *G. Tintner*.

Revue de Statistique Appliquée—

Vol. 5, No. 1—Efficience de l'échantillonnage double par attributs: *H. C. Hamaker* and *R. van Strik*. Sur les échantillonnages de matériaux: *W. E. Deming*. Etude du choix des plans d'échantillonnage simples d'après leurs conséquences pécuniaires: *M. Courtillot*. Problèmes relatifs aux échantillonnages à plusieurs niveaux: *J. Ulmo*, *D. Schwartz* and *A. Vessereau*. Mise sous contrôle de machines à paqueter la poudre à priser: *B. Cyffers*.

Méthode graphique de séparation de deux populations: *H. Pin.* Quelques exemples d'applications du contrôle statistique des fabrications dans les entreprises américaines: *H. Legros.*

GERMANY—

Mitteilungsblatt für mathematische Statistik—

Vol. 9, No. 1—Die Pascalsche Fragestellung für Merkmalsiterationen: *O. Ludwig.* Der Likelihood-Anpassungstest: *K.-A. Schäffer.* Über einige Aspekte des Demingplanes: *H.-J. Zindler.*

ITALY—

Giornale degli Economisti e Annali di Economia—

January–February 1957—La ricerca operativa come scienza delle decisioni: *F. Brambilla.* La ricerca operativa nel campo militare: *V. Barengo.* Di uno schema macroeconomico applicato alla finanza nella nuova edizione dello *Handbuch der Finanzwissenschaft: E. Gerelli.*

Rivista Italiana di Economia, Demografia e Statistica—

July–December 1955—Atti della XVI riunione scientifica della Società Italiana di Economia, Demografia e Statistica (Perugia, 3–5 maggio, 1956). Vol. I. Aspetti strutturali dell'agricoltura italiana.

Statistica—

January–March 1957—Characteristics and convergence of Gram-Charlier series: *E. Ikenberry.* Diminuzione della mortalità e struttura per l'età della popolazione: *M. De Vergottini.* Una tavola di eliminazione e permanenza dell'istruzione in Italia: *R. Lenzi.* Questioni attuali dell' organizzazione dell'industria cinematografica italiana: *L. Bergonzini.* Bibliografie: L. Einaudi, R. A. Fisher, C. Gini.

Università degli Studi, Bari, Annali dell'Istituto di Statistica—

Vol. 28—Indagini sui bilanci delle famiglie contadine di Puglia e Lucania: *G. Lasorsa.*

JAPAN—

Annals of the Institute of Statistical Mathematics—

Vol. 8, No. 2—Decision rule, based on the distance, for the classification problem: *K. Matusita.* Validity of approximate normality values for $\mu \pm k\sigma$ areas of practical type continuous populations: *J. E. Walsh.* On a zero-one process and some of its applications: *H. Akaike.* Order statistics for discrete case with a numerical application to the binomial distribution: *M. Siotani.* On the distribution-free classification of an individual into one of two groups: *H. Hudimoto.* Note on the decision of optimal tolerance in the design of a simple random assembling: *I. Higuti.* Random number generator: *M. Isida* and *H. Ikeda.*

NETHERLANDS—

Statistica Neerlandica—

Vol. 11, No. 1—Statistical priesthood (Savage on personal probabilities): *D. van Dantzig.* De invloed van de keuze der gewichten op de hoogte van een gewogen indexcijfer: *P. de Wolff.* Opmerkingen over de standaardafwijking van de constante term bij regressieanalyse: *C. J. van Eijk* and *H. Moerman.*

SPAIN—

Trabajos de Estadística—

Vol. 7, No. 3—Inferencia estadística en los modelos biométricos y su aplicación al seguro de vida: *A. Vegas.* Sobre la noción de estimador consistente: *S. Rios.* On the variance stabilizing properties of certain logarithmic transformations: *B. M. Bennett.* Algunas notas sobre la estadística en Holanda: *F. Azorin Poch.* Programación lineal: un instrumento matematico al servicio de los empresarios: *E. Cansado.*

Vol. 8, No. 1—Sampling without replacement from finite populations: *E. Cansado.* The modality of Neyman's contagious distribution of type A: *D. E. Barton.* A demand curve for luxuries: *M. J. Beckmann.* Note on the method of inverse sampling: *B. M. Bennett.* Sobre el ensayo de la vacuna del Dr. Salk en 1954: *R. Pro.* Elaboracion de un modelo biometrico, su interpretacion estadistica y aplicaciones actuariales: *U. Nieto de Alba.*

SWITZERLAND—

Mitteilungen der Vereinigung schweizerischer Versicherungsmathematiker—

April 1957—Orthogonale Polynome mehrerer Veränderlichen und ihre Anwendung in der ein- und zweidimensionalen Ausgleichsrechnung: *R. Hüsser.*

U.S.S.R.—

Matematicheskiĭ Sbornik—

Vol. 41(83), No. 1—O vzaimootnoshenii mezhdu deskriptivnoĭ izmerimost'yu, absolyutnoĭ izmerimost'yu i svoĭstvom Béra: *A. V. Gladkiĭ,* K voprosu ob interpolirovanii v klasse tselȳkh funktsiĭ konechnogo poryadka: *A. F. Leont'ev.*

Uspekhi Matematicheskikh Nauk—

March-April 1957—Stroenie lokal'no bikompaktnȳkh grupp i pyataya problema Hilberta: *V. M. Glushkov.* Osnovnȳe polozheniya o defektnȳkh chislakh, kornevȳkh chislakh i indeksakh lineĭnȳkh operatorov: *I. Ts. Gokhberg* and *M. G. Krein.*

INTERNATIONAL—

Estadística—

September 1956—Algunos errores en la declaración de edad en censos de población de 1950 en Centro América y México: *J. Arias B.* Aproximación de la tasa anual promedio de cambio: *J. S. Siegel.* Sources, procedures of compilation, and types of current industrial statistics in Canada: *Dominion Bureau of Statistics, Canada.* Programación del desarrollo estadistico nacional: *Oficina de Estadistica, Naciones Unidas.* Collection of mental disease statistics in the United States: *M. Kramer.* Problemas en la aplicación del muestreo en encuestas agropecuarias en la America Latina: *Oficina de Estadistica, Organizacion de las Naciones Unidas para la Agricultura y la Alimentacion.* Problemas encontrados en estudios de gastos de la familia hechos recientemente en paises latinoamericanos: *P. B. Paro.*

World Health Organization, Bulletin—

Vol. 16, No. 2—Cancer mortality in various countries: *A. J. Phillips* and *M. Owchar.*

LIST OF ADDITIONS TO THE LIBRARY

Since the issue of Part II, 1957, the Society has received the publications enumerated below.

I.—OFFICIAL PUBLICATIONS

United Kingdom. *Central Office of Information.* Town and country planning in Britain. London, H.M.S.O., 1956. [iii], 28p. 9¼″. 2*s.* (Ref. Pamphl. 9)

General Register Office. Census 1951, England and Wales. Industry tables. London, H.M.S.O., 1957. xiii, 648p. fold. table. 13¼″. 147*s.*

Medical Research Council. Individual differences in night-vision efficiency; [by] M. H. Pirenne, F. H. C. Marriott and E. F. O'Doherty: with a section on "the frequency of seeing at low illumination"; by H. K. Hartline and P. R. McDonald. London, H.M.S.O., 1957. vii, 83p. plate. 9¾″. 8*s.* (Spec. Rep. Ser. 294)

Leukaemia and aplastic anaemia in patients irradiated for ankylosing spondylitis; [by] W. M. Court-Brown and R. Doll. London, H.M.S.O., 1957. vi, 135p. plate. 9¾″. 10*s.* 6*d.* (Spec. Rep. Ser. 295)

Ministry of Defence. Defence statistics, 1957/58. London, H.M.S.O., 1957. 8p. 9¾″. 6*d.* (Cmnd. 130)

Ministry of Education. Standards of reading, 1948 to 1956. London, H.M.S.O., 1957. vi, 46p. 8½″. 2*s.* 6*d.* (Pamphlet 32)

Ministry of Health & Ministry of Housing and Local Government. The bacteriological examination of water supplies. London, H.M.S.O., 1957. viii, 52p. 9½″. 3*s.* (Rep. Publ. Hlth. Med. Subj. 71)

Ministry of Housing and Local Government. Local government: functions of county councils and county district councils in England and Wales. London, H.M.S.O., 1957. 9p. 9¾″. 8*d.* (Cmnd. 161)

National Physical Laboratory. The direction of research establishments: proceedings of a symposium held ... on 26th, 27th and 28th September, 1956. London, H.M.S.O., 1957. Various paging. 9¼″. 25*s.*

Office of the Lord President of the Council. Forestry, agriculture and marginal land: a report by the Natural Resources (Technical) Committee. London, H.M.S.O., 1957. v, 67p. 3 plates. 9¾″. 4*s.*

Royal Commission on Common Land. Minutes of evidence. 17–30. London, H.M.S.O., 1957. 14 pts. 9¾″. 24*s.* 6*d.*

Scottish Office & Ministry of Power. Capital investment in the coal, gas and electricity industries. London, H.M.S.O., 1957. 11p. 9¾″. 8*d.* (Cmnd. 132)

Social Survey. The adolescent in Britain; by Leslie T. Wilkins. [London], Central Office of Information, 1955. 117p. 13″. (S.S.148 (P))

The diets of young children; by J. E. Fothergill. [London], Social Survey, 1952. [ii], 51p. 2 diagrs. 13½″. (N.S. 169/2)

The effects of a local road safety campaign on the behaviour of road-users; by H. D. Willcock ... [London], Central Office of Information, 1951. 18p. 13″. (S.S. 175)

Incentives in industry; by Geoffrey Thomas. [London], Social Survey, 1952. 24p. 13½″. (S.S.185/2)

Some useful data when sampling the population of England and Wales; by P. G. Gray, Elizabeth A. Parr and R. M. Blunden. London, Central Office of Information, 1956. xxxiv, 383p. 16¼″ × 13″. 105*s.*

The use of heating appliances and the expenditure on fuel by urban households living in dwellings of medium and low rateable values; by P. G. Gray ... [London], Central Office of Information, 1954. 46p. fold. diagrs. 13″.

University Grants Committee. University development: interim report on the years 1952 to 1956. London, H.M.S.O., 1957. 26p. 9¾″. 1*s.* 3*d.* (Cmnd. 79)

Australia. *Bureau of Agricultural Economics.* The Australian sheep industry survey, 1954. South Australia. Canberra, 1957. vi, 54p. 9¾″.

Canada. *Dominion Bureau of Statistics. General Assignments Division.* Industrial research-development expenditures in Canada, 1955. Ottawa, 1956. 28p. 11″. 50*c.* (Ref. Pap. 75)

Dominion Bureau of Statistics. Industry and Merchandising Division. Energy sources in Canada: commodity statements for 1926, 1929, 1933 and 1939. Ottawa, 1957. 72p. 11″. $1. (Ref. Pap. 74)

Denmark. *Statistiske Departement.* Erhverv, fag og arbejdsstilling ved folketaellingen 1950. Industry, occupation and social status, population census 1950. København, 1957. 105p. 9¼″. *kr.* 2.50. (Statist. Meddelelser 4.162.3)

Finland. *Statistiska Centralbyrån.* Väestötilastoa ... 1950 population census. Vol. VI. Properties and buildings. Helsinki, 1957. 83p. 4 tables. 11¾″. (Official Statistics of Finland VI. C 102)

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Issue Department									
Liabilities			Assets						
Notes in Circulation	Notes in Banking Department		Govt. Debt. (£11,015) and Other Govt. Securities	Other Securities	Coin other than Gold Coin	Total of cols. 4, 5 and 6 (Fiduciary Issue)	Gold Coin and Bullion†		Total of Liabilities or Assets
£	£		£	£	£	£	£	per oz. fine *s. d.*	£
1,848,308	52,051	Jan. 4	1,896,246	743	3,012	1,900,000	359	*249/6*	1,900,359
1,811,616	38,743	,, 11	1,846,288	701	3,012	1,850,000	359	*249/3*	1,850,359
1,786,489	38,869	,, 18	1,821,227	761	3,012	1,825,000	359	*249/2*	1,825,359
1,771,665	53,693	,, 25	1,821,304	683	3,012	1,825,000	359	*249/3*	1,825,359
1,770,735	54,624	Feb. 1	1,821,292	697	3,011	1,825,000	359	*249/3*	1,825,359
1,777,364	47,995	,, 8	1,821,243	746	3,011	1,825,000	359	*249/3*	1,825,359
1,779,239	46,119	,, 15	1,821,292	698	3,010	1,825,000	359	*249/5*	1,825,359
1,779,307	46,052	,, 22	1,821,223	764	3,013	1,825,000	359	*249/2*	1,825,359
1,787,882	37,476	,, 29	1,821,290	699	3,011	1,825,000	358	*249/1*	1,825,358
1,801,159	24,200	Mar. 7	1,821,241	749	3,011	1,825,000	359	*249/7*	1,825,359
1,808,048	17,310	,, 14	1,821,284	705	3,011	1,825,000	359	*249/4*	1,825,359
1,816,136	59,223	,, 21	1,871,247	740	3,014	1,875,000	359	*249/3*	1,875,359
1,841,297	34,062	,, 28	1,871,219	766	3,015	1,875,000	359	*249/5*	1,875,359
1,852,376	22,983	Apr. 4	1,871,282	707	3,011	1,875,000	359	*249/3*	1,875,359
1,850,103	25,255	,, 11	1,871,233	753	3,015	1,875,000	358	*249/1*	1,875,358
1,842,075	33,283	,, 18	1,871,194	795	3,011	1,875,000	358	*249/0*	1,875,358
1,841,951	33,408	,, 25	1,871,264	723	3,013	1,875,000	358	*249/0*	1,875,358
1,848,749	26,610	May 2	1,871,216	772	3,011	1,875,000	359	*249/2*	1,875,359
1,855,691	19,667	,, 9	1,871,279	708	3,013	1,875,000	358	*249/1*	1,875,358
1,860,985	14,373	,, 16	1,871,178	808	3,013	1,875,000	358	*249/1*	1,875,358
1,867,776	32,582	,, 23	1,896,280	706	3,015	1,900,000	359	*249/2*	1,900,359
1,871,274	29,085	,, 30	1,896,253	736	3,011	1,900,000	359	*249/6*	1,900,359
1,871,083	29,277	June 6	1,896,191	797	3,012	1,900,000	359	*249/9*	1,900,359
1,875,286	25,073	,, 13	1,896,262	726	3,013	1,900,000	359	*249/4*	1,900,359
1,879,135	21,224	,, 20	1,896,220	767	3,013	1,900,000	359	*249/5*	1,900,359
1,893,410	56,950	,, 27	1,946,195	793	3,012	1,950,000	360	*249/11*	1,950,360
1,906,565	43,795	July 4	1,946,266	723	3,011	1,950,000	360	*250/2*	1,950,360
1,922,454	27,906	,, 11	1,946,196	789	3,014	1,950,000	360	*250/3*	1,950,360
1,940,641	59,719	,, 18	1,996,259	729	3,012	2,000,000	360	*250/2*	2,000,360
1,963,007	37,353	,, 25	1,996,213	772	3,014	2,000,000	361	*250/8*	2,000,361
1,979,215	21,148	Aug. 1	1,996,166	820	3,014	2,000,000	363	*252/0*	2,000,363
1,977,668	22,695	,, 8	1,996,230	759	3,012	2,000,000	362	*251/9*	2,000,362
1,950,999	49,363	,, 15	1,996,178	811	3,010	2,000,000	362	*251/6*	2,000,362
1,924,391	25,971	,, 22	1,946,175	811	3,013	1,950,000	362	*251/5*	1,950,362
1,907,490	42,872	,, 29	1,946,236	753	3,011	1,950,000	362	*251/5*	1,950,362
1,900,668	24,694	Sept. 5	1,921,206	779	3,015	1,925,000	362	*251/5*	1,925,362
1,892,966	32,396	,, 12	1,921,249	736	3,015	1,925,000	362	*251/6*	1,925,362
1,882,781	42,581	,, 19	1,921,226	759	3,014	1,925,000	362	*251/6*	1,925,362
1,880,647	44,715	,, 26	1,921,280	710	3,010	1,925,000	362	*251/7*	1,925,362
1,881,434	43,927	Oct. 3	1,921,206	784	3,010	1,925,000	362	*251/5*	1,925,362
1,883,219	42,142	,, 10	1,921,183	804	3,013	1,925,000	362	*251/4*	1,925,362
1,878,606	46,756	,, 17	1,921,204	782	3,014	1,925,000	362	*251/5*	1,925,362
1,874,122	51,240	,, 24	1,921,214	773	3,013	1,925,000	362	*251/5*	1,925,362
1,877,846	47,517	,, 31	1,921,278	710	3,012	1,925,000	363	*252/0*	1,925,363
1,886,894	38,468	Nov. 7	1,921,220	768	3,012	1,925,000	362	*251/7*	1,925,362
1,892,559	32,802	,, 14	1,921,201	789	3,010	1,925,000	362	*251/5*	1,925,362
1,894,440	30,921	,, 21	1,921,266	724	3,010	1,925,000	361	*251/3*	1,925,361
1,906,332	19,029	,, 28	1,921,222	764	3,014	1,925,000	361	*250/11*	1,925,361
1,937,672	37,689	Dec. 5	1,971,277	708	3,015	1,975,000	361	*250/11*	1,975,361
1,976,232	49,128	,, 12	2,021,217	769	3,014	2,025,000	360	*250/3*	2,025,360
1,996,980	28,380	,, 19	2,021,225	761	3,014	2,025,000	360	*250/4*	2,025,360
1,997,709	27,652	,, 26	2,021,248	741	3,011	2,025,000	361	*250/7*	2,025,361

WEEKLY RETURN*

during the year 1956

(000's omitted)

10	11	12	13	14	15	16	17	18	19
Banking Department									
Liabilities					Assets				Total of Liabilities or Assets
Capital (£14,553) and Rest	Deposits				Govt. Securities	Discounts and Advances	Other Securities	Reserve (Notes and Coin)	
	Public‡	Bankers	Other Accounts						
£	£	£	£		£	£	£	£	£
18,175	26,030	231,509	72,487	Jan. 4	204,475	72,360	17,007	54,360	348,202
18,210	20,950	228,114	69,573	,, 11	250,285	28,535	16,991	41,037	336,848
18,251	15,788	232,871	70,299	,, 18	246,045	32,940	17,059	41,164	337,208
18,314	15,225	236,931	70,213	,, 25	234,790	32,780	17,120	55,992	340,682
18,318	18,244	233,625	73,220	Feb. 1	247,802	21,520	17,158	56,927	343,407
18,354	12,740	234,392	70,585	,, 8	237,372	31,322	17,072	50,305	336,071
18,390	16,213	249,754	70,693	,, 15	265,802	23,812	17,005	48,432	355,050
18,427	13,744	242,026	71,514	,, 22	248,537	32,152	16,673	48,350	345,711
18,464	13,694	258,100	70,915	,, 29	283,832	20,592	16,988	39,760	361,172
18,482	14,465	226,657	69,310	Mar. 7	256,422	28,803	17,195	26,494	328,914
18,497	15,100	242,887	69,925	,, 14	290,277	19,316	17,210	19,607	346,410
18,504	13,736	250,889	73,486	,, 21	260,522	17,426	17,219	61,450	356,616
18,532	14,257	243,274	76,481	,, 28	276,682	22,091	17,494	36,276	352,543
18,549	18,898	225,082	79,255	Apr. 4	276,562	23,510	16,615	25,097	341,784
17,711	19,068	231,268	72,476	,, 11	272,207	25,831	15,265	27,219	340,523
17,746	25,963	213,931	77,822	,, 18	251,617	33,416	15,176	35,253	335,462
17,754	15,911	225,942	76,395	,, 25	254,852	30,252	15,522	35,377	336,002
17,821	12,894	231,890	76,278	May 2	270,987	23,887	15,437	28,573	338,883
17,836	19,723	228,333	74,576	,, 9	278,562	24,792	15,480	21,634	340,467
17,906	22,800	219,896	74,919	,, 16	291,762	12,002	15,416	16,341	335,521
17,955	16,278	222,760	75,348	,, 23	267,257	15,062	15,467	34,555	332,341
17,964	14,474	231,594	77,017	,, 30	277,952	15,347	16,689	31,061	341,048
18,006	11,697	224,735	75,844	June 6	273,342	9,193	16,492	31,255	330,282
18,014	14,188	226,352	75,595	,, 13	280,727	9,698	16,667	27,056	334,148
18,070	18,386	231,771	80,112	,, 20	293,327	15,188	16,618	23,205	348,339
18,108	10,722	238,223	78,472	,, 27	254,357	15,585	16,651	58,933	345,525
18,145	8,941	233,897	72,840	July 4	214,557	56,445	17,039	45,782	333,823
18,180	19,021	229,320	74,007	,, 11	270,017	23,615	17,010	29,888	340,529
18,239	19,026	207,788	72,255	,, 18	221,887	16,515	17,202	61,705	317,308
18,271	12,134	215,620	71,267	,, 25	237,097	23,835	17,024	39,336	317,292
18,307	14,393	200,671	73,872	Aug. 1	249,312	17,945	16,878	23,108	307,243
18,343	16,699	189,122	70,295	,, 8	216,802	36,185	16,824	24,648	294,459
18,382	30,290	216,202	73,507	,, 15	247,687	22,395	16,976	51,322	338,380
18,415	13,139	219,977	70,483	,, 22	258,092	19,040	16,952	27,930	322,013
18,450	13,010	222,400	72,147	,, 29	237,427	26,600	17,136	44,845	326,007
18,479	17,541	209,170	68,890	Sept. 5	256,557	13,680	17,177	26,666	314,080
18,495	16,772	226,954	70,615	,, 12	260,767	20,465	17,231	34,374	332,837
18,516	220,06	225,426	71,669	,, 19	252,647	23,073	17,336	44,561	337,616
18,532	12,944	230,903	72,359	,, 26	252,322	18,302	17,417	46,697	334,738
18,549	14,696	229,445	74,971	Oct. 3	260,407	14,623	16,722	45,909	337,661
17,685	18,701	232,263	74,070	,, 10	263,387	18,442	16,765	44,124	342,718
17,727	19,458	237,269	71,352	,, 17	263,807	16,595	16,667	48,737	345,806
17,814	11,431	242,226	72,396	,, 24	254,427	19,575	16,646	53,221	343,868
17,819	15,072	256,595	73,254	,, 31	268,327	28,175	16,749	49,490	362,741
17,857	11,244	238,793	71,048	Nov. 7	254,352	27,215	16,931	40,445	338,942
17,918	17,413	238,084	72,135	,, 14	280,957	12,910	16,905	34,780	345,551
17,936	12,633	239,348	73,444	,, 21	276,302	17,150	17,012	32,898	343,362
17,958	13,809	220,049	75,831	,, 28	262,852	26,755	17,030	21,011	327,647
17,984	13,662	220,474	73,374	Dec. 5	254,312	14,455	17,068	39,659	325,494
17,989	17,577	209,584	73,271	,, 12	239,132	11,165	17,045	51,079	318,421
18,067	12,868	200,464	76,539	,, 19	254,092	6,605	16,920	30,322	307,939
18,112	11,595	203,612	74,909	,, 26	250,917	10,981	16,751	29,580	308,228

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* Bank Rate was 4½ per cent. until February 15th; 5½ per cent. thereafter.
† Valuation, per oz. fine, given in italics.
‡ Including Exchequer, Savings Banks, Commissioners of National Debt, and Dividend Accounts.

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ANALYSIS OF GEOPHYSICAL TIME SERIES

[Papers read before a Joint Meeting of the ROYAL STATISTICAL SOCIETY and the ROYAL METEOROLOGICAL SOCIETY, held on May 15th, 1957, Professor E. S. PEARSON, C.B.E., President of the ROYAL STATISTICAL SOCIETY, in the Chair.]

An Analysis of the Slower Temperature Variations at Kew Observatory by Means of Mutually Exclusive Band Pass Filters (J. M. CRADDOCK) 387

Notes on the Specification of Atmospheric Turbulence (H. CHARNOCK) 398

Some Applications of Time Series Analysis to Atmospheric Turbulence and Oceanography (S. RUSHTON and J. NEUMANN) 409

AN ANALYSIS OF THE SLOWER TEMPERATURE VARIATIONS AT KEW OBSERVATORY BY MEANS OF MUTUALLY EXCLUSIVE BAND PASS FILTERS

By J. M. CRADDOCK

1. THIS paper is concerned with the application of a method of time-series analysis to a meteorological problem. I first show by examples that the technique is capable of separating fluctuations on various time scales, and then discuss a statistical problem which arises in setting up the technique.

Suppose we have a series of observations referring to unit intervals of time. Then by straightforward harmonic analysis we can show that the series can be represented exactly by the sum of sine waves with frequencies spread evenly over the range $(0, \frac{1}{2})$. Moreover, if the series is made up of numbers drawn at random from a normal distribution, then the amplitudes of all the harmonic components are on average equal.

Thus we can investigate the properties of a time series by finding how the variance or power of the harmonic components which represent the series varies as a function of frequency. On the null hypothesis that the series is made up of independent random numbers the power of all components should be equal, within sampling limits, so that the existence of organization in the series may be shown by the presence of peaks exceeding sampling limits in the power spectrum. The usual method of carrying out a power spectrum analysis was used by G. I. Taylor (1938) and consists in a correlogram analysis followed by a Fourier transformation. The present method consists in transforming the series by means of a linear operator which preserves the harmonic components which lie in some section of the range of frequencies $(0, \frac{1}{2})$ while destroying all components whose frequencies lie outside that section, and measuring the variance of the retained harmonics. Such a linear operator is called a filter and it is possible to produce what will be called an orthogonal set of filters such that:

(1) any oscillation on a frequency in the range $(0, \frac{1}{2})$ is passed mainly by one member of the set, and very little by any other member,

(2) if the filters are applied in turn to the same series of random numbers then the output series from any one filter of the set is almost uncorrelated with the output series from any other, and

(3) the algebraic sum of the output series from all members of the set is identical with the original series.

By measuring the variance of each output series we effectively measure the power in each of a number of subdivisions of the frequency range $(0, \frac{1}{2})$ and thus are able to construct an approximate power spectrum. Looked at purely from this standpoint, the method is less efficient than G. I. Taylor's, but it has the advantage that we can examine the actual variations taking place in any interesting frequency band instead of having merely an estimate of the average power.

2. The filters used, which will be denoted by $(w_0, w_1, \ . \ . \ . \ , w_k)$ are symmetric weighted moving averages. If $x_1x_2,$ . . . , x_n is the original series, then y_n, a typical term of the output series, is given by

$$y_n = \sum_{j=0}^{k} \frac{1}{2} w_j(x_{n-j} + x_{n+j}) \quad . \quad . \quad . \quad . \quad . \quad . \qquad (1)$$

If now

$$x = A \cos (2\pi f t + \varphi) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (2)$$

then

$$y_n = A \cos (2\pi f t + \varphi) \ . \ \sum_{j=0}^{k} w_j \cos 2\pi j f \quad . \quad . \quad . \quad . \quad . \qquad (3)$$

so that the result of filtering a strictly periodic series is to leave the period and phase unchanged and to multiply the amplitude by a factor

$$M(f) = \sum_{j=0}^{k} w_j \cos 2\pi j f \quad . \quad . \quad . \quad . \quad . \quad . \qquad (4)$$

Thus to produce a perfect band-pass filter we require that $M(f)$ should have the value unity at all points within the wanted frequency band, and the value zero at all other points of the range $(0, \frac{1}{2})$. Since the filter $(w_0, w_1, \ . \ . \ . \ , w_k)$ contains only $K + 1$ arbitrary weights, we cannot attain perfection, but instead can assign the value of $M(f)$ at only $K + 1$ frequencies. We can, of course, approach perfection as nearly as we please by taking K large enough. In fact, I worked with $K = 6$, resulting in 13-point filters, of which several approximately orthogonal sets are given in the Appendix.

If $V(x)$, $V_w(y)$ are the variances of the original data assumed to be random, and of the output series from the filter $(w_0, w_1, \ . \ . \ . \ , w_k)$, then

$$V_w(y) = V(x) \ . \left\{ w_0^2 + \frac{1}{2} \sum_{j=1}^{k} w_j^2 \right\} \quad . \quad . \quad . \quad . \quad . \qquad (5)$$

So that if we put

$$P(w) = \left\{ w_0^2 + \frac{1}{2} \sum_{j=1}^{k} w_j^2 \right\}$$

and call it the filter factor,

$$V(x) = \frac{V_w(y)}{P(w)} \quad . \quad . \quad . \quad . \quad . \quad . \qquad (6)$$

Thus we obtain an estimate of the variance of the original series on the null hypothesis by measuring the variance of the output series and dividing by the filter factor: this will be called the normalized variance of the filtered series.

3. If several such estimates are made by applying each filter of an orthogonal set to our data series, then on the null hypothesis the normalized variances should be independent estimates of the same thing, and therefore comparable by means of Snedecor's F-test. It is later suggested that the number of degrees of freedom to be attached to an estimate based on N terms of a filtered series is $N \ . \ P(w)$, and if this is accepted, there is no further difficulty in deciding which are the significant peaks in the power spectrum. However, before considering this point it is interesting to see the method in practice.

I should mention here that filtering is very hard work to do by hand, and that the best way to do it on a large scale is on an electronic computer. The work I describe was done on the Elliott Bros. 401 computer at Rothamsted Experimental Station. My programme is organized to accept a set of data followed by a filter and then to print out the filtered series followed by its mean, variance and standard deviation, and finally the normalized variance and the number of degrees of freedom. The machine then stops, and the operator can apply another filter to the same data, or insert a new set of data, as he wishes.

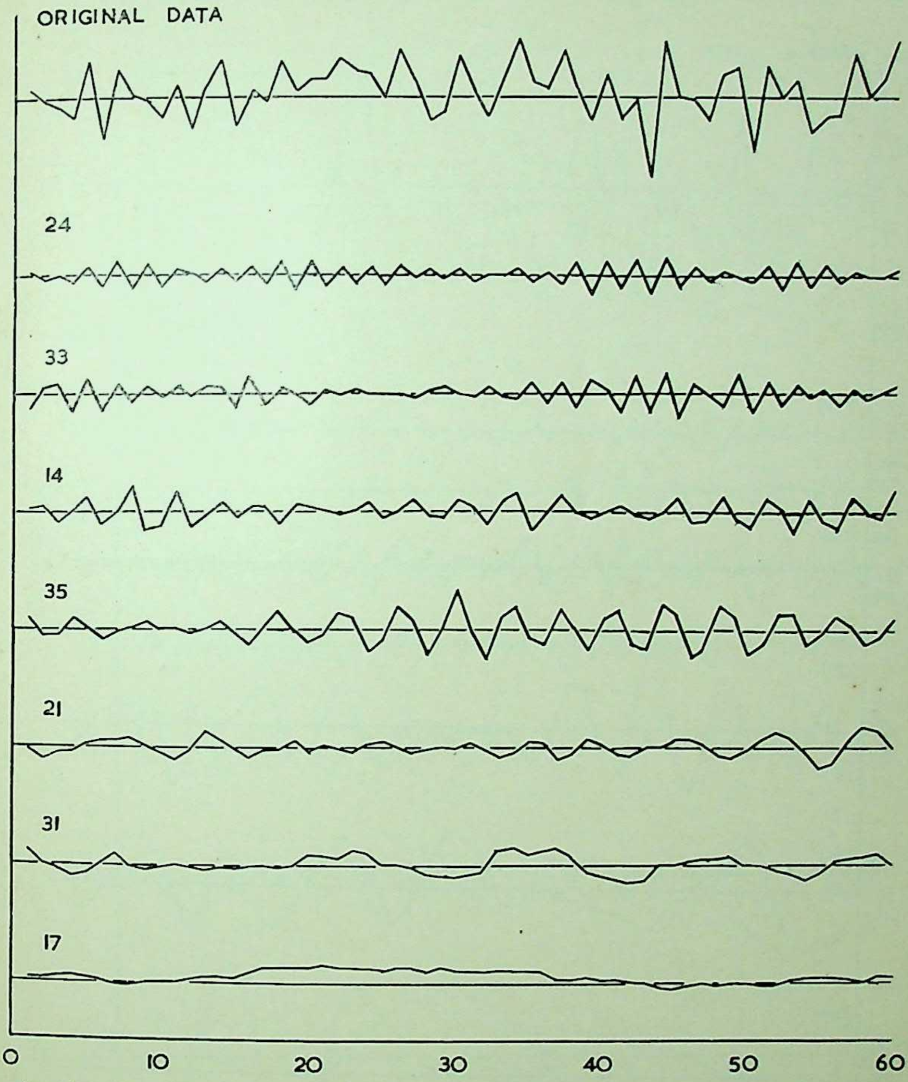

Fig. 1.—A series of numbers drawn at random from a normal distribution (of variance 10) analysed with Filters Nos. 24, 33, 14, 35, 21, 31 and 17.

TABLE 1

Filter Analysis of Random Numbers Drawn from a Normal Distribution

Filter No.	*Peak Frequency*	*Filter Factor*	*Normalized Variance,*	*Degrees of Freedom*
17	0	·0798	8·5	8
31	$\frac{1}{12}$	·1527	10·5	15
21	$\frac{1}{6}$	·1528	6·3	15
35	$\frac{1}{4}$	·1528	12·2	15
14	$\frac{1}{3}$	·1528	10·8	15
33	$\frac{5}{12}$	·1527	13·8	15
24	$\frac{1}{2}$	·0798	14·1	8

Table 1 shows the results of analysing a set of random numbers, drawn from a number distribution, by means of seven approximately orthogonal filters. These numbers are known to have variance 10, and the estimates obtained are all quite in agreement with this and with each other. Most of the original series and the seven filtered components are shown in Fig. 1. On the null hypothesis the amplitudes of the second to sixth filtered series should be about equal, while those of the first and last filtered series should be less.

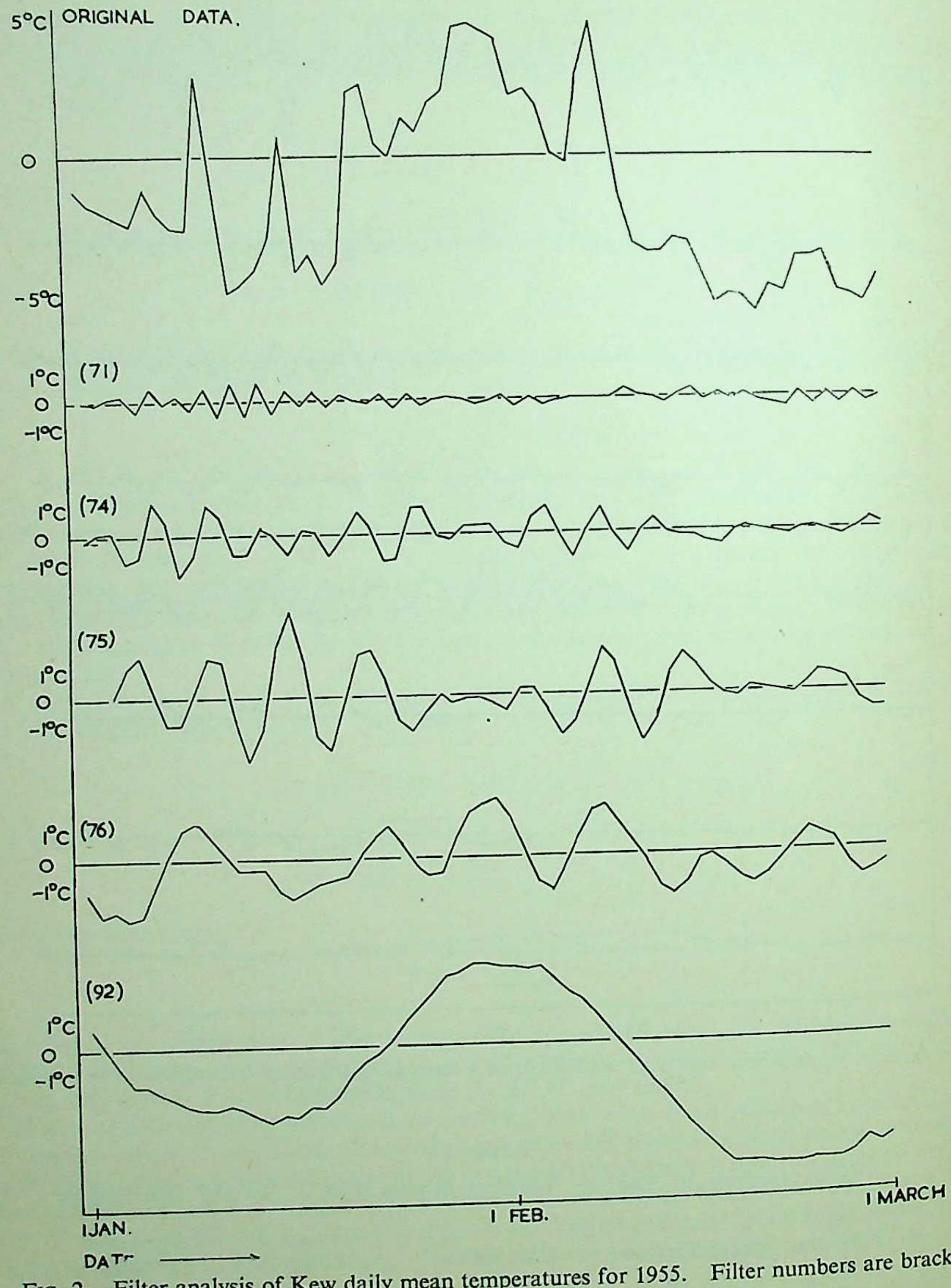

FIG. 2.—Filter analysis of Kew daily mean temperatures for 1955. Filter numbers are bracketed. Units are degrees C.

4. Table 2 shows the result of a similar analysis carried out on Kew daily mean temperatures for 1955 and Fig. 2 gives a two-month section of five out of the seven filtered series. (I have omitted the output series from filters nos. 72 and 73 to save space.)

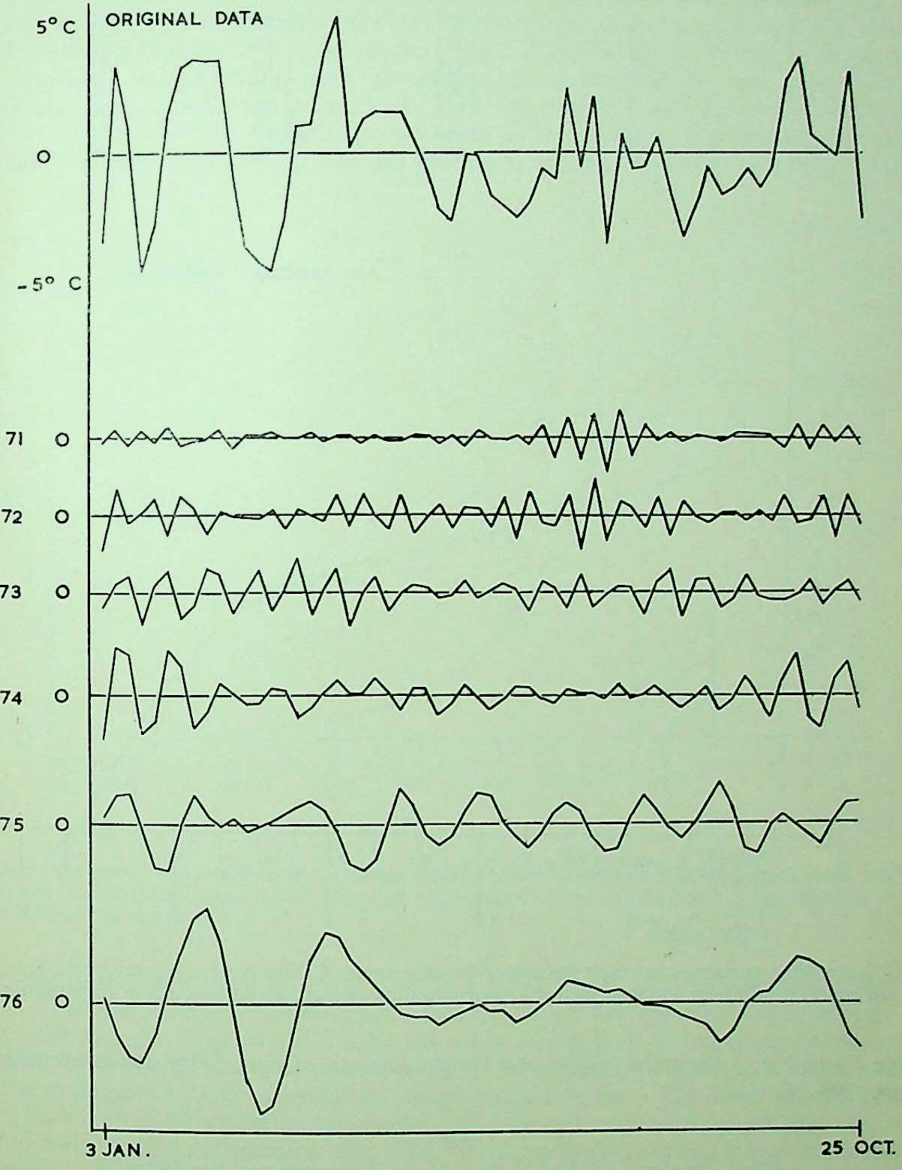

FIG. 3.—Filter analysis of Kew daily mean temperatures for January to October, 1946. Middle dates of 5-day periods are given. Filter numbers are bracketed. Units are degrees C.

The output series from the filters passing the low frequency components have much larger amplitudes, relatively, than they had when the series of random numbers was analysed and we require a method of deciding whether the observed amplitudes are likely to arise on the null hypothesis. My own suggestion is to use the *F*-test, but statisticians may be able to suggest other ways of doing the same thing.

TABLE 2

Filter Analysis of Kew Daily Mean Temperatures for 1955

Filter No.	*Peak Frequency*	*Filter Factor*	*Normalized Variances, °C.²*	*Degrees of Freedom*
71	$\frac{1}{2}$	·0852	0·95	31
72	$\frac{2}{5}$	·1493	1·03	54
73	$\frac{1}{3}$	·1516	1·64	55
74	$\frac{1}{4}$	·1502	2·49	55
75	$\frac{1}{6}$	·1516	5·39	55
76	$\frac{1}{10}$	·1493	13·63	54
92	0	·0852	46·59	31

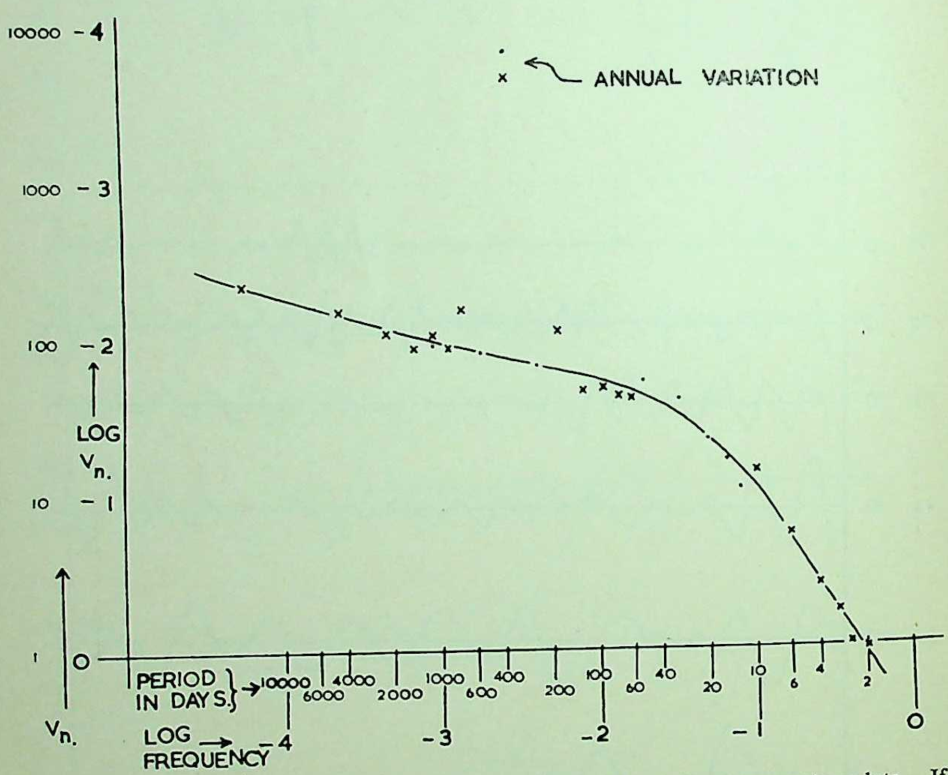

FIG. 4.—Normalized variances in many frequency bands estimated from Kew temperature data. If a series of random numbers was analysed the corresponding points should lie on a horizontal line.

Table 3 and Fig. 3 show the results of a similar analysis of Kew 5-day mean temperatures for the years 1946–50.

TABLE 3

Filter Analysis of Kew 5-day Mean Temperatures for 1946–50

Filter No.	*Peak Frequency*	*Filter Factor*	*Normalized Variance, °C.²*	*Degrees of Freedom*
71	$\frac{1}{2}$	·0852	2·79	31
72	$\frac{2}{5}$	·1493	2·15	54
73	$\frac{1}{3}$	·1516	3·27	55
74	$\frac{1}{4}$	·1502	4·31	55
75	$\frac{1}{6}$	·1516	7·92	55
76	$\frac{1}{10}$	·1493	10·55	54
92	0	·0852	20·48	31

I also carried out an analysis of monthly mean, of three-monthly mean and of annual mean temperatures. All the normalized variances estimated from these analyses are assembled in Fig. 4, which is plotted logarithmically to display the results more clearly. If a similar analysis were carried out on a series of random numbers, the corresponding points would, apart from sampling errors, lie on a horizontal line, so that the Kew values are clearly not in agreement with the null hypothesis. In fact, most of them lie quite close to the curve which I have drawn, and the refined statistical analysis is required not to decide the general course of the curve but to decide whether certain points lying slightly off the curve represent significant deviations or not. Fig. 5 shows the same data as an approximate power spectrum, with the property that the area between any two ordinates, the curve and the *x*-axis is proportional to the power of the oscillations in the frequency band between the ordinates. On this projection the smooth curve (which is the transform of that in Fig. 4) does not seem as good a fit as it did in Fig. 4, and the free-hand dotted curve appears to fit the data better. This suggests that the power spectrum has two peaks at about 10- and 30-day periods. Figs. 6 and 7 show the actual fluctuations in frequency bands including these peaks. Both curves while, of course, being of an oscillatory character, show a good deal of variation in amplitude, and many rather abrupt phase changes. The series are not quite like anything I have seen in the statistical text-books, and I am quite ready to believe that they may be awkward material for the statistician to deal with, but the meteorologist has to take the atmosphere as he finds it.

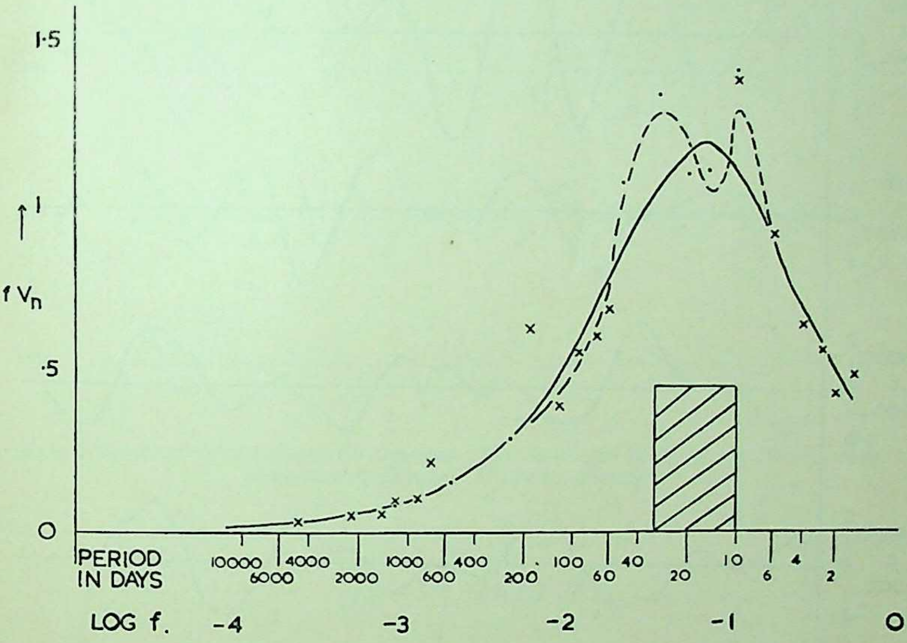

Fig. 5.—Power spectrum analysis for Kew temperatures. Ordinates enclosing equal areas under the graph include frequency bands passing oscillations of equal variance. The shaded area represents a variance of 1° C.^{2}

5. These facts are enough to show that the method of analysis by means of orthogonal band pass filters is capable of producing important results, and that it is worth while to provide it with a sound statistical background. The outstanding difficulty is that mentioned in Section 3, of estimating the number of degrees of freedom to be attached to the variance measured from *N* terms of a filtered series. The point is illustrated by Fig. 8, which shows the response curves of a perfect filter and of the sort of filter which can be achieved in practice.

Suppose we have *N* terms of a filtered series obtained by operating on a series of random numbers by means of the perfect filter with the response curve in Fig. 8 (1) and let this have N_1

degrees of freedom. Now we can represent the random series by N harmonic components of equal amplitude of which the frequencies can be regarded as points equally spaced along the segment GH, while the effect of filtering is to preserve components corresponding to points on the segment CD while removing those corresponding to points on GC and on DH. Then it seems clear to me that

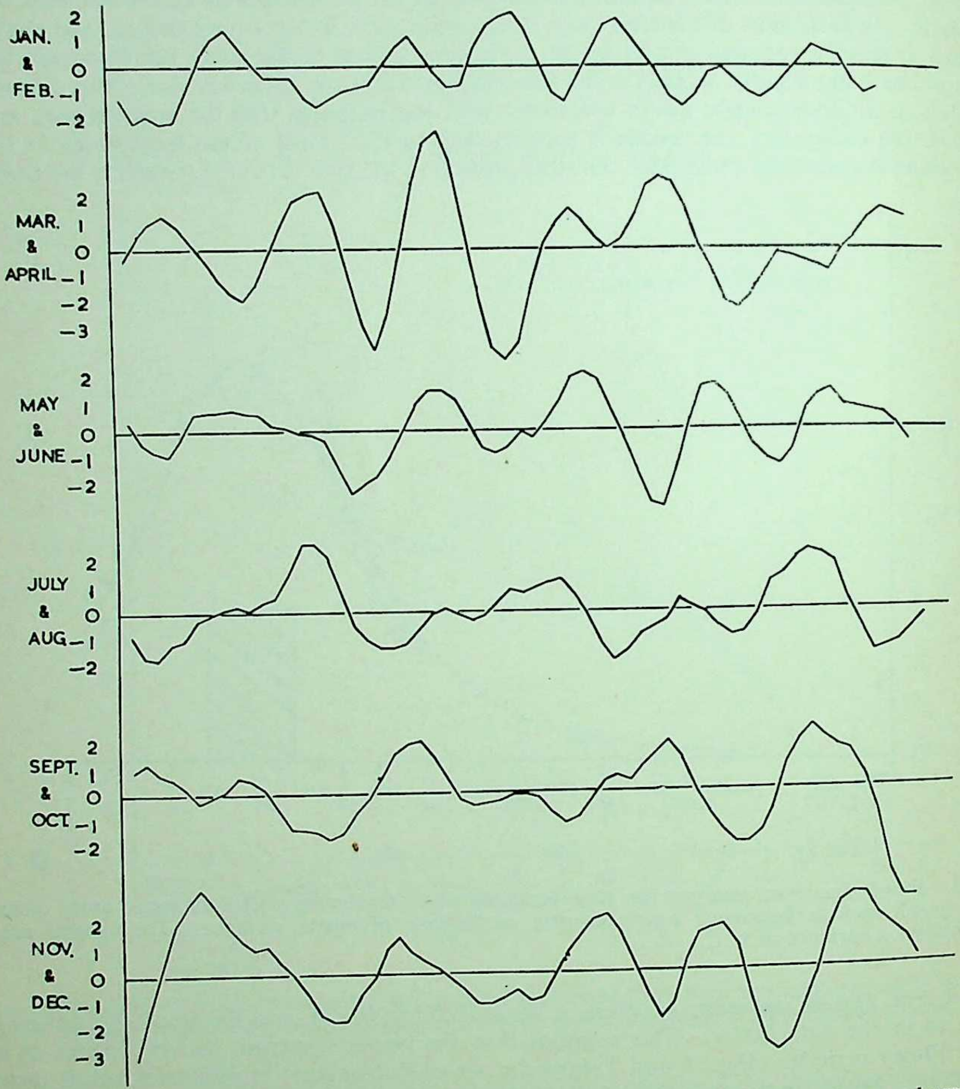

FIG. 6.—Kew daily temperatures for 1955 filtered to show the 10-day wave in its true phase and amplitude. Units are degrees C.

$$\frac{N_1}{N} = \frac{CD}{GH} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (7)$$

Now for the perfect filter

$$\frac{CD}{GH} = 2 \int_0^{\frac{1}{2}} M(f)\, df \quad . \quad . \quad . \quad . \quad . \quad . \qquad (8)$$

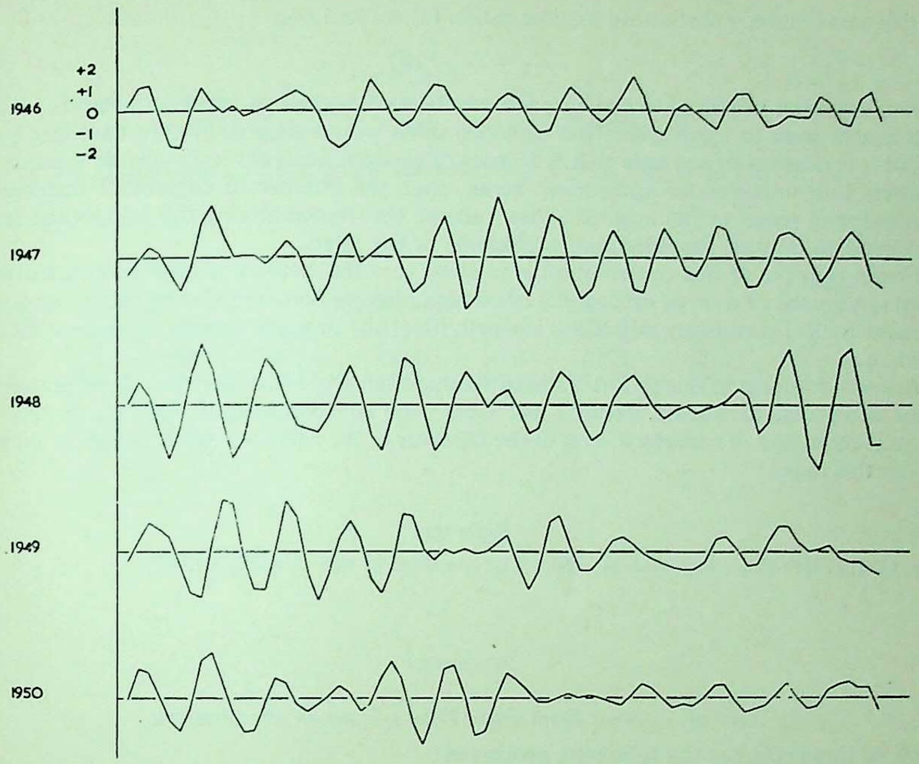

FIG. 7.—Kew 5-day mean temperatures for the year 1946–50 filtered to show the 30-day wave in its true phase and amplitude. Units are degrees C.

and also

$$\frac{CD}{GH} = 2\int_{0}^{\frac{1}{2}} (M(f))^2\, df \quad . \quad . \quad . \quad . \quad . \quad . \qquad (9)$$

Either of these forms gives the correct result for a perfect filter, and we have to decide which if either is correct for an imperfect filter. Since the presence of any harmonic, whether with positive or reversed amplitude, should contribute positively to the variability, I think that equation (9) is the correct form.

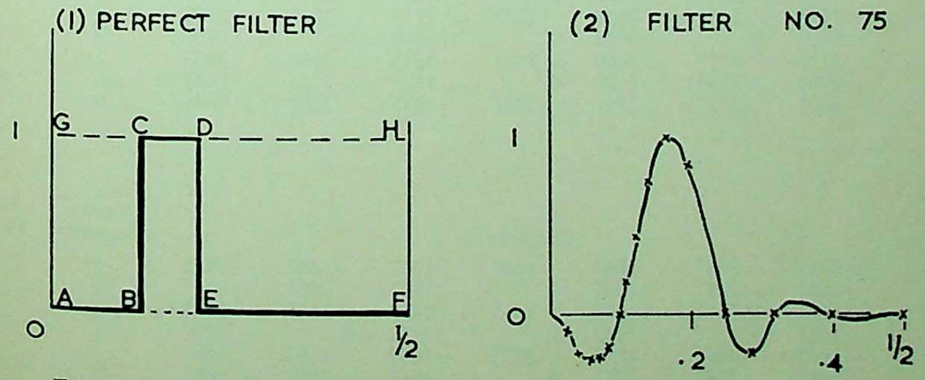

FIG. 8.—Response curves of perfect and actual filters with the same value of $P(w)$.

On this assumption, substituting from equation (4) we find that

$$N_1 = N \,.\, P(w) \qquad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

6. This equation was used to estimate the numbers of degrees of freedom given in Tables 1–3 and the results seem to agree with what common sense would suggest. More than this I do not claim, but it is obvious in any case that if a series of random numbers is divided by a set of orthogonal filters into uncorrelated component series, then the number of degrees of freedom equal to the number of terms of the original series must be distributed among the component series by a rule depending only on the weighting coefficients of the filters.

The main purpose of this contribution is to show that the method of analysing meteorological time series by means of a set of orthogonal band pass filters is potentially valuable, in the hope that some mathematical statistician will think it worth his while to work out rigorously the theoretical background.

7. Acknowledgment is due to Dr. Yates of Rothamsted Experimental Station for permission to use their computer, to Messrs. Healy and Rees, also of Rothamsted, for help in getting the programme right, and in running it, and to the Director of the Meteorological Office for permission to publish this paper.

Reference

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APPENDIX

Sets of 13-*point Band Pass Filters Used in the Analysis*

Each of these sets has the following properties:

(1) when any two of the set are applied in turn to a series obtained by random drawings from a normal distribution the two output series are almost uncorrelated (i.e. the members of the set are mutually almost orthogonal),

(2) each filter has zero acceptance at the peak frequencies of all other members of the set and unit acceptance at its own peak frequencies, and

(3) when all members of the set are applied to one data series in turn, the algebraic sum of all the output series is equal to the original series.

Set 1

Filter Number	*Coefficients*							*Peak Frequency*	*Filter Factor*
71	·1063	—·2127	·1897	—·1667	·1437	—·1206	·0603	$\frac{1}{2}$	·0852
72	·1708	—·2764	·1708	0	—·1708	·2764	—·1708	$\frac{2}{5}$	·1493
73	·1273	—·1273	—·2060	·3333	—·1273	—·2060	·2060	$\frac{1}{3}$	·1516
74	·1910	0	—·3090	0	·3090	0	—·1910	$\frac{1}{4}$	·1502
75	·1273	·1273	—·2060	—·3333	—·1273	·2060	·2060	$\frac{1}{6}$	·1516
76	·1708	·2764	·1708	0	—·1708	—·2764	—·1708	$\frac{1}{10}$	·1493
92	·1065	·2127	·1897	·1667	·1435	·1206	·0603	0	·0852

Set 2

Filter Number	*Coefficients*							*Peak Frequency*	*Filter Factor*
24	·0833	—·1667	·1667	—·1667	·1667	—·1667	·0833	$\frac{1}{2}$	·0798
33	·1667	—·2887	·1667	0	—·1667	·2887	—·1667	$\frac{5}{12}$	·1527
14	·1667	—·1667	—·1667	·3333	—·1667	—·1667	·1667	$\frac{1}{3}$	·1528
35	·1667	0	—·3333	0	·3333	0	—·1667	$\frac{1}{4}$	·1528
21	·1667	·1667	—·1667	—·3333	—·1667	·1667	·1667	$\frac{1}{6}$	·1528
31	·1667	·2887	·1667	0	—·1667	—·2887	—·1667	$\frac{1}{12}$	·1527
17	·0833	·1667	·1667	·1667	·1667	·1667	·0833	0	·0798

Set 3

Filter Number	*Coefficients*							*Peak Frequency*	*Filter Factor*
40	·2500	−·1667	−·1667	−·1667	·5000	−·1667	−·0833	$\frac{1}{4}, \frac{1}{2}$	·2465
41	·3333	−·4553	0	·3333	−·3333	·1220	0	$\frac{1}{3}, \frac{5}{12}$	·3334

These two filters form an orthogonal set with Nos. 21, 31 and 17 of Set 2.

Set 4

Filter Number	*Coefficients*							*Peak Frequency*	*Filter Factor*
114	·3317	·4581	−·0016	−·3333	−·3317	−·1248	·0016	$\frac{1}{6}, \frac{1}{12}$	·3334
115	·5874	−·6293	−·1625	·1666	·1626	−·0373	−·0875	$\frac{1}{2}, \frac{2}{5}, \frac{1}{3}, \frac{1}{4}$	·5879
117	·0809	·1712	·1641	·1667	·1691	·1621	·0859	0	·0797

NOTES ON THE SPECIFICATION OF ATMOSPHERIC TURBULENCE

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Introduction

IT is well known that fluid flow on a large scale is almost always turbulent; the flow varies with time in a more or less complicated way and instantaneous values of the variables are not determined by the boundary conditions.

Turbulence theorists attempt to determine the various statistics of the variables, given suitable boundary and initial conditions, on the basis of the Navier-Stokes equations and the equation of continuity. The Navier-Stokes equations have been verified experimentally for non-turbulent flow and are assumed by most workers to be valid for turbulent flow also. A simplified version can be written

$$\frac{\partial u_i}{\partial t} + n_j \frac{\partial u_i}{\partial x_j} + \frac{1}{\rho}\frac{\partial p}{\partial x_i} - \frac{\partial}{\partial x_j}\left(\nu \frac{\partial u_i}{\partial x_j}\right) = X_i \quad . \quad . \quad . \quad . \quad . \quad (1)$$

u_i is the i^{th} component of velocity, p is the local instantaneous pressure, and ρ is the local density. ν is the kinematic viscosity and X_i the i^{th} component of an external body force such as gravity.

The combination of non-linearity with the complicated fluctuations makes the search for general solutions unattractive, so the complexity is reduced by following the technique described by Reynolds (1895) of averaging over intervals of magnitude T to get

$$\frac{\partial \bar{u}_i}{\partial t} + \bar{u}_j \frac{\partial \bar{u}_i}{\partial x_j} + \frac{1}{\rho}\frac{\partial \bar{p}}{\partial x_i} + \frac{\partial}{\partial x_j}\overline{(u_i'u_j')} - \frac{\partial}{\partial x_j}\left(\nu \frac{\partial \bar{u}_i}{\partial x_j}\right) = \bar{X}_i \quad . \quad . \quad . \quad . \quad (2)$$

Here we have assumed incompressible fluid, and u' is defined by $u = \bar{u} + u'$ where the overbar denotes a mean value similar to that defined by Reynolds, i.e.

$$\bar{u}_i = \frac{1}{T}\int_T u_i \, dT \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

T can be a length, area or volume, but for our present purpose we can take it as a time. T is chosen so that $\bar{u}'$ converges to zero as T approaches some value small compared with the total extent of the flow.

The terms $\overline{u_i'u_j'}$, the Reynolds stresses, now constitute six further unknowns and progress is only possible in special cases when physical assumptions can be made, assumptions such as that of similarity or concerning the detailed structure of the flow. We shall refer to terms like $\overline{u_i'u_i'}$ as intensities and terms like $\overline{u_i'u_j'}$ as fluxes since they can be interpreted as measures of the turbulent eddy energy and the turbulent eddy flux of momentum.

The turbulence can transfer momentum in a direction perpendicular to the mean flow and down a mean velocity gradient, so that many workers have found it convenient to use a fictitious eddy viscosity K_{ij} defined by

$$-\overline{u_i'u_j'} = K_{ij}\frac{\partial \bar{u}_i}{\partial x_j} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

It seems likely that this can lead to a physical picture of the flow which is unprofitable. A better course is to link the Reynolds stresses with the pressure terms, when it becomes clear that they express the effect of any anisotropy of the turbulence on the flow. These comparatively recent ideas are well described by Stewart (1956).

The notion of an eddy viscosity is inserted here as an example of the general "turbulence" method, which is to relate statistical properties of the smaller scale flow to the larger scale or mean flow.

Some aspects of turbulent flow on the laboratory scale are now well understood, though many problems remain. Several of the ideas and concepts have been used to interpret the larger scale turbulence of the atmosphere and the ocean, but there are many more which are potentially useful if only we could confidently apply them.

In the following sections we first show that a formal treatment of the problem is inadequate from an observational point of view, and then describe some of the practical methods by which the problems are being explored.

Reynolds Axioms

We are concerned with real, and therefore with non-stationary, time-series. Nevertheless there are many which can be treated as stationary for most purposes. Examples are to be found in laboratory turbulence measurements, where one can almost always select a suitable value of T in equation (3).

Atmospheric motions, however, have such a broad range of scale that there is not in general any obvious choice of the averaging time T. Their statistics fluctuate widely from year to year and from minute to minute, the fine structure of one scale being the mean motion of a smaller scale and so on. The problem is then not so much to determine the statistics of the motion given the boundary conditions as to relate the statistics appropriate to one scale with those appropriate to another. From this point of view turbulence is a "mental artefact" and the extra terms in the equations which allow for the effects of smaller scale motions are a "compensation for smoothing" (Richardson and Gaunt (1930)).

For such non-stationary series time-means and stochastic averages are not equivalent, and it is of some interest to find the conditions under which the Navier-Stokes equations (1) average to the Reynolds equations (2). The conditions which the mean value must satisfy have been formulated by Kampé de Fériet (1934, 1935) and are known as Reynolds axioms. They are

(a) $\widetilde{f+g} = \tilde{f} + \tilde{g}$

(b) $\widetilde{af} = a\tilde{f}$, a being a constant

(c) $\widetilde{\tilde{f}g} = \tilde{f}\tilde{g}$

(d) $\widetilde{\lim f_n} = \lim \tilde{f}_n$ to allow,

where f and g represent flow variables and f_n a suitable sequence of functions to allow, for example,

$$\widetilde{\frac{\partial u}{\partial t}} = \frac{\partial \tilde{u}}{\partial t}$$

We have used the curly overbar to distinguish the averaging technique from that defined by equation (3).

The problem has been discussed by Isakson (1930), but his work does not appear to be well known to meteorologists. He deals with function of only one independent variable such as time. The mean value is defined by the equivalent of a symmetric linear filter, i.e. by

$$\tilde{u} = \int_{-\infty}^{\infty} D(\tau)\, u(t+\tau)\, d\tau \qquad . \quad . \quad . \quad . \quad . \qquad (5)$$

with

$$D(\tau) = D(-\tau) \text{ and } \int_{-\infty}^{\infty} D(\tau)\, d\tau = 1$$

It is assumed that both $u(t)$ and $D(\tau)$ can be written as Fourier integrals:

$$u(t) = \int_0^\infty A(\omega) \cos(\omega t - l)\, d\omega, \quad D(\tau) = \frac{1}{\pi}\int_0^\infty W(\omega) \cos \omega t\, d\omega$$

and then

$$\tilde{u} = \int_0^\infty W(\omega) A(\omega) \cos(\omega t - l)\, d\omega \quad . \quad . \quad . \quad . \quad (6)$$

Repeating the averaging process, one gets

$$\tilde{\tilde{u}} = \int_0^\infty [W(\omega)]^2 A(\omega) \cos(\omega t - l)\, d\omega \quad . \quad . \quad . \quad . \quad (7)$$

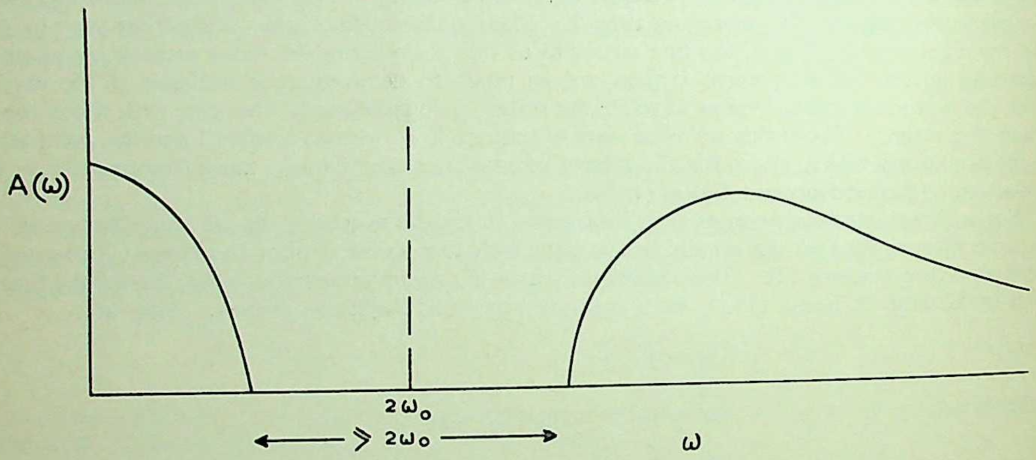


FIG. 1.—Spectrum with gap such that Reynolds axioms can be satisfied by using an ideal low-pass filter.

Clearly Axiom (c) with $g = 1$ is only satisfied in general if $W(\omega) = 0$ or 1. Isakson then limits himself to the case when

$$\begin{aligned} W(\omega) &= 1, \quad 0 < \omega < 2\omega_0 \\ &= 0, \quad \omega > 2\omega_0 \end{aligned} \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

where ω_0 is arbitrary.

The averaging process now implies the removal of the components of the spectrum down to $2\omega_0$.

This method of defining the mean value has some advantages. Unfortunately it is not a complete solution, for consider Axiom (c) in the form $\tilde{\tilde{u}}\tilde{v}' = 0$ and $\tilde{\tilde{u}}\tilde{\tilde{v}} = \tilde{u}\tilde{v}$. For the first expression to hold, it is necessary that there is a gap of $2\omega_0$ or more between the least frequencies of v' and the highest frequencies of $\tilde{u}$. For the second, it is necessary that the highest frequencies of $\tilde{u}$ and $\tilde{v}$ do not extend as far as ω_0. So this particular averaging method satisfies the Reynolds axioms only in the case of variables with a particular gap in the spectrum, as illustrated in Fig. 1. It is presumably the existence of such a gap between the molecular motion and the turbulent motion which allows us to use the Navier-Stokes equations. Extensions of the treatment can be found in Isakson's papers.

From the theoretical point of view, progress can be made (according to Kampé de Fériet, 1949) by reformulating the problem in terms of abstract algebra. Since the Isakson treatment considers the use of any linear filter (the use of asymmetric weighting functions with

$$D(\tau) \neq D(-\tau)$$

only complicates the analysis by introducing phase relations) we can perhaps assume that there is not to be any simple experimental solution. This is perhaps not unexpected nor, at this stage, to be regretted since any treatment of the Reynolds equations must rely on approximations based on physical argument or observation.

The remainder of this paper will describe some of the methods by which meteorologists are attempting to make meaningful analyses of observed time-series.

Analysis of Meteorological Series

(a) *Discontinuous Averages*

In the absence of theoretical guidance most meteorologists approach the problem of atmospheric turbulence as cautious observers. The practical importance of the subject is due to the effect of the turbulence in transporting mass, momentum and energy, and we can illustrate the various methods in use by an example which is of some meteorological interest—the meridional transport of zonal momentum.

We shall suppose that we have some device which measures U, the zonal component, and V, the meridional component, of the wind. We use U and V to distinguish the observed values from u and v, the instantaneous values of equation (1), noting that U and V can be made to approach u and v by (as yet) hypothetical instrumental development. Present "instruments" are relatively large but we can make a reasonable estimate of the volume of the atmosphere to which the measurements refer. Similarly we can assume that we know the frequency response of the instrument which, without great loss of generality, we can take to be uniform to some "cut-off" frequency and zero for higher frequencies. This implies that if we have records extending from say $t_1 - T/2$ to $t_1 + T/2$ they can be replaced without loss by say N equally spaced discrete observations.

Then we can calculate

$$\frac{1}{T}\int_{t_1-T/2}^{t_1+T/2} UV\,dt \text{ or } \frac{1}{N}\sum_{1}^{N} U_iV_i = \overline{UV} \text{ say} \quad . \quad . \quad . \quad . \quad (9)$$

and this, if we assume constant density, is a measure of the average meridional flux of zonal momentum during the period of observation, through the volume and at the place represented by the instrument, brought about by air motions of the known time-scale.

This may seem an unnecessarily cautious point of view, but it can be generalized only by further observation or by physical argument.

The usual meteorological technique, endorsed by Priestley and Sheppard (1952), now defines the eddy flux of momentum, ρF_E, by

$$F_E = \overline{(U - \overline{U})(V - \overline{V})}$$

and the advective flux, ρF_A, by

$$F_A = \overline{U}\,\overline{V} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

Here the overbar is defined as a time average over T as in equation (9).

As a method for determining the structure of air motions, the technique seems unnecessarily restricted. If we regard the observations from $t_1 - T/2$ to $t_1 + T/2$ as one sample of a possible series, then the method is seen to be related to the Reynolds technique but with non-overlapping and so discontinuous averages. The discontinuous averages are a disadvantage of the method; its advantage is that it deals with the average flux over a given interval and so gives up the idea of using the equations of motion other than on the more realistic finite difference or "energy budget" basis.

With this understood, we have the opportunity of extending our definition of the eddy and the advective flux by some form such as

$$F_E = \overline{(U - \tilde{U})(V - \tilde{V})}$$

$$F_A = \overline{\tilde{U}\tilde{V}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (11)$$

Again the overbar denotes a time average over T, but the curly overbar denotes any other way in which we choose to separate motions of smaller from those of larger time-scale. In general $\overline{(\tilde{U}(V - \tilde{V})}$ will not vanish; the magnitude of such terms is a matter for physical explanation.

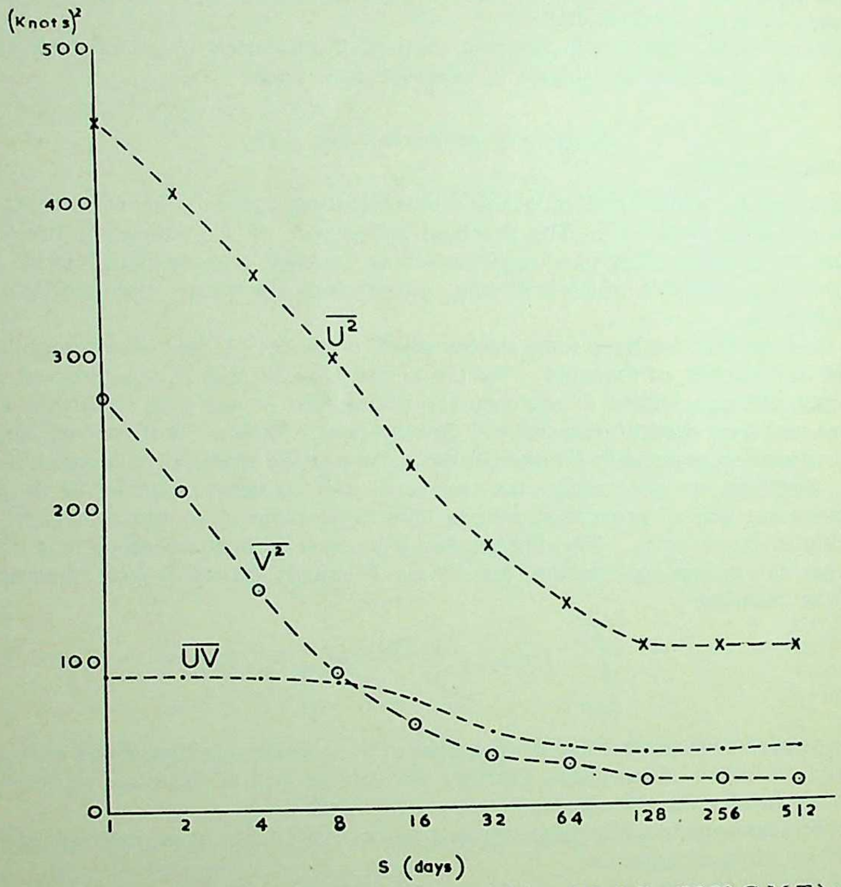


FIG. 2.—Statistics obtained by subdivision of series containing 512 daily (06.00 G.M.T.) values of zonal (U) and meridional (V) wind components at 700 mb at Larkhill, 20.iv.46 to 13.ix.47.

The ordinate is the value of

$$\frac{1}{NS} \sum_{i=as+1}^{s(a+i)} U_i \sum_{i=as+1}^{s(a+i)} V_i$$

and similarly for U_i^2 and V_i^2 (see equation (12)).

One method of this type, already used by several meteorologists, involves the subdivision of the N observations from $t_1 - T/2$ to $t_1 + T/2$ into say m equal parts. The eddy flux for each part is then computed as in equation (10) and the average flux determined.

The computations are of the form

$$F_{UV}(s) = \frac{1}{N} \sum_{a=0}^{m-1} \left[\sum_{i=as+1}^{s(a+1)} U_i V_i - \frac{1}{s} \sum_{i=as+1}^{s(a+1)} U_i \sum_{i=as+1}^{s(a+1)} V_i \right] \quad . \quad . \qquad (12)$$

It is convenient here to give an example from an investigation, still in progress, in which a particular set of meteorological series is being analysed by various methods. The observations which will be used in this paper are those of the zonal (U_i) and meridional (V_i) components of wind, measured at 700 mb over Larkhill at 06.00 G.M.T. on each day of 1946 and 1947.

To illustrate the use of equation (12), we have calculated $F_{UV}(s)$, $F_{VV}(s)$ and $F_{UU}(s)$ for

$$s = 1, 2, 4 \quad . \; . \; . \quad 512,$$

using the middle 512 terms of the series U_i and V_i. The results are shown in Fig. 2. One might be tempted to conclude that the major contribution to the intensities (variances) comes from the motions of shorter period, whereas that to the flux (covariance) arises mainly from motions of period 10 to 50 days or so. In his analysis of the same data Priestley (1949) decided, somewhat arbitrarily perhaps, to use non-overlapping means of two months to separate the "eddy" and "advective" fluxes. However, he considered that, for the particular problem with which he was concerned, the advective component had no obvious significance and he did not quote the results.

Our use of the word "period" is an indication of our tendency to think in terms of the trigonometric functions, which have been found so useful as a mathematical description of, among other things, laboratory turbulence. We wish to resolve the motion into components which make additive contributions to the energy of the motion, and it can be shown (according to Batchelor, 1953, p. 29) that the choice of trigonometric functions is in fact necessary if we wish to have a method of representation which is valid for any stationary process. We have been at some pains to emphasize that geophysical time-series are not stationary; nevertheless the convenience of using trigonometric functions is considerable and there is obvious merit in using them until some better way of analysis is developed.

For this and other reasons the curves of Fig. 2 are often interpreted as a crude form of spectral analysis. For stationary processes it is easy to derive the relation between $F(s)$, regarded as a continuous curve, and $C_+(s)$, the even part of the covariogram, viz :

$$\frac{1}{2}\frac{\partial^2}{\partial s^2}\left(s^2 F(s)\right) \approx C(0) - C_+(s) \qquad . \quad . \quad . \quad . \quad . \qquad (13)$$

The cospectra then follow. Unfortunately the determination of a spectrum requires a double differentiation of $F(s)$ with respect to s, so that the method is not to be recommended for direct estimates. Nevertheless $F(s)$ is relatively easy to compute and in special cases the method can be useful. For the particular case of vertical velocity near the ground, Charnock and Robinson (1957) have by this means derived spectra which are in fair agreement with those obtained by other methods. In other cases, when the process is more obviously non-stationary, the technique must be used with discretion. $C_+(s)$ and all the other statistics are variable with averaging period as well as in time.

(b) *Spectral Analysis*

If more computation is possible it may be better to compute spectra directly and this is slowly becoming a recognized meteorological practice. Since a meeting of the Royal Statistical Society was recently devoted to this topic it may suffice to refer to Jenkins and Priestley (1957).

The basic technique is well known; it is to invent some continuation of the record outside $t - T/2$, $t + T/2$, so that a Fourier transform can be taken. One usually either takes the function to be identically zero outside the interval, which gives a continuous spectrum, or assumes the record periodic with period T, which gives a line spectrum. In both cases there is a choice of periodogram (harmonic) analysis or correlation analysis. There is no theoretical advantage in choosing either, but for most physical purposes* it seems to me less confusing to make direct estimates of the spectral density, or of the integrated spectrum. For automatic analysis an analogue device based on repeating the record has been used most successfully in studies of ocean waves (see for example Deacon, 1952).

In general, fluctuations in the amplitude of spectral lines tend to be of the same order as the mean value and the wildly fluctuating appearance of an observed periodogram is well known (Bartlett, 1955, p. 280). We have usually dealt with it by a method which we find is attributed to

* Apart, perhaps, from attempts to forecast using the technique of Wiener (1950).

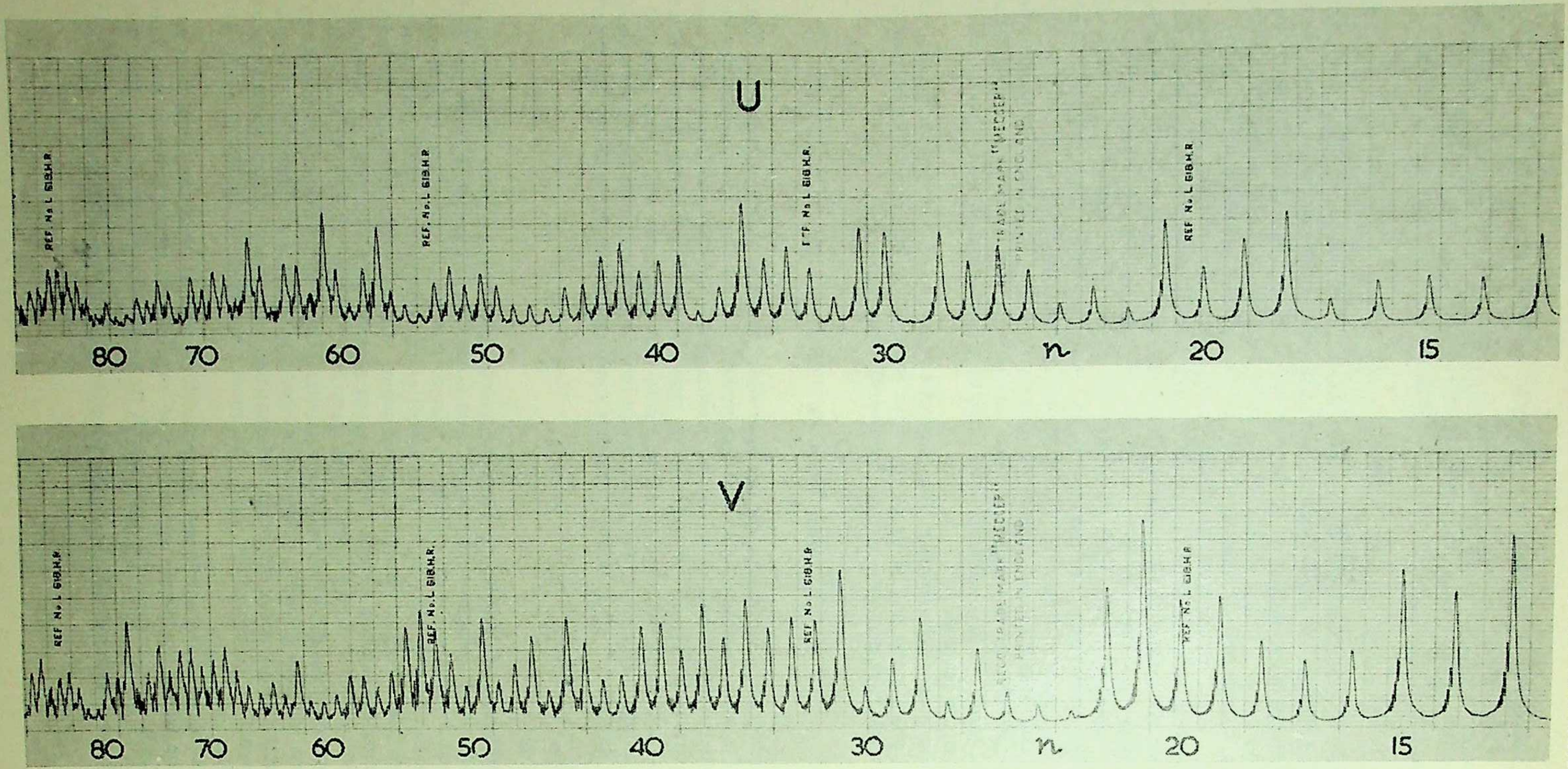


FIG. 3.—Automatic spectral analyses of 730 daily (06.00 G.M.T.) values of zonal (U) and meridional (V) wind components at 700 mb at Larkhill.

The amplitude of each peak corresponds to that of a harmonic component whose period is $1/n$ times 754 days, the length of the record examined.

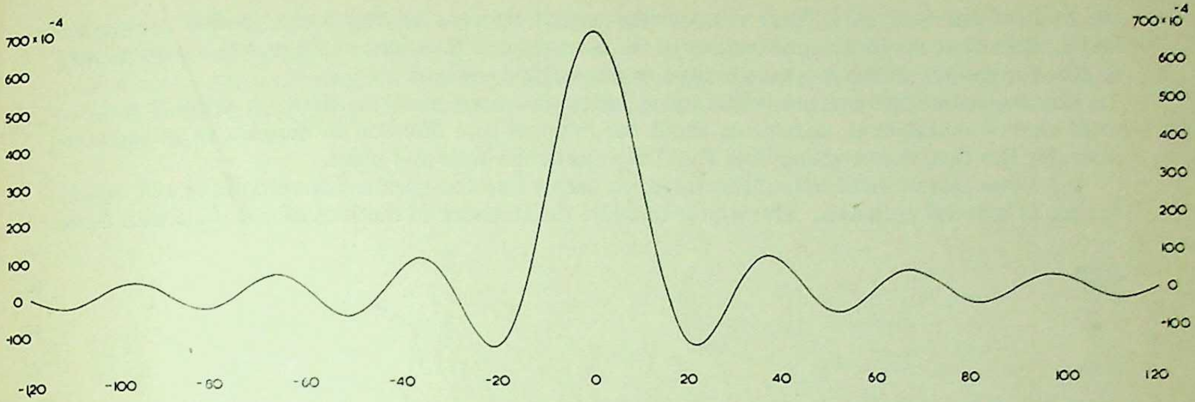


FIG. 4.—Graph of weights for running mean of 241 terms to approximate an ideal low-pass filter with "cut-off" period $T_0 = 30$ days.

P. J. Daniell, and which involves averaging the intensity over a more or less broad band of frequency. Other, numerical, methods due to Bartlett (1950) and Tukey (1949) are being investigated. They appear to work best with uniform spectra, and this has given rise to the practice of "prewhitening" or filtering the original record before the spectral analysis so that it has an approximately uniform spectrum. The spectral estimates are subsequently adjusted.

An automatic Fourier analysis is shown in Fig. 3 and the limitations of the method for records with a broad band of frequency will be obvious. In this case two years of daily values of zonal and meridional winds have been analysed and there is energy at periods up to about 60 days,

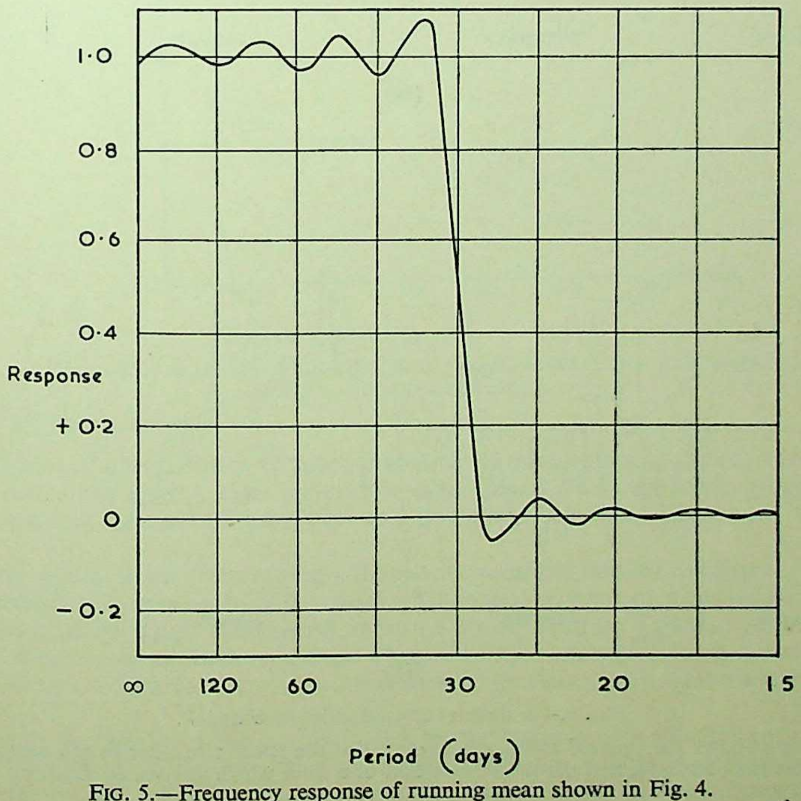
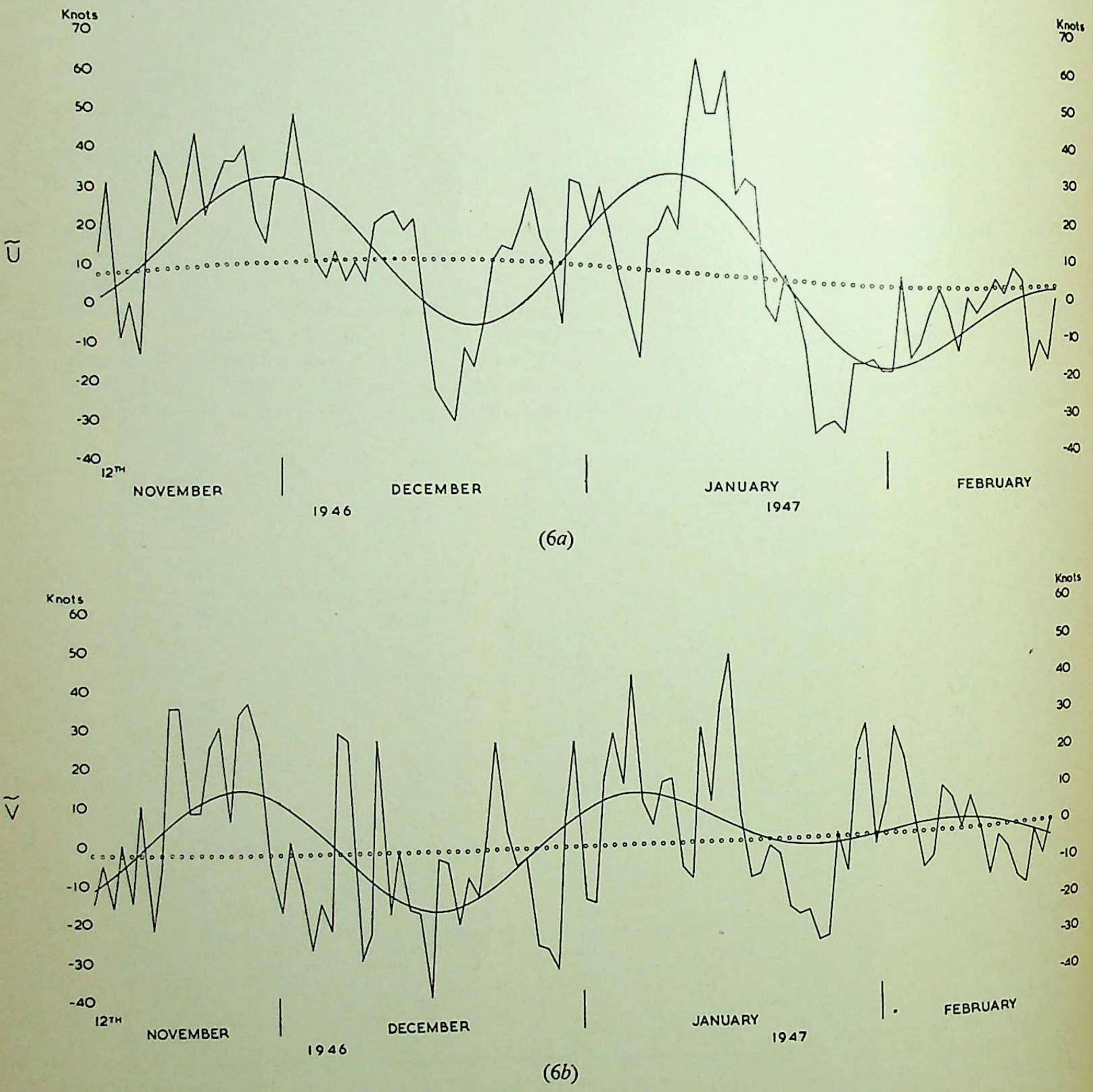


FIG. 5.—Frequency response of running mean shown in Fig. 4.

the limit of the analysis. There is no evidence that motions of even larger period are absent and it seems clear that in this case neither of the accepted continuations of the record is appropriate, so that the validity of the results is limited to the original interval.

Any meteorological statistic will vary in space and particularly in time; it appears that we must confine ourselves to statements about the intensity and flux due to motions of period considerably less than the sampling time T and at a particular time and place.

It follows that we need not, at this stage, go deeply into the question of smoothing and significance of spectral estimates. Our aim is to relate the statistics of motions of one scale with those



(6a)

(6b)

FIG. 6.—Sample of filtered series of U and V.

The light line shows the original series, the heavier line the effect of a filter to attenuate components with period less than 30 days, and the dotted line that of a filter which attenuates components of periods less than 120 days.

of another. For this we shall need a number of records taken in more or less the same conditions. From such a collection of records—I dare not call it an ensemble—we can probably assess the consistency in our estimates in a way impossible with a single record. It is rare for a geophysicist to be confined to the analysis of an isolated record; the difficulty is rather in choosing which of the many to analyse.

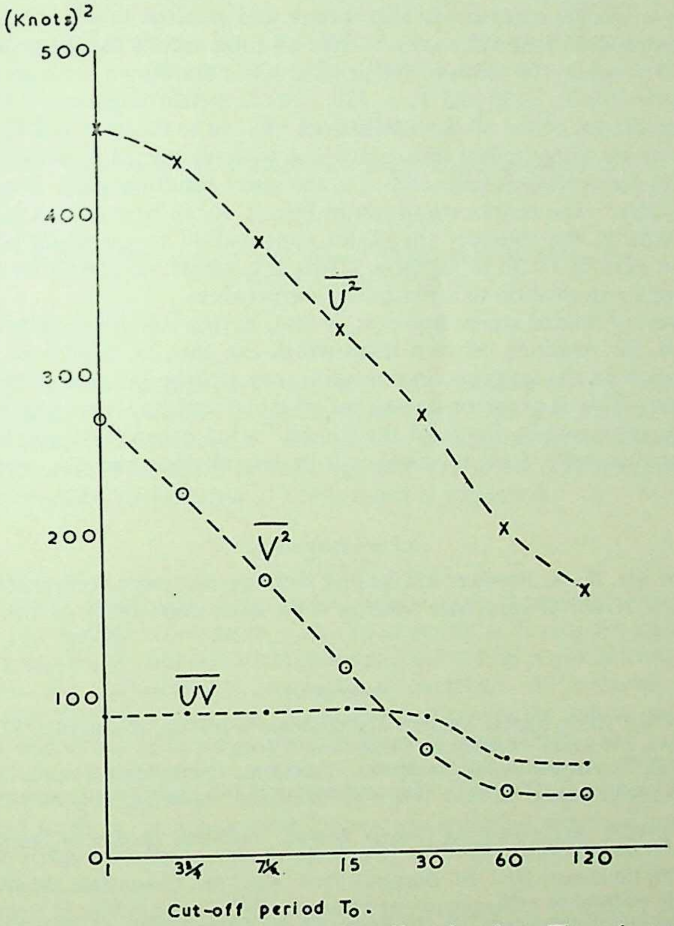


FIG. 7.—Variances and covariances of corresponding filtered series. The values entered at cut-off period = 1 are those of Fig. 2.

(c) *Linear Filters*

The difficulties of interpretation of spectral analysis of meteorological records arise to a large extent from their finite length: there are always longer-period variations which produce a trend. We are investigating the use of linear filters as a possible alternative. The general technique is well known.

The action of any linear filter can be obtained by weighted moving averages. For reasons outlined in section 3(*a*), we wanted to use filters which would separate an observed series into two sub-series, one containing only components of period greater than say T_0 days and the other containing only components of shorter period. These would be equivalent to the meaning process discussed in section 2. Similar filters were recommended by Ross (1949) for the analysis of trends in time-series.

It is easy to see that for a perfect low-pass filter the weights of the running mean ($D(\tau)$ of equation (5)) should be of the form sin x/x. Perfect filtering is impossible with a finite number of terms in the moving average and our filters were limited to 241 terms. Terms outside this range were put equal to zero and the sum of the 241 terms adjusted to unity by a constant multiplier. The form of the low-pass filter with $T_0 = 30$ days is shown in Fig. 4 and its frequency response in Fig. 5 (p. 405). Other filters were arranged to have $T_0 = 3\frac{3}{4}, 7\frac{1}{2}, 15, 60$ and 120 days: the response of the 60 and 120-day filters was, of course, even less perfect than that of 30 days.

These filters were applied in turn to the original series of observations of U and V, zonal and meridional wind components at 700 mb at Larkhill. The computations, based on equation (5), were done on the electronic machine at Manchester and resulted in six sub-series, each of 490 terms and each of restricted frequency range. Not all these results can be given here, but Fig. 6 gives a sample. The middle 100 terms of the original series are shown for both U and V, together with the filtered series for $T_0 = 30$ and $T_0 = 120$. Some meteorologists may be surprised to see the rather large amplitudes of the 30-day oscillations relative to those of 120 days.

The filtered series are to be treated statistically and we have started by calculating the variances and the covariances for corresponding series, i.e. the series resulting when U and V are operated upon by the same filter. The results are shown in Fig. 7; it can be seen that although motions of all periods contribute to the intensity the major contribution to the observed flux during this period comes from periods of 30 to 60 days. This is a rather surprising result and one which it is not easy to explain in relation to atmospheric disturbances.

The main interest of filtered series, however, is that, having isolated motions of different time-scale, one can look for relations between them which can then be confirmed statistically. For instance, the variation in the motions on one scale may well be influenced by the amplitude of motion on another. This is common in studies of wind structure near the ground, where the "gustiness" can be expressed in terms of the "mean" wind speed. At greater levels and with motions of greater time-scale, such relations will be less obvious and they remain virtually unexplored.

Acknowledgments

I am grateful to Mr. R. A. Brooker for dealing with the electronic computation and to Mrs. P. Edwards, Mr. C. L. Gulliver and Mr. Moorey who have done most of the subsequent hand computation.

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SOME APPLICATIONS OF TIME SERIES ANALYSIS TO ATMOSPHERIC TURBULENCE AND OCEANOGRAPHY*

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Summary

THE basic relations for the periodogram, spectrum and correlogram are presented. The main features of atmospheric turbulence in micrometeorology and the general circulation of the atmosphere are discussed with examples. A stochastic process model for ocean waves is reported and examples of time-series analyses in oceanography are given. There are general comments and a selected bibliography of geophysical applications is appended.

1. *Introduction*

The purpose of this contribution is to review briefly some aspects of the application of time-series analysis to geophysical problems as a step to stimulating discussion, on the one hand from geophysicists in explaining to their statistical colleagues the special physical features of the problems they deal with and the analytical and interpretative questions which thereby arise, and on the other hand from statisticians in their task of exegesis concerning the nature, scope and limitations of analytical and inferential methods in the treatment of time series. We are not attempting a comprehensive review: our examples are taken from the fields of meteorology and oceanography only. A bibliography is appended which includes a selection of recent applications (mostly since 1950) of analytical methods in geophysical time-series, with the emphasis on spectral analysis. We regret that shortage of time in preparing this contribution has prevented our surveying the important Russian literature.

Time series occur in all branches of geophysics and the familiar aspects of analysis—estimation and removal of trend, the periodogram, correlation analysis, and spectral analysis—all play their part. The basic formulae defining the periodogram, the spectral function and the correlogram, and their inter-relations are probably too well known to need repetition here, but, because of the special nature of the meeting at which this contribution is presented, it seems advisable to have a general outline. An elementary, over-simplified and therefore incomplete account is given in the next section; for a consistent theoretical account in the general context of stochastic processes the reader is referred to Bartlett (1955). At a recent symposium held by the Research Section of the Royal Statistical Society, Jenkins and Priestley (1957) presented a general review of methods associated with spectral analysis—including tests of significance in the periodogram; the estimation of and confidence intervals for the spectral density, spectral bands and the integrated spectrum; and the estimation of mixed spectra. Papers on the estimation of the spectral density function and periodogram smoothing were also presented by Lomnicki and Zaremba (1957) and Whittle (1957).

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* Fellows will have learned with regret that Dr. S. Rushton died on June 6th, 1957. The galley proofs of the present paper were corrected in September 1957 by Dr. Neumann who wishes to accept responsibility for any errors left in the paper.—*Ed.*

2. *Periodogram and Spectrum*

In the periodogram the "intensity" $I_\omega = A_\omega^2 + B_\omega^2$ is plotted against the angular frequency ω with

$$A_\omega = (2/N) \sum_{t=1}^{N} x_t \cos \omega t,\ B_\omega = (2/N) \sum_{t=1}^{N} x_t \sin \omega t, \quad . \quad . \quad . \quad (1)$$

for a discrete (equally spaced) sequence $x_1, x_2, \quad . \ . \ . \quad , x_N$, for

$$\omega = 0, \frac{2\pi}{N}, \frac{4\pi}{N}, \quad . \ . \ . \quad , \frac{(N-1)\pi}{N} \text{ when } N \text{ is odd,}$$

and

$$\omega = 0, \frac{2\pi}{N}, \frac{4\pi}{N}, \quad . \ . \ . \quad , \frac{(N-2)\pi}{N} \text{ when } N \text{ is even,}$$

and the factor 2 is replaced by $\sqrt{2}$ in the multiplier $(2/N)$ of A_ω (i) for $\omega = \pi$ when N is even and (ii) for $\omega = 0$ in all cases. Here we use "angular frequency" (or "wave number") $\omega = 2\pi/(\text{period})$; the "frequency" $= 1/(\text{period})$. There are just $N/2 + 1$ periodogram ordinates when N is even and $(N + 1)/2$ ordinates when N is odd, spaced at intervals of $2\pi/N$.

Then

$$\frac{1}{2} \sum_0 I_\omega = \frac{1}{N} \sum_{t=1}^{N} x_t^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

and

$$\frac{1}{2} \sum_1 I_\omega = \frac{1}{N} \sum_{t=1}^{N} (x_t - \bar{x})^2,$$

where

$$\bar{x} = \sum_{t=1}^{N} x_t/N.$$

The most convenient form for computation is in terms of mean serial products

$$C_s = \frac{1}{N - |s|} \sum_{t=1}^{N-|s|} x_t x_{t+|s|}, \quad . \quad . \quad . \quad . \quad . \quad (3)$$

when, for $\omega > 0$,

$$I_\omega = \frac{4}{N} \sum_{s=-(N-1)}^{N-1} \left(1 - \frac{|s|}{N}\right) C_s \cos \omega s \quad . \quad . \quad . \quad . \quad . \quad (4)$$

$$= \frac{4}{N^2} \left\{ \sum_{t=1}^{N} x_t^2 + 2 \sum_{s=1}^{N-1} \left(\sum_{t=1}^{N-|s|} x_t x_{t+|s|} \right) \cos \omega s \right\} \quad . \quad . \quad . \quad (5)$$

and

$$\frac{1}{2} I_0 = \bar{x}^2 \text{ (all } N\text{)},$$

and, for N even and $\omega = \pi$,

$$I_\omega = \frac{2}{N^2} \{(x_1 + x_3 + \quad . \ . \ . \quad) - (x_2 + x_4 + \quad . \ . \ . \quad)\}^2.$$

The periodogram ordinates are just the least squares estimates of the amplitudes of periodic terms of the form $\sqrt{I_\omega} \cdot \cos (\omega t + \varepsilon_\omega)$ and for a sequence of independent normal observations a complete decomposition of the total χ^2 on N degrees of freedom (if the population is normal with zero mean and variance σ^2) is achieved into independent component χ^2, either on 1 degree of freedom with expectation $2\sigma^2/N$ (I_0 always and the last I_ω if N is even, because in each case only one parameter is being estimated) or on 2 degrees of freedom with expectation $4\sigma^2/N$ (all other I_ω, because two parameters, the amplitude and phase, are being estimated)—see Fisher (1929). The expectation of $\frac{1}{2} \Sigma I_\omega$ is then σ^2.

To consider the periodogram as a *power spectrum* it is useful to exploit the electrical analogy (although the analogy can also be expressed in physical terms using, e.g., the idea of power in the theory of light or the theory of sound). If a voltage V is applied directly across a unit resistance the instantaneous power dissipated in the resistance is just V^2, so that for a set $x_1, x_2, \ldots, x_N$ of instantaneous inputs over a time N (at unit intervals) the total energy produced is $\sum\limits_1^N x_t^2$ and the *average power* is just $\Sigma x_t^2/N$. Now, the periodogram achieves a representation

$$\Sigma \sqrt{I_\omega} \, . \cos(\omega t + \varepsilon_\omega)$$

of this set of inputs over the "period" N, and, incidentally, therefore presupposes the exact reproduction of this representation over all successive periods of length N; the instantaneous power dissipated in the unit resistance is just $(\Sigma \sqrt{I_\omega} \, . \cos(\omega t + \varepsilon_\omega))^2$ and the *average power*, averaging over a full period N, is just

$$\frac{1}{N}\int_0^N \left\{ \Sigma \sqrt{I_\omega} \cos\left(k \frac{2\pi}{N} t + \varepsilon_\omega\right) \right\}^2 dt = \frac{1}{2} \sum_0 I_\omega,$$

which, of course, must be the same as $\Sigma x_t^2/N$. This is just the relation (2). The average power is thus completely decomposed into (line) elements represented by the ordinates of the periodogram, each ordinate giving the contribution to the average power transmitted by the "signal" with the corresponding frequency. This is just another way of achieving the *analysis of variance* of a discrete sequence into *discrete* components due to a *discrete* Fourier representation.

For a continuous (real) function $x(t)$ defined on a (finite) time interval $(-h, h)$, the total energy is

$$\int_{-h}^{h} x^2(t)\, dt$$

and the *average power* is

$$\frac{1}{2h}\int_{-h}^{h} x^2(t)\, dt. \qquad \ldots \ldots \ldots \quad (6)$$

If we now define a new function

$$y(t) = \begin{cases} x(t) & \text{for } -h \leqslant t \leqslant h, \\ 0 & \text{for } |t| > h, \end{cases} \qquad \ldots \ldots \quad (7)$$

then, if $x(t)$ is in fact defined for all t, we see that $y(t)$ will become equal to $x(t)$ as h tends to infinity, and the average power is the limit of (6), if the limit exists. For a continuous Fourier (i.e. periodogram or spectral) representation of $x(t)$ it is natural to consider the Fourier transform

$$g(\omega) = \frac{1}{\sqrt{(2\pi)}}\int_{-\infty}^{\infty} y(t)\, e^{i\omega t}\, dt$$

$$= \frac{1}{\sqrt{(2\pi)}}\int_{-h}^{h} x(t)\, e^{i\omega t}\, dt, \qquad \ldots \ldots \quad (8)$$

so that

$$y(t) = \frac{1}{\sqrt{(2\pi)}}\int_{-\infty}^{\infty} g(\omega)\, e^{-i\omega t}\, d\omega.$$

Then the *average power*

$$\lim_{h\to\infty}\frac{1}{2h}\int_{-h}^{h}x^2(t)\,dt=\lim_{h\to\infty}\int_{-\infty}^{\infty}\frac{y^2(t)}{2h}\,dt$$

$$=\lim_{h\to\infty}\int_{-\infty}^{\infty}\frac{|g(\omega)|^2}{2h}\,d\omega,\text{ by Parseval's Theorem,}$$

$$=\lim_{h\to\infty}\int_{0}^{\infty}\frac{|g(\omega)|^2}{h}\,d\omega,\quad\ldots\ldots\ldots\quad(9)$$

because $|g(\omega)|^2=g(\omega)\,g(-\omega)$ is an even function. The *power spectral density* (or *spectral function*, or *spectral density*) is the contribution $f(\omega)\,d\omega$ to the average power from a band of frequencies of width $d\omega$, that is

$$f(\omega)=\lim_{h\to\infty}|g(\omega)|^2/h$$

$$=\lim_{h\to\infty}\frac{1}{2\pi h}\left|\int_{-h}^{h}x(t)\,e^{i\omega t}\,dt\right|^2\quad\ldots\ldots\ldots\quad(10)$$

$$=\lim_{h\to\infty}\left\{\frac{1}{2\pi h}\left(\int_{-h}^{h}x(t)\cos\omega t\,dt\right)^2+\frac{1}{2\pi h}\left(\int_{-h}^{h}x(t)\sin\omega t\,dt\right)^2\right\}$$

if the limits of these expressions exist. The frequency ω is allowed to range over all positive values. This is the decomposition of the average power as a continuous spectrum $f(\omega)$, i.e. an analysis of variance into a continuous distribution of contributions from all frequencies, resting formally on Parseval's theorem, as all such Fourier decompositions of mean squares essentially do.

We may note here that the ordinary Fourier representation of $x(t)$ defined on $(-h, h)$ as a countable infinity of discrete periodic components, i.e.

$$x(t)=\sum_{0}^{\infty}c_r\cos(r\omega t+\varepsilon_r),$$

where $\omega=\pi/h$, gives for $r>1$

$$c_r^2=\frac{1}{h^2}\left|\int_{-h}^{h}x(t)\,e^{i\omega t}\,dt\right|^2,\quad\ldots\ldots\quad(11)$$

and for $r=0$ the divisor should be $1/4h^2$; and considering "average power" again (averaging over a full period of length $2h$, because this representation is exactly reproduced over all subsequent periods) we shall have

$$\frac{1}{2h}\int_{-h}^{h}x^2(t)\,dt=\frac{1}{2}\sum_{r=0}^{\infty}c_r^2.\quad\ldots\ldots\quad(12)$$

To compare (10) and (11), the factor 2 should be omitted in (10) since there is a similar factor in (12), and the change from $1/\pi h$ in (10) is accounted for because $f(\omega)$ in (10) is the contribution per unit band-width, whilst in (11) c_r^2 is a contribution over a band-width interval π/h.

Now the relation (4) shows that the periodogram ordinates I_ω are just cosine transforms of the sample serial products C_s, with more weight being attached to the C_s of short lag than to those of longer lags. To obtain the corresponding relation in the continuous case we consider the mean serial product of lag τ

$$C(\tau) = \operatorname*{Lim}_{h\to\infty} \frac{1}{2h} \int_{-h}^{h} x(t)\, x(t+\tau)\, dt, \qquad (13)$$

$$= \operatorname*{Lim}_{h\to\infty} \frac{1}{2h} \int_{-\infty}^{\infty} y(t)\, y(t+\tau)\, dt,$$

$$= \operatorname*{Lim}_{h\to\infty} \frac{1}{2h} \int_{-\infty}^{\infty} g(\omega)\, g^*(\omega)\, e^{-i\omega\tau}\, d\omega, \text{ by the convolution theorem,}$$

$$= \operatorname*{Lim}_{h\to\infty} \frac{1}{2} \int_{-\infty}^{\infty} \frac{|\, g(\omega)\,|^2}{h}\, e^{-i\omega\tau}\, d\omega, \qquad (14)$$

$$= \int_{0}^{\infty} f(\omega) \cos \omega\tau\, d\omega, \qquad (15)$$

because $f(\omega) = \operatorname*{Lim}_{n\to\infty} |\, g(\omega)\,|^2/h$ is an even function.

By simple inversion

$$f(\omega) = \frac{1}{\pi} \int_{-\infty}^{\infty} C(\tau)\, e^{i\omega\tau}\, d\tau. \qquad (16)$$

$$= \frac{2}{\pi} \int_{0}^{\infty} C(\tau) \cos \omega\tau\, d\tau. \qquad (17)$$

There are complications arising from considerations of ergodicity and stationarity, but we will merely note here that if both conditions are satisfied then the function $C(\tau)$ may be identified with the *autocovariance function* of $x(t)$ and is a function of τ only. A further complication which it is useful to mention arises if $x(t)$ actually possesses discrete periodic components in addition to a non-periodic component: in this case, $C(\tau)$ will also contain corresponding periodic components (cf. the well-known property of the correlogram), and, since the Fourier transform of a sinusoidal component is a delta function, discrete components appear in the spectral function at stage (17) the spectrum is quite properly a "mixed spectrum". Normalization with respect to mean and variance allows us to write the autocorrelation function $\rho(\tau)$ for $C(\tau)$ in (17).

The *integrated spectrum* $F(\omega)$ is given by

$$F(\omega) = \int_{0}^{\omega} f(\omega')\, d\omega' = \frac{2}{\pi} \int_{0}^{\infty} C(\tau)\, \frac{\sin \omega\tau}{\tau}\, d\tau \qquad (18)$$

with $F(0) = 0$, $F(\infty) =$ "average power" or *mean square* and the contribution to the average power from a band width (ω_1, ω_2) is $F(\omega_2) - F(\omega_1)$; with normalization and $\rho(\tau)$ for $C(\tau)$ in (18), $F(\infty) = 1$.

Without going into details, we shall merely state about the case not so far considered of a discrete (in time) sequence defined for all time that the spectral function is

$$f(\omega) = \frac{1}{\pi} \sum_{s=-\infty}^{+\infty} C_s \cos \omega s, \qquad (19)$$

with integrated spectrum

$$F(\omega) = C_0\, \frac{\omega}{\pi} + \frac{2}{\pi} \sum_{s=1}^{\infty} C_s\, \frac{\sin s\omega}{s}, \qquad (20)$$

where C_s = "mean serial product" of lag s and C_0 = mean square; and $f(\omega)$ is defined only on the interval $(0, \omega)$. For a complete treatment see Wold (1954). It is well known that if the sequence considered in fact consists of discrete observations (at unit intervals, say) on a continuous process then the spectral functions will coincide only if that of the continuous process is really zero outside the interval $(0, \pi)$: this is just a re-statement of the familiar fact that no information can be extracted about periodic components having a half-period less than the interval between observations; it gives rise to Shannon's sampling theorem in the theory of communication that "if a function contains no frequencies higher than W cycles/second it is completely determined by giving its ordinates at a series of points $1/2W$ seconds apart".

The periodogram has acquired a quite unenviable reputation amongst numerical techniques as having led many good men astray. Its often wildly fluctuating appearance and its ability to indicate "spurious" cyclical periods, e.g. when applied to series generated by known autoregressive schemes, are very familiar. Yet, the "failure" of classical periodogram analysis can be attributed: to its use in isolation from other aspects of analysis, to attempts to *describe* phenomena in terms of a discrete spectrum (i.e. discrete cyclical components of fixed periods) when this was inappropriate and the attaching of physical significance where this was unjustified—and failures in *description* inevitably lead to failures in *prediction*. It is now recognized that the different aspects of time-series analysis form an essential unity. The extensive computational work involved in calculating periodogram ordinates, correlations and spectral estimates has been enormously facilitated by the advent of electrical computers of the analogue and digital type; it may, however, not be out of place to repeat the warning that "forgetting the physics" is just as disastrous in the present as it has been in the past.

The peaks of the periodogram give the least squares estimates of the frequencies of discrete harmonic components and the corresponding ordinates provide the least squares estimates of the amplitudes which have minimum variance among all second order functions of the observations. This justifies the classical approach. When we come to consider the periodogram ordinates calculated in the usual way in relation to a continuous spectral function, it still remains true that the periodogram ordinate I_ω provides an estimate of $f(\omega)$ at a continuous part of the spectrum and this estimate is asymptotically unbiassed, i.e. the bias vanishes with increasing length of the record. Further, if it is assumed that the observations are normally distributed then the equidistant periodogram ordinates are independent and furnish virtually all the information in the sample about the spectral ordinates derivable from second order functions of the observations, although the first property is lost if the assumption of normality is relaxed, at least for finite samples (it is still true asymptotically)—see Whittle (1957). The well-known wild fluctuations of the periodogram ordinates as estimates of the spectral ordinates arise because the standard deviation of the estimate is of the same order of magnitude as the spectral ordinate being estimated and does not diminish with increasing length of record, and also because of the asymptotic independence mentioned above. Hence the development of "smoothing" techniques. In cases where the model fitted consists of both discrete and continuous components, Whittle (1952) has devised an iterative procedure for separating them: the periodogram ordinates, however, still provide estimates of the discrete harmonic components. There is an obvious point about "smoothing", viz., that it may be quite unjustified since the parent spectrum may be itself irregular. A full discussion of smoothing procedures (with references) is given in the symposium papers cited above (see also Daniels (1957) and Whittle (1954*b*)).

3. *Correlogram*

For a discrete sequence the sample autocorrelations

$$r_s = \frac{\sum_{j=1}^{N-|s|} x_j x_{j+|s|} - (N-|s|)\left(\frac{\sum_{j=1}^{N-|s|} x_j}{N-|s|}\right)\left(\frac{\sum_{j=|s|+1}^{N} x_j}{N-|s|}\right)}{\left\{\sum_{j=1}^{N-|s|} x_j^2 - (N-|s|)\left(\frac{\sum_{j=1}^{N-|s|} x_j}{N-|s|}\right)^2\right\}^{\frac{1}{2}}\left\{\sum_{j=|s|+1}^{N} x_j^2 - (N-|s|)\left(\frac{\sum_{j=|s|+1}^{N} x_j}{N-|s|}\right)^2\right\}^{\frac{1}{2}}}$$

where $r_0 = 1$, $r_{-k} = r_k$, are plotted against lag s; or the approximate form $r_s = C_s/C_0$ is used.

4. *Atmospheric Turbulence*: *Micrometeorology*

Before citing some examples of spectral analysis in meteorology, we shall explain the basic background features of atmospheric turbulence.

The covariances of a wind-component and a property such as sensible heat, latent heat or water vapour, and "momentum", play a major role in studies of both extremes of atmospheric motion, viz., in micrometeorology (the meteorology of the air layer nearest to the earth's surface) and in investigations of the general circulation of the atmosphere (the study of air motions on the largest scale). In this section we consider some micrometeorological applications.

Consider a steady horizontal mean wind near the earth's surface, and denote by u and w the respective velocity components in the direction of the mean wind and in the direction of the vertical. These instantaneous values can be expressed

$$u = \bar{u} + u', \quad w = \bar{w} + w',$$

where $\bar{u}$ and $\bar{w}$ are the mean values of the wind components (i.e. time-averages at a point), and u' and w' are the turbulent fluctuations. In the Reynolds' modification of the Navier-Stokes equations of motion (see equation (2) in the paper by Charnock at this symposium), there occur terms of the form $\overline{u'w'}$ (Reynolds stresses, or eddy shearing stresses, or momentum fluxes). Here and in similar expressions below we omit certain multipliers which for all practical purposes are constants when we consider a given point in the atmosphere. The particular term $\overline{u'w'}$, which is obviously a covariance, represents the transfer, or flux, of horizontal momentum along the vertical, i.e. the transfer of fast moving air towards the surface and the simultaneous upward transfer of frictionally retarded air from near the surface. The physical significance of the term is that no sustained motion of the air could be maintained without such a transfer, or flux, since the air would be brought to a standstill by friction without a re-arrangement in the motion.

Further examples of fluxes are the fluxes of heat (sensible heat) and of water vapour (latent heat). The fluxes of heat and water vapour along the vertical have so far received most attention. They are defined by the covariances $\overline{w'T'}$ and $\overline{w'q'}$, where T' and q' are the fluctuations from their respective means of air temperature and absolute humidity. In the surface layer, $\overline{w'q'}$ in the steady state is just the evaporation or condensation (dew) of water vapour. According as evaporation or condensation is taking place, so the sign of the covariance changes.

When fluxes are measured by an instrument, the latter acts as a filter, suppressing usually the high frequency eddies of the relevant variables. Spectral analysis of the fluxes shows which range or set of ranges are important in effecting the flux. If, for example, a flux is primarily effected by high frequency eddies, it becomes necessary to use or develop instruments of appropriate rapid response.

5. *Examples in Micrometeorology*

Example I.—Broadly speaking, small-scale atmospheric turbulence may be divided into two classes with regard to its generation: mechanical (frictional) turbulence and heat convection. Heat convection in any fluid such as the atmosphere is intimately connected with (amongst other things) the variation of temperature with height, but, since the latter depends on radiation conditions, so also does heat convection, at least in a general manner.

By measuring and evaluating the spectrum of vertical velocity in the lower atmosphere, Van der Hoven and Panofsky (1954) were able to demonstrate that the low frequency part of the spectrum depends strongly on the intensity of solar radiation. They found that with increasing solar radiation the maximum of the spectral curve of vertical velocity moves to the lower frequencies and that the "energy" in the low-frequency part of the spectrum increases, whereas the high-frequency part of the spectrum remains practically unchanged (see their Fig. 36), and they therefore suggest that this high-frequency region represents mechanical turbulence, whilst the radiation-dependent low-frequency region represents turbulence connected with heat convection. Their findings also suggest that as convection intensifies, the convection cells become taller relative to their width, an inference which is supported by the work of Gifford (1955), who tracked the motion of no-lift balloons. By studying the spectrum of the product of horizontal and vertical wind speeds, Van der Hoven and Panofsky found that with increasing intensity of solar radiation, more and more of the vertical transfer of momentum is supplied by the low-frequency eddies.

Example II.—Deacon (1955) measured the vertical flux of momentum $\overline{u'w'}$ over a level grassland near Melbourne at a height of 7 m. in conditions of light wind and a moderately strong lapse of temperature with height. To get a reliable estimate of the mean flux, whose standard error would not differ by more than a given fraction from the mean for a very large sample, he considered the maximum permissible time interval between successive observations. The autocorrelation coefficients were first computed from a record of 300 readings at 1 second intervals and the observed correlogram approximated by $\rho(t) = e^{-\mu t}$, with $\mu = 0{\cdot}38$ (see Deacon (1955), Fig. 10).

For a sample of size n from an exponentially autocorrelated time-series, with observations taken at equal intervals of time over a total period of duration D, the variance of the sample mean is easily determined, hence the "effective" number of observations and so the required interval of observation over the duration D to achieve the necessary precision. The spectral function $f(\omega) = 4\mu/(\mu^2 + \omega^2)$ associated with an exponential autocorrelation function is closely approximated by the empirical spectrum, with some fluctuation in the high frequency range.

6. *General Circulation of the Atmosphere*

Meteorologists are particularly interested in the fluxes vu, vT, and vq, where u and v are respectively the westerly and poleward components of the wind, T is air temperature and q is absolute humidity. The flux vu denotes the meridional flux of "westerly momentum"; vT and vq respectively denote the meridional fluxes of heat and water vapour. These fluxes are of interest for the following reasons:

(i) The surface winds of the middle latitudes (about 35° to 60°) are westerlies; those of the tropical regions are easterlies. The surface frictional retardation of the low-latitude easterlies has the effect of injecting westerly momentum in these winds; the frictional retardation of the mid-latitude westerlies involves a loss of westerly momentum. Unless westerly momentum were continually being transferred poleward from the low-latitude easterlies, the general circulation of the atmosphere could not be maintained. This meridional transfer or flux of momentum is vu.

(ii) It is calculated that if in any latitude belt the sole source of heat were radiation, then the high latitudes of both hemispheres would be much colder and the tropical belts would be much hotter than they are at present. In fact, a transfer of heat (sensible heat) takes place from the low latitudes, where there is a net gain of radiation, to the high latitudes, where there is a net loss of radiation. This meridional flux of heat is vT.

(iii) In some latitudes precipitation exceeds evaporation whilst in other latitude belts the opposite holds, and a transfer of water vapour takes place from regions of net gain of water vapour to regions of net loss. This meridional flux is vq.

(Throughout the above certain multipliers of the fluxes have been omitted.)

If we consider the average flux for a given time period at some pressure level at a point over the Earth's surface, then, if Reynolds axioms are satisfied,

$$\left.\begin{aligned}\overline{vu} &= \bar{v}\bar{u} + \overline{v'u'},\\ \overline{vT} &= \bar{v}\bar{T} + \overline{v'T'},\\ \overline{vq} &= \bar{v}\bar{q} + \overline{v'q'},\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \qquad (21)$$

where the first term on the right in each equation represents an advective flux by a mean meridional circulation. The second term is the "eddy flux".

If instead of the flux at one station, we consider the average flux for a number of different stations (e.g. stations along a latitude circle), then the eddy fluxes $\overline{v'u'}$, $\overline{v'T'}$ and $\overline{v'q'}$ will in fact each be the sums of two terms. Denoting a space-average by { . . . }, and letting u'_t and v'_t be deviations from a time-average, and similarly u_x' and v_x', deviations from a space-average, then

$$\{\overline{v'u'}\} = \{\overline{v_t'u_t'}\} + \{\bar{v}_x'\bar{u}_x'\} \quad . \quad . \quad . \quad . \quad . \quad . \qquad (22)$$

and similar equations hold for the heat and water-vapour fluxes. The first term on the right-hand side is simply the space average of the term previously denoted $\overline{v'u'}$. The second term on the right arises from a possible correlation in space between the time means of v and u, and is called the "standing" eddy flux. It is due (when it exists) to semi-permanent features of the atmosphere such as large eddies set up by major mountain ranges.

Several attempts have been made to compute the magnitude of such terms as are involved in (21) and it is generally agreed that in the case of momentum and sensible heat the eddy fluxes considerably outweigh in importance the terms involving the mean for meridional circulation.

The terms $\overline{v'u'}$, $\overline{v'T'}$ and $\overline{v'q'}$ are just covariances and a spectral analysis of the terms vu, vT and vq helps to throw light on the frequencies, or periods, of the eddies responsible for the major part of the flux.

The correlations involved in studies of the fluxes are Eulerian time-correlations and the information derived from the corresponding spectral analysis is in terms of frequencies or periods. Eulerian space correlations which would supply information on the *wave numbers*, instead of frequencies or periods, of the most important eddies would be much more valuable as they would yield information on the *spatial structure* of atmospheric disturbances. Taylor (1938) showed that if the magnitude of the turbulent fluctuations of the wind are small compared with the mean wind, then the form of the time correlation function for a point in the turbulent fluid will be virtually equal to the spatial correlation function, i.e. the variables times and distance are interchangeable (see also Ogura (1953)). MacCready (1953*b*) presented observational data indicating that the equivalence of the Eulerian time and space correlations is, at least on occasions, approximately valid even when the turbulent fluctuations of the wind are not small compared with the mean wind.

In 1941, Kolmogoroff presented a model of turbulence, called the *similarity hypothesis* (for an account of the model and of subsequent work by other authors, see Batchelor (1953); for a summary see Priestley and Sheppard (1952)). This model is capable of predicting on the basis of physical-dimensional considerations statistical characteristics such as the form of the correlation function for a certain range of eddy "sizes" (wave numbers) called the inertial or equilibrium sub-range. This range will be well developed when the Reynolds number of the motion of the fluid is large, such as is usually the case in the atmosphere. Some of the predictions of the model have been verified in practice. The unexpected thing is that the statistical laws predicted for the isotropic range have in several cases been found to hold, at least approximately, for wave numbers where the turbulence is certainly not isotropic. Examples of this are given by McCready (1953*b*); further examples relating to large-scale flow are given by Hutchings (1955).

7. *General Circulation of the Atmosphere*: *Example*

Estoque (1955) considers the turbulent flux of momentum and heat at the 850 mb. pressure level over the south-eastern United States for the year 1949. Annual means and harmonic components corresponding to the "annual eddy" were initially subtracted from u, v and T. Covariances of the form

$$\tfrac{1}{2}\,\overline{(v'(t)\,u'(t+\tau)+v'(t)\,u'(t-\tau))}$$

and

$$\tfrac{1}{2}\,\overline{(v'(t)\,T'(t+\tau)+v'(t)\,T'(t-\tau))}$$

were computed and analysed by spectral analysis (see Estoque (1955), Fig. 4).

It has been pointed out above that computations of meridional fluxes of momentum, heat and water vapour indicate that the most important mechanism for the transfer of momentum and heat are the eddy fluxes ($\overline{v'u'}$ and $\overline{v'T'}$), i.e. the turbulent "elements" of the general circulation—presumably related with the migratory cyclones and anticyclones. Since the passage of cyclones and anticyclones over any station takes but a few days, it is to be expected that the peak of the flux spectrum will be at a period of a few days. This anticipation is fully confirmed by Estoque's results.

8. *Stochastic Process Model for Ocean Waves*

A theoretical study of a model for surface waves in the open ocean as a stationary normal stochastic process with a two-dimensional spectrum has recently been made by Longuet-Higgins

(1957) and he has suggested that the model may be applicable to other geophysical phenomena, e.g. to microseisms or perturbations of the ionosphere. To take account of the fact that waves in the open sea are highly irregular and lack any apparent single direction or orientation, a two-dimensional Fourier synthesis in terms of a continuous spectrum involving wave-numbers in two horizontal directions is considered appropriate. The surface elevation z is represented in terms of the horizontal coordinates, x and y, and time t as

$$z(x, y, t) = \Sigma \cos (u_n x + v_n y + \sigma_n t + e_n),$$

where it is supposed that the wave numbers (u_n, v_n) are densely distributed throughout the u, v plane, i.e. there are an infinite number in any elementary area $du \,.\, dv$. The phases e_n are taken to be uniformly distributed in the interval $(0, 2\pi)$; σ_n is regarded as a function of u_n and v_n, i.e.

$$\sigma_n = \sigma(u_n, v_n)$$

and the amplitudes c_n are random variables such that in any element $du\ dv$

$$\sum_n \tfrac{1}{2} c_n^2 = E(u, v)\, du \,.\, dv$$

= the contribution to the mean energy from an element $du \,.\, dv$.

The function $E(u, v)$ is the spectrum of z, and the mean-square value of z per unit area of the sea surface per unit time is given by

$$\lim_{X,Y,Z,\to\infty} \frac{1}{8XYT} \int_{-X}^{X} \int_{-Y}^{Y} \int_{-T}^{T} z^2\, dx\, dy\, dz = \sum_n \tfrac{1}{2} c_n^2 = \int_{-\infty}^{\infty} \int_{-\infty}^{\infty} E(u, v)\, du\, dv.$$

The correlation function $\psi(x, y, t)$ is just

$$\begin{aligned} \psi(x, y, t) &= \lim_{X,Y,T\to\infty} \frac{1}{8XYT} \int_{-X}^{X} \int_{-Y}^{Y} \int_{-T}^{T} z(x', y', t')\, z(x' + x, y' + y, t + t')\, dx'\, dy'\, dt' \\ &= \Sigma \tfrac{1}{2} c_n^2 \cos (u_n x + v_n y + \sigma_n t) \\ &= \int_{-\infty}^{\infty} \int_{-\infty}^{\infty} E(u, v) \cos (ux + vy + \sigma t)\, du\, dv, \end{aligned}$$

so that ψ is the cosine transform of $E(u, v)$. This is just the three-dimensional analogue of the usual (one-dimensional) Fourier series representation of a normal stochastic process, used especially in communication theory. Such a model for ocean waves rests on the principle of superposition of disturbances due to different generating mechanisms, i.e. the mechanical system is assumed to be linear. This appears to be largely valid for low waves in deep water, but it is recognized that it would not apply to waves approaching the maximum height, or to surf, or to the rolling motion of a ship when this is large enough for non-linear terms to become important. The effect of non-linearity in the mechanism of waves is to make crests higher and troughs shallower than for a surface described by the above model.

If m_{rs} is the $(r, s)^{\text{th}}$ moment of the spectral distribution $E(u, v)$, i.e.

$$m_{rs} = \int_{-\infty}^{\infty} \int_{-\infty}^{\infty} E(u, v)\, u^r v^s\, du\, dv,$$

then the conditions for the sea surface to degenerate in the following ways can be expressed solely in terms of the moments m_{rs}: (i) the surface to be "long-crested", i.e. for the energy to travel always in the same direction—in this case E degenerates into a single one-dimensional spectrum;

(ii) the surface to consist of not more than n long-crested systems—so E degenerates into n one-dimensional systems; (iii) the energy to correspond to wave components of the same length but possibly different directions, i.e. E to degenerate into a "ring" spectrum; (iv) the surface to have a "standing-wave" pattern with the energy situated at two diametrically opposite points of the spectrum; (v) energy uniform in both wavelength and direction, i.e. the spectrum is *narrow* with the energy concentrated at a single point; (vi) energy concentrated in not more than two narrow bands, i.e. the spectrum consisting solely of two opposite corners of a rectangle $u =$ constant, $v =$ constant.

The spectrum E^θ of a curve of intersection of the wave surface by a vertical plane in an arbitrary direction can be found and hence the *principal direction*, i.e. the direction in which the second moment of E^θ (and therefore the root-mean-square wave number) is a maximum, with the minimum r.m.s. wave number in the direction perpendicular to this principal direction. The ratio $(1/\gamma)$ of the r.m.s. wave numbers in these two directions is taken by Longuet-Higgins as a convenient measure of the long-crestedness of the surface, since for a long-crested system of waves the r.m.s. wave number is a maximum perpendicular to the crests and a minimum parallel to the crests. For a perfect long-crested system, i.e. a single one-dimensional spectrum, $\gamma = 0$ and for very short-crested waves $\gamma = 1$. The properties of the "carrier wave" and "wave envelope" are also derived.

The distribution of the elevation z is normal and that of the components of slope, $\partial z/\partial x$ and $\partial z/\partial y$ is bivariate normal with the greatest r.m.s. slope in the principal direction of the surface.

The distribution of the magnitude of the slope regardless of its direction can be reduced to a Rayleigh distribution (χ^2 on two degrees of freedom) for very short-crested waves ($\gamma = 1$) and it tends to a normal distribution for very long-crested waves (with a certain anomalous behaviour for very small slopes). A number of statistical properties, such as the average number of zero-crossings of z along a line in an arbitrary direction, are determined and an approximation sequence is derived for determining the spectrum given its statistical properties in terms of moments of the spectral distribution in any direction θ, it being supposed that the height and slope of the surface can be measured along such a line, e.g. by an aircraft flying on a fixed course over the sea surface at high speed and constant altitude.

The distribution of wave height (the difference in level between a crest and the preceding trough) had also been previously discussed by Longuet-Higgins (1952) for a narrow spectrum as a Rayleigh distribution and it had been shown that the values of the r.m.s. wave-height, the mean height of the highest one-third waves (defined by Sverdup and Munk (1947) as "significant" waves), and the most probable height of the largest wave in a given time-interval were in close agreement with observation. General agreement with the Rayleigh distribution in this case had already been found by Barber (1950).

It was suggested that such a situation for wind-generated waves with a narrow spectrum could arise from a single storm area at a great distance (compared with the dimensions of the storm); so that in the course of propagation, different frequencies in the spectrum, being propagated with different velocities, would become spread out in space, and over a short interval of time only a narrow range would be present. The analysis would then only be appropriate as long as the recording time was neither too long to allow a significant change in the frequency and amplitude of the waves nor too short to allow a representative sample of wave heights to be recorded. Further, it was suggested that the model would be more appropriate for a pressure wave record on the bottom than to actual surface elevation, since in the former case the high frequencies are damped out relative to the lower frequencies and the corresponding frequency spectrum is narrower; or, alternatively, to the angular deflection of a ship's mast to the vertical, since a ship acts as a resonant filter, amplifying those components of the spectrum close to its natural period.

Incidental results related to the above have also been given by Cartwright and Longuet-Higgins (1956). They derived the distribution of the height z between a wave-crest and the mean level by applying directly the results of Rice (1944, 1945) in communication theory on the distribution of the maximum of a stationary normal (one-dimensional) stochastic process represented by its Fourier synthesis. The distribution of z is found to depend only on the r.m.s. height and on a parameter $\varepsilon^2 = (m_0m_4 - m_2^2)/m_0m_4$, where the m_r are the moments of the spectral distribution. The parameter ε is obviously a measure of the relative width of the spectrum; when ε is small (an infinitely narrow spectrum) the distribution of z tends as expected to a Rayleigh distribution and when ε tends to its maximum value of unity (which can occur when one wave of high frequency and small amplitude is superposed on another disturbance of lower frequency) the distribution tends to a normal distribution. Statistical properties are derived: moments, mean values of the highest $1/n^{\text{th}}$ of the crest heights and the expectation of the highest in a sample of N crest heights. The distribution of crest heights as measured from records of ocean wave phenomena was found to agree with the theoretical distribution. The records examined comprised one of pressure on the sea-bed (off Cornwall), two records of wave-heights in deep water (Bay of Biscay), and two records of the pitching and rolling motion of *R.R.S. Discovery II* in a seaway in the North Atlantic. On the other hand, *crest-to-trough* heights were found to depart significantly, as expected, from the Rayleigh distribution for reasonably broad spectra.

The three-dimensional stationary normal stochastic process model of ocean waves with a two-dimensional spectrum has also been treated by Pierson (1955) and applied to the study of the motion of a ship by St. Denis and Pierson (1953). Regarding the appropriateness of the model, Pierson (1955) concludes that, depending on the meteorological conditions, areas of many hundreds of square miles for many hours, and wave records as a function of time lasting from twenty minutes to many hours, are so closely represented by treating them as samples from particular realizations of a stationary normal process that the departures are unimportant. In fact, of course, the spectra involved are slowly varying functions of time and space, so that the assumptions of stationarity hold only if the translations in time and space that are considered are not too large.

9. *Oceanographic Examples*

Example I: That the correlogram is an effective method of detecting a periodic "signal" in random "noise" is well illustrated by analyses by Seiwell and Wadsworth (1949) and by Seiwell (1949) of records of pressure pulses at the sea bottom. A typical (unsmoothed) periodogram from these records showed a characteristic fluctuating appearance with many well-defined peaks, whilst the corresponding correlogram showed a remarkably smooth curve maintaining a constant period and damping slowly to a terminal amplitude, which was thereafter maintained by smooth cycles. The correlogram of residuals after the single cyclical component had been estimated by least squares and subtracted from the record damped in a fashion typical of an autoregressive sequence. The analysis showed, therefore, a spectrum consisting of a single discrete component and a continuous component representing a wide band of frequencies with an average frequency equal to that of the single cyclical componnent but damping out rather rapidly. The physical characteristics of the sea surface wave pattern corresponding to such a record, it was suggested, comprised a fundamental wave (sea swell—the "signal"), on which was superimposed a series of local impulses (local sea—"noise") of the same average frequency, probably generated by local winds, which died out rather rapidly. One record analysed by Seiwell showed that in the complete absence of sea swell, with local influences dominating the pattern, the correlogram damped almost to zero, indicating almost completely autoregressive data. The author's interest in correlogram analysis led him (Seiwell (1950)) to carry out a number of experimental analyses which parallel the work of Professor M. G. Kendall (see Kendall (1946*a*, 1946*b*).)

Further analyses of ocean waves have also been given by Rudnick (1951). The actual computation of correlograms was carried out on an electronic analogue computer developed for the purpose. Signals were furnished to the correlator through a simple resistance-capacity high pass filter whose limit corresponded to a period equal to the longest correlation interval to be used, so that the correlograms were designed to be comparatively free of the effect of slow oscillation, or trend in the data. The correlograms were roughly described by the author as a curve oscillating under a diminishing envelope, with the two first-order parameters of the correlogram being the period for the oscillation (the dominant period of the wave spectrum) and the decay time of the envelope, i.e. $\rho(\tau) = e^{-|\tau|/aT} \cos 2\pi\tau/T$, with a corresponding spectrum of the form

$$\frac{1}{1 + \dfrac{(f - f_0)^2}{(bf_0)^2}},$$

where $f_0 = 1/T$. Broad agreement with this spectrum was obtained by determining one wave spectrum directly with a wave analyser. Repetition of the records at six-hourly intervals showed that only the first three periods of each correlogram were at all informative, the rest of the correlogram mainly constituting sampling errors, which the author discussed in the light of the results of Bartlett (1946) for the sampling errors of autocorrelations. The fact that the broad smoothed form of the spectrum is given by the initial part of the correlogram (i.e. for small lags) is the basis of the smoothing procedure of Bartlett (1950).

Another conclusion from Rudnick's work was that the overall distribution of displacements was normal, so that the record of waves as they pass a fixed point could be regarded as a realization of a normal stochastic process.

Example II: The analysis of a seiche record of 660 observations at 15-second intervals of water level in a rock channel on the coast of New Zealand has been presented by Whittle (1954*a*). The analysis required the development of a method for estimating the components of the mixed spectrum involved (for discussion of this, see Jenkins and Priestley (1957)). The observations were taken at the turn of the tide in order to minimize its effect. In spite of this, the data showed a definite trend which was eliminated by fitting a second-order polynomial.

The correlogram of actual deviations from trend showed oscillations damping away until about lag 30, after which they maintained a fairly constant amplitude; the tail showed a period of about 11 units (unit = 15 sec.) with an oscillation that was not simple. The first estimate of the spectrum was made using the smoothing method of Bartlett (1950) with autocovariances C_s up to lag 114. Sections of the periodogram were calculated in the usual way in the immediate neighbourhood of the maxima of the smoothed spectrum. When the smoothed spectrum was replotted as a function of period instead of the angular frequency ω, peaks were at $T = 11$ (narrow, well defined), 14 (strong, broad), 25 and 36 (subsidiary), and about 48 (faint suggestion). From the relations $25 = 11 + 14$, $36 = 2\ (11) + 14$, $48 = 3\ (11) + 14$ (nearly)—notice that these relations are for periods, not frequencies—Whittle suggested that a non-linear mechanism might have been present. A fourth order autoregressive scheme was then fitted to the data, the presence of wave-trains with periods $T = 10{\cdot}87$ and 123 established by a significance test, with amplitudes estimated from the periodogram; modified autocovariances computed and finally a further (fifth-order) autoregressive scheme fitted. Since no further periods were found to be significant, the spectral estimates consisted of two lines corresponding to the sustained waves of periods $T = 10{\cdot}87$ and $T = 123$ and a continuous spectrum corresponding to this autoregressive residual component. The possible subsidiary oscillations of periods $T = 25$ and $T = 36$ were not sufficiently pronounced to be explained by the final autoregressive scheme, which could in any case only reproduce the broader spectral peaks. The sustained sets of waves were identified as being due to reflection at the open end of the channel. The non-linear phenomenon giving unforced oscillations subject to damping was identified with intermittent washing in of water at the top (shore) end of the channel.

Example III: Aerial photographs of the glitter of the sun on the sea surface were used by Cox and Munk (1954*b*) to derive the statistics of the slope distribution. It was found that the joint distribution of the up-wind and cross-wind components of slope was very nearly normal, with a slight negative skewness in the up-wind direction which increased with increasing wind speed. This negative skewness implied a modal slope of a few degrees rather than zero, the azimuth of ascent pointing downwind, so that small slopes were slightly more frequent than would have arisen from a purely normal distribution. A slight positive kurtosis was also found, which did not vary with wind speed. It was suggested that the superposition of non-linear effects accounted for the slight departure from a strictly normal slope distribution.

A number of other aspects was studied, including the effect of oil "slicks": it was found that such slicks reduced the mean square slope by a factor of two or three, eliminated skewness but left kurtosis unchanged.

The justification for studying slope and curvature of the sea surface rests partly on the simple fact that, if the elevation spectrum is $f(\omega)$, then the slope and curvature spectra are respectively $\omega^2 f(\omega)$ and $\omega^4 f(\omega)$, so that the latter are weighted progressively in favour of higher frequencies. The areas under the three spectral curves are respectively mean squares of elevation, slope and curvature. It follows that studies of slope and curvature provide direct knowledge of the higher frequency waves which play an important part in considerations of the momentum transfer from wind to water (i.e. wind stress) and the generation of the more important parts of the wave spectrum (i.e. larger waves). The action of a slick is to remove only high frequency components: in the experiments mentioned above, the mean square elevation was unchanged, the mean square slope was substantially reduced and the mean square curvature almost eliminated.

10. *Some General Remarks*

(i) It will be clear from the examples quoted in the foregoing sections that many of the time series occurring in geophysical work, particularly in meteorology, are multivariate (or multiple) series. Considerations of the "co-spectrum" (the in-phase relationship) and the "quadrature spectrum" (the out-of-phase relationship) occur in treating, for example, the turbulent flux of momentum and heat in the general circulation of the atmosphere, although we have omitted these complications (see Estoque (1955)). From the statistical point of view, some sampling results have been obtained (see Bartlett (1955) and references there), but the formulae involved become complicated; work, particularly in the field of econometrics, has been published on the simultaneous estimation of a number of autogreressive models, where the complication of "identification" arises; methods for testing the goodness of fit of the correlogram can also be extended to multivariate series; and a general treatment of such series has been given by Whittle (1953).

(ii) The over-riding feature of geophysical time-series is their essential non-stationarity. The examples quoted in previous sections show that stationarity or quasi-stationarity is only a reasonable assumption over relatively short time-scales, and obvious observational devices have to be adopted to take account of this physical fact.

(iii) The purely statistical problems involved in the analysis of geophysical time series are not different in kind from those arising in other fields.

(iv) It has been said of oceanography that the purpose of the science is "to understand the present, to interpret the past and to predict the future". This applies equally well to other branches of geophysics. On prediction, or forecasting, it appears that purely statistical techniques without regard to physical "determinants", whilst of incidental interest, are likely to have only limited success.

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DISCUSSION ON THE PAPERS

Mr. R. P. WALDO LEWIS: One can often approximately assess the significance of fluctuations in a time series, when mathematical analysis is difficult or impossible, by constructing a set of comparable series, using random numbers. A practical example is considered where an apparently plausible result is shown not to differ in any obvious way from results produced by random processes.

Dr. G. M. JENKINS: I should like to thank the authors of these three papers for a comprehensive account of the numerous applications of spectral analysis now being made in geophysics.

The use of moving average formulae for eliminating trend is not new to statisticians—what is perhaps new is the interpretation in terms of frequency and the use of an almost orthogonal set of moving averages to separate out the contributions into various band widths.

One of the many uses to which spectral analysis is put in the physical sciences is to provide a check on analogue equipment, e.g., a spectrum analyser or harmonic analyser. In this sort of instrument, the power in a frequency range is determined by precisely this method of filtering. Up to the present, results from a harmonic analyser have been compared with a power spectrum analysis whereas it would seem more logical to use a numerical filter with the same frequency response as the instrument itself.

As it has been already pointed out, the advantages of the filtering procedure are:

(i) It gives information about the distribution of power in a band width as opposed to the spectrum approach which gives the average power in a band; in addition it preserves information about phase relationships which the spectrum does not.

(ii) What is more important, the relation between the power at different frequencies may be investigated. In linear oscillations, these are uncorrelated but in non-linear disturbances, there is correlation between the power at the various frequencies which is of considerable interest in a practical problem.

Whilst I think that numerical filtering is a useful addition to the armoury of the time-series analyst, there are a number of points which require theoretical investigation first. In this connection, it is necessary to add a word of warning about interpretation. It is well known that if there is a random component in the series, moving average smoothing results in a residual with spurious periodicities. Since several smoothing operations have been performed before we are left with the contribution due to the higher frequencies, it may be that the effect on this end of the spectrum is quite serious.

Another disconcerting feature of the filters as they stand is that the total power of the output is approximately 0·9 that of the input—this anomaly also creeps into the degrees of freedom where the sums of the degrees of freedom of the filtered outputs is not equal to the total degrees of freedom, although this is not essential since the outputs are correlated. Although the point raised by Mr. Craddock about the testing of a number of variances is well known to statisticians, it is worth pointing out that a test due to Bartlett is available for testing the homogeneity of several variances.

Another question which requires further work is the optimum choice of filter; also how efficient is the method as an estimate of the average power when compared with the periodogram. Another question not raised at this meeting is what would happen if the band width of the filters were narrowed. In the case of harmonic analysers, one obtains surprisingly different answers by narrowing the band width.

An attractive feature of the method of filtering is that it gives a method of dealing with non-stationary time-series. It does not enable us to distinguish stationary from non-stationary components but it seems that the output from each of these filters is itself a stationary series. It might well be that the outputs from all but the low frequency filters could be considered together as a stationary series and a spectrum analysis applied. In fact, this is the sort of answer that the physicist or engineer requires—the method would separate out the low frequency trend giving him an estimate of the power at the lower frequencies and leave him with a spectrum for the noise component in which he is primarily interested.

I was at loss to make out where the functions *f* and *g* in the Reynolds' axioms mentioned in Mr. Charnock's paper appeared from. The contention in this paper that there is no theoretical advantage in choosing spectrum analysis in preference to harmonic (or periodogram) analysis is not correct. Generally speaking, one would not use spectrum analysis unless the correlogram damped out; neither would harmonic analysis be used unless the correlogram exhibited sustained oscillations.

Lastly, I do not agree with the point of view that geophysical time-series present peculiar features of their own—there are copious examples of similar non-stationary series in all the physical sciences.

Professor BARTLETT: There was one minor point I was not clear about in the structural analysis by the filter method in that it was suggested by Dr. Jenkins that this method would enable trends to be eliminated. Periodograms would perhaps also do that because they would record trends as waves of low frequencies.

I think there is a further word of warning about the degrees of freedom, which I do not think Dr. Jenkins mentioned. In evaluating methods of testing significance in relation to such procedures, you might well work out at least an approximate method for handling the various components for a purely random series, the only difficulty there being the approximation involved with the filters not being perfect. But the whole idea is in analysing a series which is not random, and in discussing the significance of the different components when a series as a whole has not got a uniform spectrum.

It must be remembered that the definition of what is meant by degrees of freedom for the different components is then much more complicated. There is an example in the original series. The original series is one which, in one sense, has *N* degrees of freedom for the *N* observations, but, as has been mentioned, this series is made up of a complicated set of components of different frequencies; it follows that the actual sampling distribution of the total variance would be extremely complicated and would not bear any simple relation to these *N* degrees of freedom. It is possible that an approximate method might still be worked out for the filtering method, for as filtering proceeded the frequency band would get smaller and smaller. One would ultimately isolate out a body of components, for which you would have for each amplitude two degrees of freedom, approximately independent of degrees of freedom for the other components. However, the method by then would probably be equivalent to the periodogram approach.

One field of application related to the last paper is the work going on at Cambridge and elsewhere by Dr. Ratcliffe and his co-workers on problems of reflection from the ionosphere. This work is related to questions of large atmosphere movements and the character of the reflection changes occurring when random movement goes on.

One small point: it seemed to be suggested in Mr. Craddock's paper that the first spectral analysis was due to G. I. Taylor. Taylor was certainly a pioneer in starting a statistical theory of turbulence and introducing spectral analysis in that field but hardly the first to use spectral analysis in any field.

Dr. S. K. ZAREMBA: I should like to make some general comments which, however, are more particularly connected with Mr. Craddock's paper. The concept of power spectrum applies, by definition, only to stationary processes. Testing stationarity is a very complicated matter, but before we apply statistical spectral analysis to a process, we should be able, at least, to feel that this assumption is plausible. Temperatures are certainly not stationary throughout the year. There is obviously a seasonal trend which should be removed in the first place. In this context it might be worth mentioning that, in a paper written by Mr. Lomnicki and myself, due to appear in the next issue of the *Annals of Mathematical Statistics*, the elimination of a polynomial trend is treated together with its influence on the estimation of the covariance and autocorrelation functions. In the case considered by Mr. Craddock, one would think of a trigonometric trend, but the problem is quite similar, and the same methods apply with the substitution of trigonometric functions for the rather less familiar Chebyshev polynomials. However, it ought to be remembered that trend elimination is necessarily based on various specific hypotheses concerning the structures of the trend and of the residuals; in particular, the latter must be assumed to be stationary. It would be helpful if Mr. Craddock told us whether he did anything to remove the trend from the temperature records, and, if so, how exactly this was done. It would also be interesting to know if there is anything to substantiate the belief that the residuals are stationary.

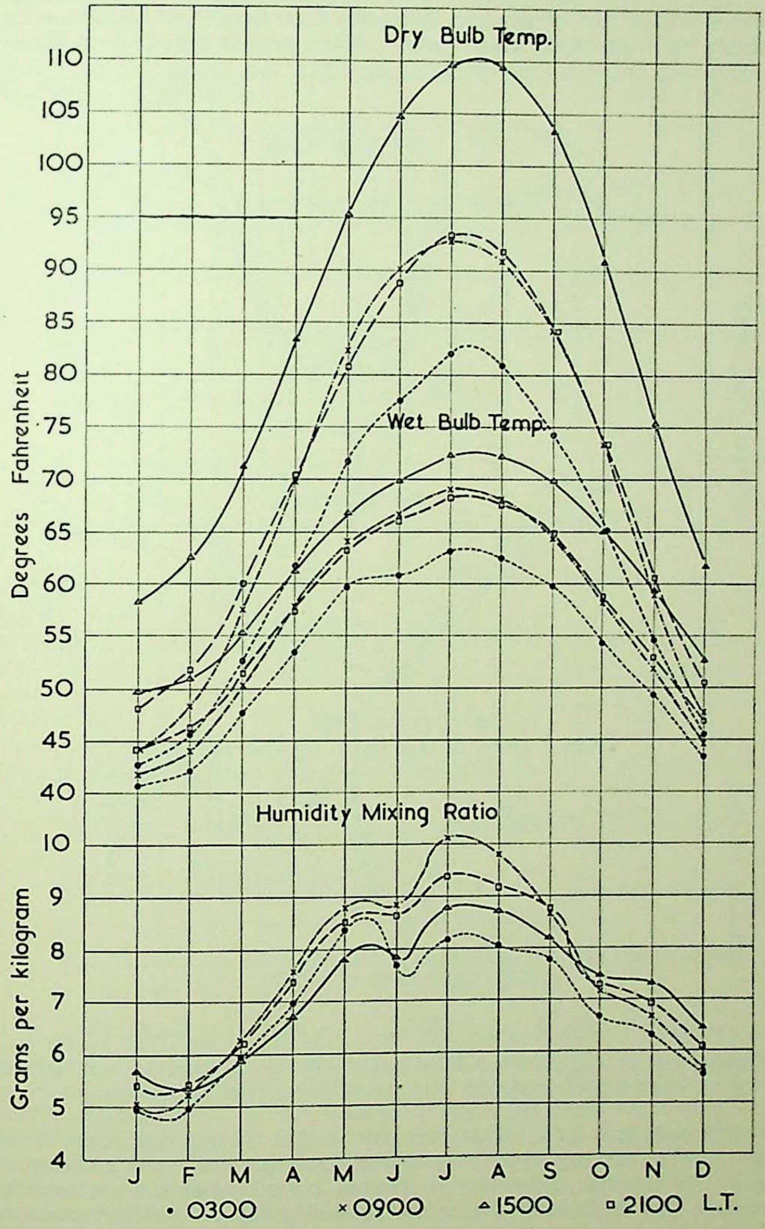

FIG. 1.—Monthly variation of long period means of dry bulb temperature, wet bulb temperature, and humidity mixing ratio at Habbaniya.

There is no doubt that the method of filters can be useful when it is embodied in suitable analogue devices and is applied for the purpose of quickly analysing reasonably long records. When operating digitally, it is not clear what advantages can be derived from it, as opposed to the methods based on estimates of the covariance function. What, if any, is the precise physical information conveyed by the filtered records, apart from their contribution to the estimation of the spectrum?

It is clear that peaks in the spectrum are best detected and located by the use of a method of statistical spectral analysis which optimizes the estimator of the derivative of the spectral density functions; this point was discussed during the Symposium held here last November.

Mr. G. A. Tunnell: For some years I have been analysing the frequencies of wet and dry bulb temperatures at Aden, Habbaniya and Accra for fixed hours and fixed months. The period of years used varies but is generally about seven. These periods are not long enough but are the best available with sufficient data. The work shows that the physical state of the atmosphere controls the random fluctuations, which may change discontinuously with a change of state.

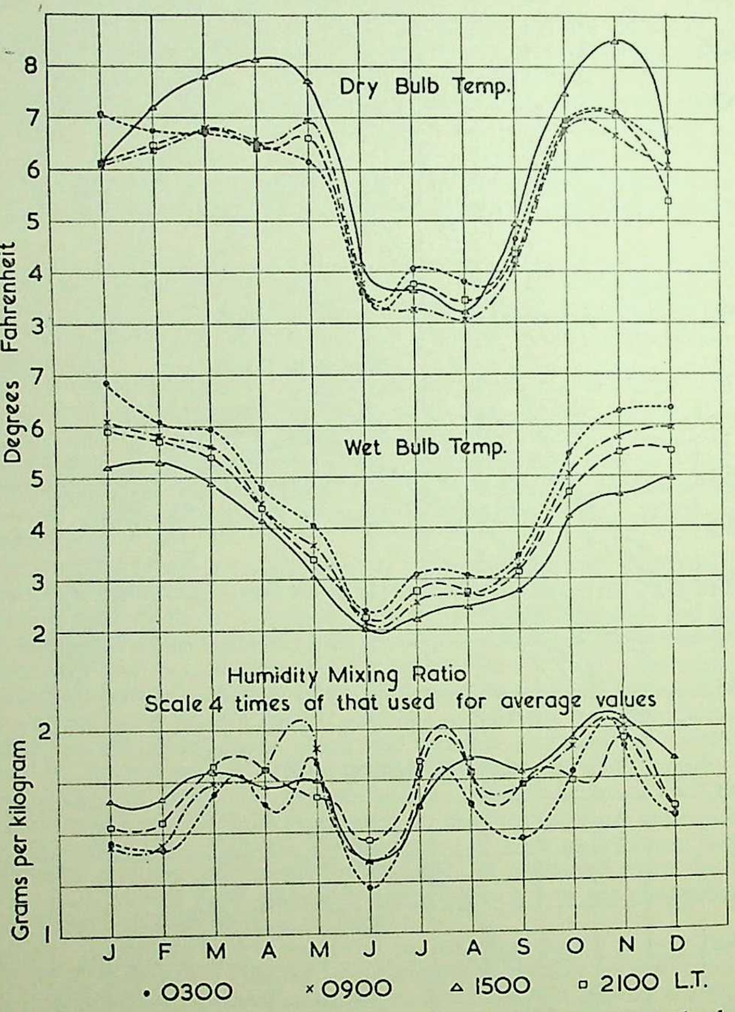

Fig. 2.—Monthly variation of standard deviations of wet bulb temperature, dry bulb temperature, and humidity mixing ratio at specific hours at Habbaniya.

At Habbaniya, for example, the seasonal changes in the long-period average temperature and long-period average wet bulb temperature are smooth with no obvious discontinuities. The standard deviations, however, for each hour over a long period show a large fall at the time of the discontinuous change from the low polar to the high tropical monsoon tropopause. [This change has been described in Meteorological Office Professional Note No. 107 by Frost.] I have deduced from this that the fluctuations of the day to day observations are largely a product of macroscopic turbulence and the general stability of the atmosphere. To check this I have correlated standard deviations with average values for fixed hours for individual months at all three stations and have got significant correlations. The fluctuations of wet and dry bulb temperature appear to depend on their average values. If one considers the monthly average wet and dry build temperature as a measure of the average state of the atmosphere during the month then the level of fluctuation during that month is a function of its mean state. I have divided the period at Aden into months of purely easterly flow and months of purely south-westerly flow and I have treated the monthly average temperatures and standard deviations of temperatures (i.e. wet and dry bulb temperature) as bivariate distributions.

I will now discuss just a single result obtained. For 0600 G.M.T. (0900 L.T.) easterly flow there was no useful correlation between the standard deviation of dry bulb and its average value (these parameters are, of course, for single months) but the standard deviation of wet bulb σ_t' could be expressed in the following way

$$\sigma_t' = \cdot 26[\bar{t} - \bar{t}'] + \cdot 04[\bar{t} - 85]$$

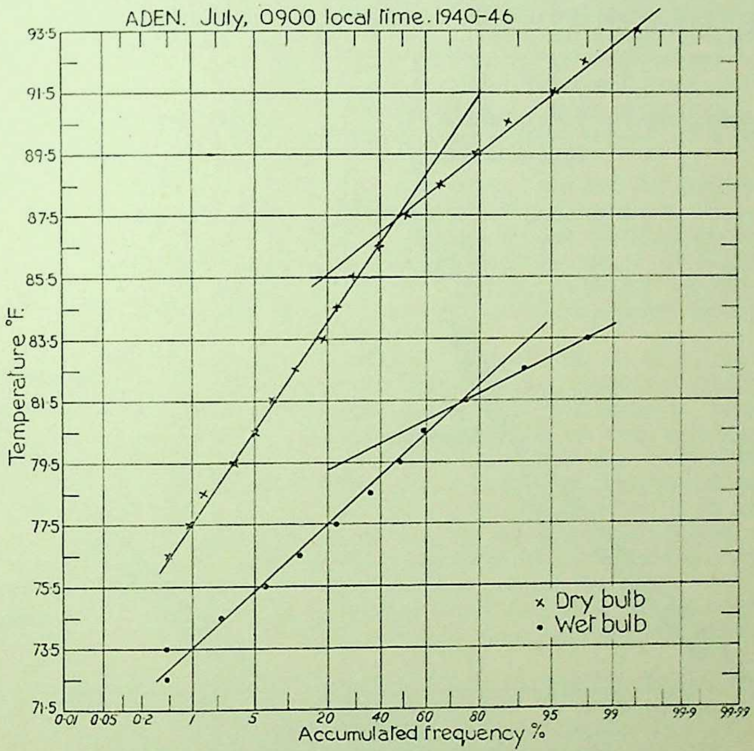

FIG. 3.—Accumulated frequencies of wet and dry bulb temperatures plotted on probability paper.

[where σ_t' = standard deviation of wet bulb temperature and $\bar{t}'$ and $\bar{t}$ are average values of wet and dry bulb temperatures for single months] with a highly significant multiple correlation of 0·73. This equation suggests control of fluctuations by the mean dryness of the atmosphere and its average dry bulb temperature. In the season of westerlies there were fewer observations but expressions were found for both σ_t' and σ_t (standard deviation of dry bulb temperature).

An examination of frequency distributions by means of probability paper has suggested some of the mechanisms by which the standard deviation and average values of wet and dry bulb temperatures may be related. The accumulated frequencies plotted on probability paper indicate sharp discontinuities of gradient and therefore of average value and standard deviation at specific temperatures. If these bends are very sharp they can only be produced by a linear transformation of the population above or below a specific temperature, i.e., increments are added or subtracted linearly to the original population. However, I believe these bends are not quite sharp but are hyperbolas. [This allows some randomness in a transformation from one population to another.] I believe the bends are caused by air mass modification [like cooling by passage over cold sea during upwelling, night cooling or subsidence] during which small increments are added or subtracted selectively to the original wet or dry bulb temperature. I have reproduced a number of typical bends in statistical experiments by taking two correlated sets of normal deviates (correlation coefficient r_{xv}). One set y is regarded as an unmodified temperature population and another set x with a small average is the population of modifying increments which are added to y selectively to form a new population z. To produce an upward bend x is added to y only when x is positive. i.e.

$$z = y, \qquad x \leqslant 0$$

$$z = x + y, \qquad x > 0$$

In the experiments described above the average value of x is negative and only the positive members of the x population are used. This produces an upward bend; a downward bend could be produced similarly by taking an x population with a positive average and using only negative members. Diagram 4 gives the results of two of these experiments, in which the average of the y population is $\bar{y}$ and of the x population is $\bar{x}$ and the corresponding standard deviations are σ_x and σ_y. The dots show observed frequencies and the circles indicate estimated frequencies assuming the probit lines are hyperbolas. The results of these experiments have been applied to actual wet and dry bulb temperature observations by means of probit techniques described by Finney, and the various population elements [i.e. modified and unmodified populations] have been estimated by means of the principles of least square and maximum likelihood.

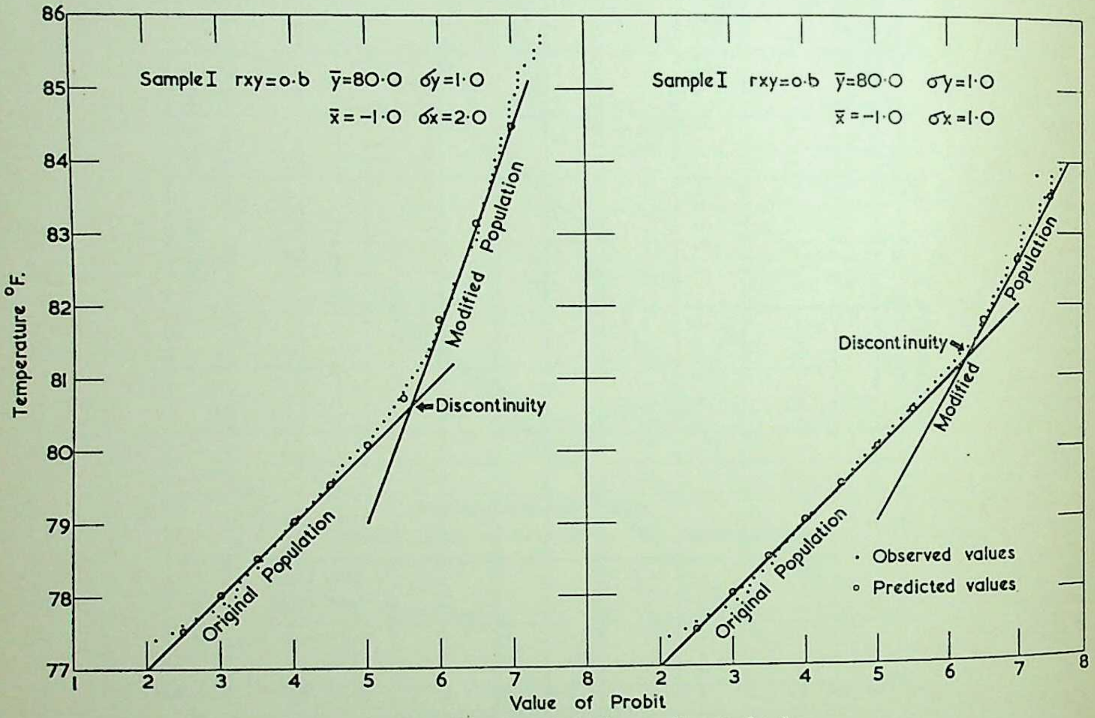

FIG. 4.—Results of statistical experiments in the synthesis of temperature populations.

Mr. R. G. VERYARD: The question must be asked whether in using mean temperatures one can be quite satisfied that values are being used which are separated by equal intervals of time.

Mr. CRADDOCK: I was very conscious of the difficulty of getting a good estimate of the number of degrees of freedom: I think it is correct to say the longer you make the filter and the more perfect it is, the smaller the uncertainties. These uncertainties do not arise in the perfect filter but proceed from the rough and ready nature of the filters in use. I was willing to take the risk of a little uncertainty on the statistical side because I wanted to get an idea of the atmosphere on the physical side. If you take a 240 term filter the filtered series represents something which is averaged over groups of 240 terms, and if the phase of an oscillation is changed several times during the time interval covered by the filter, the average is likely to be very different from the series at a particular time. My filters which have only 13 terms are less efficient than longer filters for eliminating unwanted oscillations, but more efficient at showing phase changes in the oscillations which interest us. It occurred to me that the fact that Mr. Charnock got considerable transfer in the 30-day band and very little in the 10-day band, might be due to the averaging effect of his 240 term filters. My diagram 7 relating to the 30-day band shows that it was not a badly disturbed oscillation: a 240 term filter, which spans about eight wavelengths at a time, would pass this oscillation fairly accurately. On the other hand diagram 6 shows that the 10-day wave is most heavily disturbed: a 240 term filter operating on this oscillation would almost certainly smooth out everything of interest.

With regard to the choice of band widths, if one wants to identify a strict periodicity such as the semi-annual variation this can be done by narrowing the spectral window; that is, by using a filter which passes the exact frequency but has a lower acceptance for neighbouring frequencies. By this means the normalized variance is increased, and perhaps reaches statistical significance. If, however, the process is carried too far, it will be found that the number of degrees of freedom will drop, say, to 4 or 2 and the variance ratio can reach quite high values without its being significant. In practice, one knows how long a series is available for investigation, and chooses filters which divide it into parts each having a reasonable number of degrees of freedom. If the filtering is carried too far nothing significant can be obtained because there are too few degrees of freedom attached to some of the filtered components.

There is also the question of the annual variation in the 5-day and the daily series; this was eliminated by subtracting the best fitting two-term harmonic approximation. In dealing with the monthly and longer period data two of the fitting points in each set of filters were arranged so that the annual and semi-annual variations were passed intact by one of the filters in the set and were removed entirely by the other filters of the set. The zero frequency was also fitted, so that any long-term trend was passed intact by one filter of the set and removed by each of the others.

Professor P. A. SHEPPARD (amplified in writing): I would like to stress that the problems facing a geophysicist are those arising from non-stationary time series and that we have so far received very little assistance from the statistician in dealing with such series. Nearly every essay on time-series analysis in the literature of statistical theory includes some such sentence as "It is assumed that any trend, if present, is first removed". I hope that one result of today's meeting will be that we shall receive more guidance than has been available hitherto on non-stationarity which is not confined to geophysical time-series and, I suppose, characterizes all real time-series.

Mr. Charnock has demonstrated in his paper that one way round the problem of non-stationarity is to ask a different kind of question about such series from the questions we generally ask when dealing with stationary time series. We should no longer describe a series in terms of *a* mean value and *the* associated turbulence, but give every scale of disturbance present an equal importance, analytically speaking. The idea is not, of course, new, but Mr. Charnock has made it definite and usable.

I would like to pose the problem of non-stationarity in slightly different terms. Given a time series of length T say, what is the range of periods in the power spectrum which can be reliably extracted from the record, and given series of two variables each of length T what is the range of periods which can be reliably extracted in the co-spectrum? The trends assumed to be present in the series not only set an upper limit to the periods which can be reliably extracted but may also determine the spectral form in the range which can be reliably extracted. This implies the need for a suitably concise specification of the larger scale components excluded from the derived spectra. (These larger-scale components are not, of course, in general any less simple than the smaller scale components although a long-period such as an annual variation may make a large contribution to variance.) The analysis of a family of such series, observed at different times but all of length T, might then be a useful method of investigating the effects of different scales of disturbance on each other. Until we know what these procedures should be and have tried them out it is difficult to guess what the ultimate physical value of time-series analysis as usually conceived might be in geophysical research.

Professor BARTLETT: Not so much stationarity was found as one would like. It might perhaps be regarded partly as a matter of interpretation, as an apparent non-stationary component might be part of a stationary series with components of long wavelength; but there is one fundamental warning which one must make and that is that questions cannot be dealt with statistically unless there is replication of some kind. In a stationary time series one expects to get replication by the repetition of the pattern as one goes along in time. With a non-stationary series there will be a limitation on the questions that can be answered unless there is replication by some other means of the same non-stationary series.

Mr. Craddock: The temperatures used were either the average of 24 daily temperatures or if that could not be obtained, something approximating this. I do not think this sort of uncertainty or that arising from treating months as of equal length compares with the importance or the scale of the things we are trying to find out. Accepting such uncertainties is one of the risks one is justified in taking.

The following contributions were received in writing after the meeting.

Mr. A. M. Walker: In the applications of time series techniques to geophysical problems which have been considered by the contributors to this symposium the emphasis has been mainly on spectral analysis. I should like to mention a somewhat different type of application in connection with the problem of the variation of latitude, with which I have recently been concerned in collaboration with Mr. A. Young of Liverpool University. Here the data consisted of values of direction cosines l, m of the Earth's instantaneous axis of rotation relative to axes fixed in the Earth, calculated from astronomical observations which give the changes in the latitudes of a number of stations. Dynamical theory yields the equations of motion

$$\dot{l} + \kappa l + \gamma m = -\omega_0 F/A = -F_1, \dot{m} + \kappa m - \gamma l = \omega_0 G/A = G_1,$$

where ω_0 is the mean angular velocity of the Earth, (A, A, C) are moments of inertia and F, G products of inertia of the Earth, $2\pi/\gamma$ is the "Chandler" period of the free motion of the axis of rotation, and κ is a damping factor. The determination of κ and γ from series of calculated values of l and m is of considerable geophysical interest, the value of the free period being particularly important since it gives a direct determination of one of "Love's numbers" which provides a measure of the Earth's elasticity.

A statistical model for the series (l_n, m_n) of values of l, m at times separated by a constant interval was formulated by assuming that $F_1(t) = F_s(t) + f_1(t)$, $G_1(t) = G_s(t) + g_1(t)$, where the "systematic" terms F_s, G_s are functions of t with a yearly period and the "fluctuation" terms f_1, g_1 are independent stationary impulse processes (with zero means)—so that only their integrals, which are independent homogeneous additive processes, strictly exist. These assumptions seemed a reasonable approximation in view of the way in which changes in F_1 and G_1 arise, the main causes of these being movements of air masses and the accumulation of snow and subsequent melting. It follows from them that $l_n = l_{sn} + x_n$, $m_n = m_{sn} + y_n$, where l_{sn}, m_{sn} are systematic terms with a yearly period, and (x_n, y_n) is a bivariate autoregressive process with independent residuals. We therefore had a fairly simple parametric model of a type for which inferential methods are by now quite well established, the estimation of the parameters being performed by a combination of harmonic analysis and autocorrelation analysis. However it was necessary, as was pointed out by Sir Harold Jeffreys, who in 1940 considered a similar model, and seems to have been the first writer to subject variation of latitude data to a proper statistical treatment, to take account of superimposed observational error by the addition of independent components (u_n, v_n) (which might be comparable with (x_n, y_n)) to (l_n, m_n). This modification of the model, which is probably required less infrequently than might be supposed from an examination of the existing literature on time series analysis, makes the autocorrelation analysis appreciably more complicated, and because of it we found it necessary to introduce the additional assumption that the distributions of both (x_n, y_n) and (u_n, v_n) were normal. A detailed account of this work and its application to the analysis of a 35-year series of observations has been given by us in a recent paper—Walker & Young, *M.N.R.A.S.*, **115**, 443–459, 1955.

Since then we have carried out several more analyses on a number of different series covering different intervals of time and obtained from the original observations in different ways, and the results of these are discussed in a further paper which will appear in *M.N.R.A.S.* **117** Part 2 shortly. Here we also examine the question of inhomogeneity in the data, due for example to changes in the number of stations in action, in the sets of stars used for observation, and in the method of calculating l and m, and discuss the bias in the estimates of the free period and the damping factor due to this inhomogeneity (of which we find definite evidence) and other causes such as correlation between the residuals of the autoregressive process (which may very well not be negligible as we previously assumed).

Mr. M. B. Priestley: I should first like to add my thanks to those of previous speakers for the interesting methods and applications which have been presented to us. In particular, the use of linear filters has been suggested as a means of constructing estimates of quantities which are effectively band spectra, and of using such estimates as a basis of a test of randomness of the original series. Although this device has previously been employed as the appropriate method of analysing a *continuous* record on an *analogue* computer, it is surprising to learn that it has been used for analysing *discrete* series on a *digital* computer. Apart from the problem of non-stationarity (for which it is doubtful whether a satisfactory solution exists anyway), theoretically the method of filters seems to have little advantage over the more usual correlogram and periodogram methods for estimating power spectra. In fact, there is a simple relationship between the two methods (which holds for both continuous and discrete processes) and which may be demonstrated as follows.

For a continuous process $X(t)$, we have the fundamental Fourier expansion in the form,

$$X(t) = \int_{-\infty}^{\infty} e^{i\omega t}\, dZ(\omega)$$

where $Z(\omega)$ is an orthogonal process, and the integral is defined in the mean square sense. A process $Y(t)$ which has been obtained by filtering $X(t)$ may be written as

$$Y(t) = \int_{-\infty}^{\infty} X\,(t-u)\, g(u)\, du.$$

Then

$$Y(t) = \int_{-\infty}^{\infty} e^{it\omega}\, [h(\omega)\, dZ(\omega)]$$

where

$$h(\omega) = \int_{-\infty}^{\infty} g(u)\, e^{-iu\omega}\, du$$

(corresponding to what has been termed the "filter factor".) If we now define

$$h(\omega) = \begin{cases} 1, & \omega_1 \leqslant \omega \leqslant \omega_2 \\ 0, & \text{otherwise} \end{cases}$$

then

$$\begin{aligned} Y(t) &= \int_{\omega_1}^{\omega_2} e^{it}\, dZ(\omega) \\ &= \frac{1}{2\pi} \int_{-\infty}^{\infty} X(t-u) \left(\frac{e^{i\omega_2 u} - e^{i\omega_1 u}}{iu} \right) du. \qquad . \quad . \quad . \quad . \qquad (1) \end{aligned}$$

Given a sample $\{X(t)\}$, $0 \leqslant t \leqslant T$, take

$$Z_T(\omega) = \frac{1}{2\pi} \int_0^T \mathrm{X}\,(t) \left(\frac{1 - e^{it\omega}}{it} \right) dt$$

(so that $Z_T(0) = 0$) and estimate (1) by

$$Y_T(t) = \int_{\omega_1}^{\omega_2} dZ_T(\omega)\, e^{it\omega}\, d\omega.$$

Then it may be shown that (cf. Walker (1957), *Biometrika*, **43**, 257–275)

$$s_Y^2 = \frac{1}{T} \int_0^T Y_T(t)\, Y_T^*(t)\, dt = \int_{\omega_1}^{\omega_2} I_T(\omega)\, d\omega + R$$

where

$$\mathscr{E}(R) \longrightarrow 0 \text{ as } T \longrightarrow \infty.$$

Here $I_T(\omega)$ is the periodogram, defined as

$$\frac{1}{2\pi T}\left|\int_0^T X(t)\,\overline{e}^{i\omega t}\,dt\right|^2.$$

For discrete processes, we may write the filtered series in the form

$$y_i = \sum_{j=-k}^{k} \omega'_j\, x_{i-j} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \tag{2}$$

where, in Mr. Craddock's notation,

$$\omega'_{-j} = \omega'_j = \tfrac{1}{2}\omega_j, \qquad j \neq 0,$$

$$\omega'_0 = \omega_0.$$

If the x_i's are independent (e.g. uncorrelated and normal), $\{y_i\}$ is generally called a "linear process". It then follows from (2) that

$$\sigma_y^2 f_y(\omega) = \sigma_x^2 f_x(\omega)\ .\ |\,M(i\omega)\,|^2 \quad . \quad . \quad . \quad . \quad . \quad . \tag{3}$$

where f_x, f_y, σ_x^2, σ_y^2 are the spectral densities and variances of the two series, and

$$M(i\omega) = \Sigma_j\, \omega'_j\, e^{-i\omega j} \qquad \text{(writing } \omega = 2\pi f\text{).}$$

Integrating (3) from $-\pi$ to π, we have,

$$\sigma_y^2 = \sigma_x^2 \int_{-\pi}^{\pi} f_x(\omega)\ .\ |\,M(i\omega)\,|^2\, d\omega$$

and we also have the corresponding asymptotic result for the sample function (cf. Bartlett (1955), *Introduction to Stochastic Processes*, p. 279)

$$s_y^2 = \frac{1}{n}\sum_{i=1}^{n} y_i^2 = \int_{-\pi}^{\pi} I_{n,\,y}(\omega)\, d\omega$$

$$\sim \int_{-\pi}^{\pi} I_{n,\,x}(\omega)\ |\,M(i\omega)\,|^2\, d\omega + 0\left(\frac{1}{n}\right)$$

where $I_n(\omega)$ is the periodogram. This latter form is of the general type studied by Grenander and Rosenblatt, and is nothing more than a smoothed periodogram. If the original series is random (i.e. has a "white spectrum"), then

$$f_x(\omega) = 1, \text{ all } \omega,$$

so that

$$\sigma_y^2 = \mathscr{E}(s_y^2) \sim \sigma_x^2 \int_{-\pi}^{\pi} |\,M(i\omega)\,|^2\, d\omega$$

$$= \sigma_x^2\ .\ P(\omega).$$

(assuming $\mathscr{E}(x_i) = 0$)

(It might be pointed out at this stage that even if the terms of the original series are random, those of the filtered series are not (in general), and consequently the variance estimates obtained from these filtered series are no longer strict χ^2. However, even if an approximate χ^2 distribution could be shown to hold, an appropriate test of randomness would be something similar to Professor Bartlett's homogeneity of χ^2 rather than the ordinary F-test.)

If we now go back to the relation,

$$s_y^2 = \frac{1}{n}\sum_{i=1}^{n} y_i^2 \sim \int_{-\pi}^{\pi} I_{n,\,x}(\omega)\ .\ |\,M(i\omega)\,|^2\, d\omega + 0\left(\frac{1}{n}\right)$$

it appears that one can approach the "perfect filter" (corresponding to $M(i\omega)$ as a step function) much more closely by considering simply

$$\int_{\omega_1}^{\omega_2} I_{n,\,x}(\omega)\,d\omega$$

corresponding to the Daniell estimate of the band spectrum. Confidence bands for this estimate were discussed at the recent symposium on spectral analyis.

The use of filters may be a suitable method for use with an analogue computer where, in general, a high degree of accuracy is not required. It may even prove useful as an alternative to the periodogram method, requiring (perhaps) less numerical computation. However, I cannot see what advantage is gained by using filters when an electronic computer is available (cf. Mr. Charnock) and the amount of numerical computation is almost irrelevant.

Dr. Neumann and Dr. Rushton have discussed the analysis of mixed spectra and have referred to Whittle's iterative method which he applied to the analysis of a Sieche record. It has previously been pointed out (see the discussion to the Symposium on Spectral analysis) that this method suffers from several disadvantages, in particular with regard to the approximate standardization of the periodogram. A typical autocorrelation function of "mixed" process (e.g. see the references to Seiwell (1949*a*,*b*)) will contain a periodic component $\rho_s^{(1)}$ (corresponding to the discrete part of the spectrum), and a component $\rho_s^{(2)}$ (corresponding to the continuous part of the spectrum) which dies out as s increases. This suggests an alternative approach to the problem. Given a sample $X_1, X_2, \; . \; . \; . \; X_N$, consider

$$P(\omega) = \sum_{n > |s| \geqslant m} \left(1 - \frac{|s|}{n}\right) C_s \cos s\omega$$

where the C_s are the sample covariances, m is chosen so that

$$\rho_s^{(2)} \sim 0 \text{ for } s > m$$

and $n \to \infty$ with N so that $\lim\limits_{N\to\infty} \dfrac{n}{N} = 0$. It follows that if $\rho_s^{(1)} \equiv 0$ (i.e. there are no periodic components in the process),

$$\mathcal{E}[P(\omega)] \sim 0.$$

Further, it may be shown that

$$\operatorname{var}[P(\omega)] \sim \begin{cases} \dfrac{1}{N}\left(\dfrac{2n}{3} - 2m + \dfrac{2m^2}{n}\right) . f^2(\omega) & \omega \neq 0 \\[2ex] \dfrac{2}{N}\left(\dfrac{2n}{3} - 2m + \dfrac{2m^2}{n}\right) . f(0), & \omega = 0, \end{cases}$$

and

$$\operatorname{cov}[P(\omega), P(\omega')] \sim 0\left(\frac{1}{N}\right), \qquad \omega \neq \omega'.$$

However, if the process contains a harmonic component of frequency λ,

$$\mathcal{E}[P(\lambda)] \sim 0(n - 2m).$$

On the basis of these results it is possible to construct a significance test of $P(\omega)$ similar to that used by Grenander and Rosenblatt for deriving confidence bands for the integrated spectrum. This method has the advantage that in addition to using a statistic whose expected value is asymptotically independent of $f(\omega)$, it does not require the computation of the complete correlogram. For the test to work efficiently we must restrict the bandwidth of any peaks in the spectral density (i.e. we cannot allow m to become too large). However, since Professor Bartlett has already pointed out (see the discussion to the Symposium on spectral analysis) that given a finite sample one cannot possibly hope to distinguish strict harmonic components from sufficiently narrow peaks in the continuous spectrum, it seems essential to impose some such condition on the smoothness of the spectral density.

At present this method is being tested on several artificial series of the "mixed" type.

Professor H. Wold: The authors have given a forceful demonstration of the scope and power of modern methods of time series analysis. Their line of approach is most promising, in regard of the broad sector of potential applications, and also because the methods are still in an early stage of development. As yet the methods are primarily designed for long series with a stable structure; it is therefore highly desirable to develop methods that are less demanding with regard to the number of cycles covered by the data. Such more flexible methods are called for not only in the socio economic sciences but also in many applications in the natural sciences. There is also the point that the statistical methods at issue have only partly exploited the recent rapid development in the pure mathematics of spectral theory and related topics. In particular I am thinking of approximation by the use of linear manifolds of translations $f(x - t)$ of a given function $f(x)$; for a fundamental result reference is made to A. Beurling, *Acta Mathematica*, 77 (1945), 127.

Mr. M. J. TUCKER: Mr. Craddock's paper raises some interesting problems in connection with the "equivalent number of degrees of freedom" of a filter. Consider a stationary gaussian signal (though this seems to be becoming old-fashioned!) whose power spectrum $E(f)$ is shown diagrammatically in the figure. The response of one of Mr. Craddock's filters to power is $M^2(f)$ so that the spectrum of the signal after passing through his filter is $E(f)\, M^2(f)$.

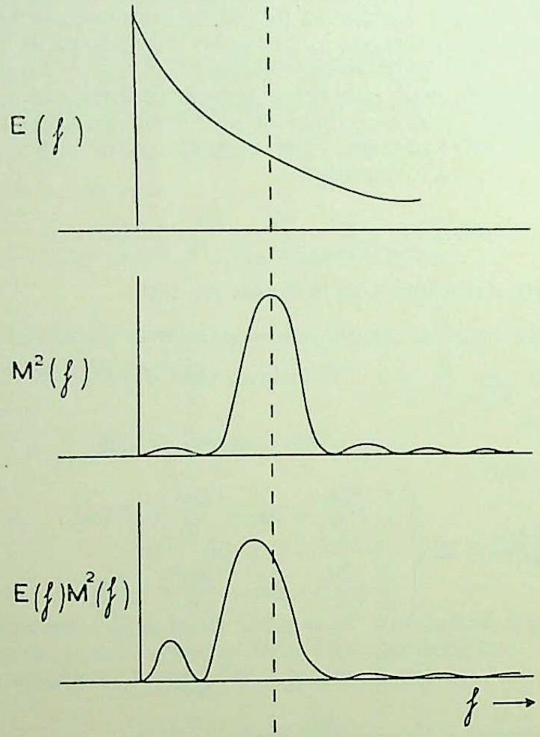

FIG.—Diagrammatic representation of the energy spectrum $E(f)$ of a fluctuating quantity, the energy response $M^2(f)$ of a filter, and the spectrum of $E(f)$ after passing through the filter.

Using Mr. Craddock's notation where applicable, if we take a record of length N units of time, where $1/N$ is small compared with the bandwidth of the filter, it can be shown by surprisingly simple arguments (Tucker, 1957, *Brit. J. Appl. Phys.*, **8**, 137) that the variance σ_v^2 of the statistical error in the estimate of the total power coming through the filter is given by

$$\sigma_v^2 = \frac{1}{N} \int_0^\infty E^2(f)\, M^4(f)\, df \quad . \quad . \quad . \quad . \quad . \quad (1)$$

If $\overline{V_w(y)}$ is the average over many independent samples of $V_w(y)$, that is, it is the quantity we are estimating, then

$$\overline{V_w(y)} = \int_0^\infty E(f)\, M^2(f)\, df,$$

and it is convenient to write equation (1) in the form

$$\frac{\sigma_v^2}{V_w(y)^2} = \frac{1}{N} \frac{\int_0^\infty E^2(f) M^4(f)\, df}{\left[\int_0^\infty E(f)\, M^2(f)\, df\right]^2} \quad . \quad . \quad . \quad . \quad (2)$$

It is thus apparent that the proportional error is not dependent solely on the filter characteristics, and a filter cannot strictly be said to have an equivalent number of degrees of freedom. However, in most cases the filters will be comparatively narrow and $E(f)$ may be treated as constant within their pass band.

A further difficulty arises owing to the use of discrete data. From Mr. Craddock's equation (4), $M(f) = M(1 - f) = M(1 + f) = M(2 - f)$, etc., so that the filter has an infinite number of pass bands. It is necessary to ensure, therefore, that $E(f)$ is zero for $f > \frac{1}{2}$ if the results are to be interpreted simply.

Assuming that $E(f)$ obeys both these conditions, equation (2) becomes

$$\frac{\sigma_v^2}{V_w(y)^2} = \frac{1}{N} \frac{\int_0^{\frac{1}{2}} M^4(f)\, df}{\left[\int_0^{\frac{1}{2}} M^2(f)\, df\right]^2} \quad . \quad . \quad . \quad . \quad (3)$$

Now a chi-squared distribution with N_1 degrees of freedom has a proportional variance of $2/N_1$ about its mean value, so equating this to the right-hand side of equation (3) we obtain the effective number of degrees of freedom of the filter

$$N_1 = 2N \frac{\left[\int_0^{\frac{1}{2}} M^2(f)\, df\right]^2}{\int_0^{\frac{1}{2}} M^4(f)\, df}$$

This is a rather more complex expression than the guess made by Mr. Craddock.

There remains one minor pitfall. The probability distribution of $V_w(y)$ is *not* in general a chi-squared distribution and will not have the same confidence limits. However, they are not likely to differ much and for large values of N_1 they both tend to normal distributions, so that this difference is not usually important.

I should add that equation (1) and some of the subsequent arguments assume that $1/N$ is small compared with the scale of any changes in $E(f)$ with f.

A cordial vote of thanks to the authors, proposed by Professor Barnard and seconded by Mr. Fieller, was put to the meeting and carried unanimously.

Mr. Craddock and Mr. Charnock subsequently replied in writing as follows:

Mr. Craddock: I hope by filtering meteorological time series to separate atmospheric fluctuations into parts, each due to a different group of physical processes. In particular, I may be able to separate fluctuations arising from the dynamics of the atmospheric motion from those of thermodynamic origin, depending on the processes which transfer heat poleward from low latitudes to make good losses by radiation in high latitudes. The power spectrum analysis is really a preliminary to decide which frequency bands are likely to repay investigation, the main purpose being to examine the actual oscillations lying in each important frequency band, to study their variations from place to place, and if possible to establish which physical processes are producing them. By filtering we can obtain the basic material for these steps, whereas I do not see how periodogram analysis or correlogram analysis could provide it.

I am grateful for the suggestions and advice offered by contributors to the discussion and particularly to Mr. Tucker, whose formula I hope to use in future work.

Mr. Charnock: I should like first to express my appreciation of Mr. Craddock's paper. He and I are using similar methods from a somewhat different point of view and with a different object: nevertheless our results afford interesting comparisons and suggest that the use of linear filters is a potentially valuable technique.

In connexion with the problem discussed in Section 5 of his paper it is of interest to recall some of the results given in the comprehensive papers of Rice (*Bell Syst. Tech. J.*, **24**, 93 *et seq.*). If we denote a sample variance of a gaussian series by

$$V \equiv \frac{1}{T} \int_0^T x^2(t)\, dt$$

then

$$\sigma_T{}^2 \equiv \overline{(V - \overline{V})^2} = \frac{4}{T} \int_0^T \left(1 - \frac{\tau}{T}\right) C^2(\tau)\, d\tau$$

where

$$C(\tau) \equiv \overline{x(t)\, x(t+\tau)} \qquad C(0) = \overline{V} = m \qquad \text{say.}$$

Rice gives an approximate distribution for V; the probability of V lying in dV is, approximately,

$$\frac{1}{\Gamma(n+1)} \left(\frac{mV}{\sigma_T{}^2}\right)^n \exp\left(-\frac{mV}{\sigma_T{}^2}\right) d\left(\frac{mV}{\sigma_T{}^2}\right) \qquad n \equiv \frac{m^2}{\sigma^2} - 1$$

This would provide a suitable test for Mr. Craddock. If we assume that the original unfiltered series is gaussian and with uniform spectrum we could take m as its variance and

$$C(\tau) = \int_0^\infty w(f) \cos 2\pi f \tau\, df$$

is known from the filter specification.

When T becomes large

$$\sigma_T{}^2 \simeq \frac{1}{T} \int_0^\infty w^2(f)\, df, \qquad m = \int_0^\infty w(f)\, df$$

and the distribution of V is approximately gaussian. In these conditions the effective number of degrees of freedom for one of Mr. Craddock's filters would be, in his notation,

$$N_1 = 2N \frac{\left[\int_0^{\frac{1}{2}} M^2(f)\, df\right]^2}{\int_0^{\frac{1}{2}} M^4(f)\, df}.$$

This result is that given by Mr. Tucker in his contribution to the discussion.

Following Professor Bartlett's comment it may be of interest to record that in his paper Rice states that the correlation function apparently was introduced by G. I. Taylor (in 1920 not in 1938) but that Kenrick (1929) appears to be one of the first to apply to noise problems the correlation function method of computing the power spectrum.

My knowledge of statistics is so limited that I cannot do more than thank most of the contributors to the discussion for their interest. It is kind of Dr. Jenkins to summarize the advantages of numerical filtering and to review some of the difficulties of interpretation. Unfortunately he is incorrect in stating that the output from a particular filter is itself a stationary series. I have tried to clarify the section which, as he pointed out, had undefined symbols but I have left my contention that correlation and spectral analysis are formally equivalent. I have no opinion on the theoretical advantages of spectrum and harmonic analysis in various circumstances and look forward to reading of Dr. Jenkins's work on the subject. I agree that there are many non-stationary series—indeed I doubt if there are any which are stationary. The peculiar feature of many meteorological and oceanic time series (apart from the fact that they fascinate me) is that the variables obey the equations of motion and have fluctuations over a broad range of scale.

The problem of non-stationary series is also stressed by Professor Sheppard, to whom I am grateful for his kind comments and for his continued interest in my struggles with this and related problems. I regret that I cannot give a brief answer to his main question. The power spectrum is so closely linked to the idea of stationary series that I would much prefer to reformulate his question. Nevertheless I fully agree with his suggestion that the analysis of a family of curves would be valuable. This is an extension of my plea for the analysis of a collection of records and is doubtless the sort of replication of which Professor Bartlett has so rightly stated the necessity.

I would not recommend the use of numerical filters for estimating band spectra in series which are assumed to be stationary. Other methods need less computing and are, as Mr. Priestley shows, formally equivalent. I regard them as a reasonable first approach to the problem of non-stationary series to which, according to Mr. Priestley, it is doubtful whether a satisfactory solution exists anyway. I believe our different points of view arise only from imprecise definition; the different points of view are admirably indicated in Professor Bartlett's second contribution, with which I completely agree.

I shall be more than content if my essay leads to more interest in large-scale fluid flow on the part of statisticians and to more careful specification of their results by meteorological and oceanographic observers.

As a result of the ballot taken during the meeting the candidates named below were elected Fellows of the Society:—

Alan Charles John Andrews
Ronald Frederick William Aylott
Sidney Callaway
John Colin Campbell
John Withrington Cooper
Merriss Cornell
Alison Grant Doig
Joseph Bertold Gilchrist
Thomas Ian Hardie
Douglas James Lawrence
Christopher William Maynard
Donald Stuart Muir
Norman Plant
Edward Sinclair
Zainu Adenyi Ajayi Shodeinde
John Sparrow

THE RELATIVE NUMBERS AND ABILITIES OF SOME TEN-YEAR-OLD TWINS

By FRANK SANDON

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1. *Introduction*

AFTER the Scottish Mental Survey of 1947, Mehrotra and Maxwell, (1949), in a review of the number and intelligence test scores of pairs of twins, compared results reported by five different workers. The maximum number dealt with by any of these workers was 468 pairs, so that when the writer, in the course of his duties, was able to compile returns relating to nearly a thousand individual twins, it seemed worth while looking his data over to see how far he could confirm, or otherwise, the conclusions reached by Mehrotra and Maxwell. The following is a summary of his results.

For a number of years past he has been the chief examiner to an English county local education authority, and has thus been concerned with examining annually, with a view to determining their admission to grammar schools, the complete age-group of children aged, approximately, ten years. In the course of several years he thus passed in review about 21,970 children—the exact figure is not available. From the data given on the mark sheets, or on other forms, he has identified 360 individual twins—61 pairs of like-sex boys, 61 pairs of like-sex girls, and 55 pairs of unlike-sex twins, with two sets of triplets. The sex ratio for all children was 102 boys : 100 girls. It may be noted that, as the three groups of twins are very nearly equal in size, at birth, there is no evidence that there is any obvious great differential death rates between the three groups.

2. *Proportion of Twins Aged Ten in an English County*

The above data give us a ratio of about 16 twin children for every thousand individuals. For five examinations, over which time the figures for the second and third years can be given approximately only, as the population at risk then is not known accurately, the figures are respectively 17·3, 12 (about), 18 (about), 16·5 and 16·6, so that it seems fairly steady at about 16 per mille.

Last year the Registrar General reported that there were 16,340 children born as living members of pairs of twins among a total of 673,651 live births, a rate of about 24·25 per mille, so that our figure of 16 per mille is only about two-thirds of that expected. This is the kind of result found by Mehrotra and Maxwell. They say of the Scottish Mental Survey that it "contains, as nearly as may be, two-thirds of the estimated number of pairs. It is unfortunate that a considerable number of twins have apparently not been recorded as such . . . ".

As it was desirable for purposes of policy decisions that the accurate number of twins should be known, the administration went to some trouble to find the missing pairs. They were not successful in appreciably increasing the proportion. There were a few children who did not enter for the examinations, as they were regarded as "educationally subnormal", and though the number of twins among them was not known (it may be rather above the "expected" number in view of the level of ability found among twins, to which point we return later) the number of such E.S.N. children, together with those persistently absent, did not account for more than, at most, some 3 per cent. of the total "field", so that no sufficient explanation is to be found here.

The count of the twins may be slightly at fault, for, as the forms did not describe the twins as such, they had to be identified from the entry forms. This meant looking for pairs of children with the same surname, the same guardian, the same address, and with dates of birth say at most two days apart. Some individual forms, especially those relating to children of unlike-sex and those where the two of a pair were at different schools, may not have been brought together and there may be very slight errors arising from other features, but the total error was thought to be far less than could account for the "lost" one-third of cases.

3. *A Shortage of Twins and an Explanation*

The explanation is probably to be found, I suggest, in another way. The twins reported are individuals who are now members of surviving pairs of twins.

Suppose that we have 100 pairs of twins, and that they are individually subject to a death-rate of, say, 10 per cent. Then, if there is no correlation between the two of the pair in this matter, we shall have a table such as Table 1, the individuals of each pair being designated Castor or Pollux by the use of random sampling numbers. There will be only 81 pairs of survivors. If there is perfect correlation, then we shall have a table such as Table 2 for the same individual death rates. In this there are 90 pairs of survivors.

TABLE 1

	Castor		
Pollux	*Survives*	*Dies*	*Total*
Survives .	81	9	90
Dies . .	9	1	10
Total .	90	10	100

TABLE 2

	Castor		
Pollux	*Survives*	*Dies*	*Total*
Survives .	90	0	90
Dies . .	0	10	10
Total .	90	10	100

Tables for intermediate values of the correlation ratio can, on the assumption of normal distributions, be computed by the use of Karl Pearson's Tables for the Volume of the Normal Bivariate Surfaces. An extract from the resulting table is as follows:

TABLE 3

Survival Rates of Pairs of Twins under Various Conditions

	Survival Rate of Individuals of Pairs									
	·9452	·8413	·7257	·6554	·5000	·4207	·3446	·2119	·1151	·0548
Correlation within Pairs for Survival Rate of Individuals	*Effected by Dichotomy at Variate, in s.d. units, from Mean, of*									
	−1·6	−1·0	−0·6	−0·4	0	+0·2	+0·4	+0·8	+1·2	+1·6
·95 . .	·9313	·8107	·6835	·6088	·4495	·3712	·2980	·1753	·0906	·0409
·90 . .	·9257	·7981	·6659	·5892	·4282	·3504	·2784	·1600	·0806	·0353
·80 . .	·9180	·7802	·6407	·5609	·3976	·3204	·2501	·1383	·0665	·0276
·60 . .	·9079	·7551	·6041	·5200	·3524	·2763	·2092	·1073	·0471	·0175
·30 . .	·8987	·7281	·5620	·4717	·2985	·2239	·1609	·0726	·0271	·0083
0 . .	·8934	·7078	·5266	·4295	·2500	·1770	·1187	·0449	·0132	·0030
−·30 . .	·8904	·6843	·4675	·3495	·1476	·0808	·0387	·0057	·0004	·0000

Table 3 is based on the function

$$y = \int_h^\infty \int_k^\infty \frac{1}{2\pi(1 - r^2)^{\frac{1}{2}}} e^{-(x_1^2 - 2rx_1x_2 + x_2^2)/2(1 - r^2)}\, dx_1\, dx_2$$

where I, the survival rate for individuals given in the top line of the table, is the value of the function when $k = -\infty$, and P, the survival rate for pairs given in the body of the table, is the value where $k = h$.

There is a remarkable and unexpected approximate relation (one that I have not been able to prove) between I and P, that has not, as far as I have been able to trace, been previously referred to in the literature. If we plot $\log_{10} I$ against $\log_{10} P$ we find that the points seem to lie on a straight line, given by the relation $\log P = \rho \log I$, where ρ, a function of r, increases from $\rho = 1$ for $r = 1$ to $\rho = 2$ for $r = 0$ and to larger values of ρ for r negative.

Arithmetical substitution, however, indicates that, although the error is not noticeable using ordinary graphical methods, this relation is only approximately true (except for $r = 0$). There is a difference between the logarithm as estimated from the linear formula and that found from the table, though it is, for r positive and h negative, less than, at most, about ·005 (if ρ is determined from the tabular value for $h = 0$), corresponding to an error of estimate of, say, 1½ per cent.: the estimated value is slightly too large. If r is negative, the estimated value is slightly too small. For h positive, the error changes sign and its relative magnitude increases with h, so that, e.g., for $r = \cdot 80$ and $h = +0\cdot 8$, the estimate is ·127, about 10 per cent. too small compared with the value for P given by the table, ·138.

4. *Application of the Differential Survival Rates for Individuals and for Pairs of Twins*

Life tables give that, for every hundred children born, about 95 survive to the age of ten.

The death rate of twins is heavy. Karn (1952, 1953) has given figures which suggest that it is about five times as heavy as that for other children. Her figures relate, apparently, to children still in the maternity wards with the mother, but this is when their death rate is heaviest. Padley (1956) has pointed out that the difference between twins and other children has probably worked itself out soon after the discharge from hospital. If, then, we take the death rate among normal children over the first ten years of life as ·05, the part of this during the time when twins are subject to the extra heavy death rate may, perhaps, be taken as ·025 for ordinary children and thus ·125 for twins, with the balance of ·025 for both over the later years up to ten. So that we may take for our approximation a death rate for individual twins over the first ten years of life as about ·15.

Karn has also given the correlation for death rates within pairs of twins as about ·5. The usual correlation between sibs for any physical characteristic is about ·6, so that we may take the figure of ·5 or ·6 to apply for individual twin death rates over the whole of the first ten years of life.

If, then, we take the above data, i.e. survival rate for individual twins as ·85 and correlation between death rates of such twins as ·5, we find from our table that the survival rate for twin pairs surviving as such is about 0·75. This means that there are 75 per cent. pairs of twins surviving as such compared with 95 per cent. of children surviving as such, or only about 80 per cent. of the "expected" number of twins.

The figures used above are only approximate. But they have reduced the expected number, though not to the required extent. A smaller correlation, or a longer period over which the death rate of twin individuals is more than that of the ordinary child, would bring our 80 per cent. down, so that it may be that fuller information will find here a complete explanation of the "missing" twins.

I have not been able to find any more accurate figures about the death rates of individual twin children or about the within-pair correlation for the death rate of twins over the first ten years of life.

5. *The Proportion of Twins Aged Ten in an English County Borough*

The writer has also been responsible for a number of years for examining children aged ten in a large county borough for grammar schools selection. In this case, only those children whose parents wished it took the examination; in addition, children from outside the city were able to sit. The number of children at risk is thus not known precisely. This is unfortunate as there is some self selection. This takes two forms. There is self selection between the sexes—girls enter slightly more freely than do the boys (the sex ratio for candidates over eight years of examination is about 94 boys : 100 girls; it is fairly steady; the range is from 90 boys : 100 girls to 99 boys : 100 girls). In addition the "extra-district" candidates are not an unbiased sample of the outside children or of all children; on the whole they are of ability above the average.

Over eight years there were 64,389 candidates, and among these there were 628 individuals identified as twins (95 pairs of like-sex boys, 116 like-sex girls, 97 unlike-sex pairs, and four sets of triplets). Only 524 actually finally sat all the examination. Twins had been identified as before, though the head teachers of the contributory schools had been asked in the later years specially to report about all their twins. Even then, from other information, it appeared that some were omitted. But the 524 individuals meant a proportion of 8·1 twin individual candidates for every thousand candidates. The 628 individuals give a proportion of 9·8 per mille. The number of "lost" pairs is therefore much greater than among the country children. There is no reason to think that the death rate or the correlation co-efficient of death rates is vastly different from that found in the country area, and we must seek an alternative "cause".

6. *The Shortage Even Greater in the Town*

A suggestion is that this "cause" may be found as a result of differing abilities of twin children compared with other children. Information in part confirming this is given by some data relating to later stages of some of the examinations for the county borough. On account of the "bulge" in the child population and of the increasing popularity of the admission examination since the Education Act of 1944, the procedure for the examination had been slightly changed for the last three annual examinations that we are reviewing. Instead of a one-stage examination there was a two-stage one, the sequential procedure being that only the better fraction (about one-half) of the first set of candidates went forward to the second stage. In this second stage, over the period of three years, there were 19,103 candidates, and among them were only 151 individual members of pairs of twins (or sets of triplets), a figure of about 7·9 per mille, compared with 304 out of 33,561 in the full examination, or 9·1 per mille. The second stage seems to have definitely a smaller proportion of twins than does the first stage. For the separate years the figures are 6·2 compared with 8·0, 7·6 compared with 7·0, and 9·2 compared with 11·7: the second year appears anomalous —it is suggested that this is merely a sampling effect. The difference was often found to be due, when a case study was made, to the fact that in some cases one of a pair who qualified on Part I withdrew when it was found that the co-twin did not qualify; it was, in fact, to determine the proper policy in cases of this kind that some of this study was conducted.

As above indicated, when the "shortage" of this kind had been noticed repeatedly year after year, special steps were taken to try to guarantee that all cases were discovered. In the last year under review, the candidates from the city proper were noted. There were 202 individuals in the age-group ("field") of 18,377 or 11·0 per mille, a figure of the same order of magnitude as we had above for the candidates as a whole.

This case study feature suggested, however, that perhaps the different "survival" rates for twin individuals and for twin pairs, already referred to above, might be at work here. For a review of this we need some details about the performance of twins in the examinations. Let us therefore now turn to consider this.

7. *The Relative Ability of Twins and of Ordinary Children*

Mehrotra and Maxwell found that there was a difference of about five points of I.Q. (intelligence "quotient") between their twins and other children, the latter accomplishing the better performance. Such a difference is about one-third of the s.d. of the test used, so that it is clearly significant when their number of nearly a thousand individuals is considered.

The examination in both the areas for which the writer was responsible used, in nearly all cases, "unstandardized", "old type" question papers, which, as they were original, were not standardized for age so that we could not compare by "quotients". To effect a standardization we proceeded as follows. The s.d.'s of the mark distributions of the two sexes indicate that the girls' marks are less widely spread than are those of the boys, a feature noted elsewhere but which we could not assume applied to the county borough in view of the self selection effect in the entry. But for both areas the ratio of the s.d.'s of the marks of the two sexes is about ·98, so that the difference is not great. We therefore take, as a measure of the sex difference in marks, the actual mark difference, boys' and girls', and divide this by the s.d. of the boys' marks, so that a positive sign indicates that the boys do better than the girls.

In the county examinations, this standardized difference is $-\cdot 127$. For the city it is more nearly zero, ranging from about $-0\cdot 1$ for one of the earlier years to about $+0\cdot 1$ for one of the later years. The difference is due in part to self selection, in part to the effects that other workers have found evidenced between the sexes at this age, and probably also in part to other causes.

Let us now use this method for comparing the performance of twin children with ordinary children. Let us first consider the average marks, standardized as above.

In the country area the boys of like-sex pairs of twins average $-0\cdot 741$ below other boy candidates, and of unlike-sex pairs $-0\cdot 705$. Girls of like-sex pairs average $-0\cdot 633$ below other girls and for unlike-sex pairs $-0\cdot 529$. It seems definite that the twins do less well than do the other children.

For the county borough, we have pointed out that there is self selection in the matter of entry, and that there is an unduly small number of identified twin pairs entering. The individuals of twin pairs that have entered, however, still do less well than do the ordinary children. The differences for boys is $-0\cdot 251$ for like-sex boys and $-0\cdot 182$ for unlike-sex boys: for the girls they are $-0\cdot 218$ for like-sex girls and $-0\cdot 134$ for unlike sex girls. For the last three years, in Stage 2, the differences were as follows:

Like-Sex:	Boys,	$-0\cdot 244$
Like-Sex:	Girls,	$-0\cdot 517$
Unlike-Sex:	Boys,	$-0\cdot 199$
Unlike-Sex:	Girls,	$-0\cdot 689$

If we compare the country and the town figures we can perhaps conclude that the bigger the proportion of the twins sitting the examination the more the average mark of the twins is depressed—the extra twins are dredged up more from the bottom of the barrel.

Looking at these figures together, we can say that they confirm generally the results of Mehrotra and Maxwell, though the figures for the separate groups differ and our twins seem on the whole to be more markedly weaker than the average child than were the twins of the Scottish Mental Survey. When there is self selection for the candidature, this fact seems to be recognized by the twins concerned, for they enter in smaller proportions.

We note also that, in practically every case, the unlike-sex twins do better, in the case of each sex, than do like-sex twins. Two suggestions can be submitted at this stage to deal with this observation. It may be that, as elsewhere, the unlike-sex twins are more vigorous than the like-sex twins, and that this greater vigour exhibits itself in intellectual ways as well as in more physical ways. Alternatively, it may be that the shortage that we have found in the numbers of unlike-sex twins is due to unidentified pairs of this kind who are, on the whole, of weaker academic capacity than the rest of the unlike-sex twins, so that, even if the like-sex twins and the unlike-sex twins, as complete groups, were of equivalent merit, the absence of the weaker unlike-sex twins would raise the average performance of the identified ones above that of the like-sex twins.

It may be noted that there is found to be no "enhancement effect" such as has been reported by Beatty in the matter of birthweight.

8. *Correlations within Pairs of Twins for Examination Marks*

Figures such as those just given suggest that the self selection that results from the weaker children not choosing to sit for the examination as freely as do other children would lead to some, at least, of the greater shortage found for the numbers of city children when compared with those of the country.

But, as we have seen, a difference between the survival rates for individuals and for the twin pairs depends, in part, on the correlation within the pairs in respect of such survival rates. It is necessary therefore for us to look at the correlations between the various types of twin children.

We get the following results (the value of r for each type over its several examinations has been averaged by using the z transformation).

TABLE 4

Correlation within Pairs of Twins of Examination Performance

Type of Examination	Like-Sex Boys	Like-Sex Girls	Unlike-Sex Boys and Girls
County	·876	·851	·618
City	·760	·831	·440
City, 2nd Stage . . .	·427	·761	—·307

The correlations in the last two cases are of groups for which there has been selection based on the measures on which the correlations have been computed, so that the latter are reduced in consequence. The first line is on unselected material. The values found there can be compared with those of Mehrotra and Maxwell. Theirs range from ·69 to ·88 for like-sex boys, from ·63 to ·86 for like-sex girls, and from ·50 to ·63 for unlike-sex pairs. Our figures are on the whole slightly higher. It may be noted that there is one negative value in the table: even this may be due to using the basis of selection as that on which the correlations have been calculated: the three values from which this average has been obtained are respectively + 0·029, − 0·689 and − 0·256.

Let us see if these correlation coefficients give us, when applied to the differential survival table, some indication of the reason for the differing proportions of twin pairs found in our various types of pairs.

9. *Effect of Correlation on Survival Rates of Pairs of Twins*

Let us consider the effect of using the first stage, for selection for the second stage, in the last three years of the city examination. The result of the first stage has been to "survive" about 50 per cent. of the first-stage candidates. Using the three correlation coefficients of the "city" line of the table we have, instead of 50 per cent. surviving pairs, only about 39 per cent. for the like-sex boys, about 41 per cent. for the like-sex girls, and about 32 per cent. for the unlike-sex pairs. On all kinds of twin pairs the average would be about 35 per cent. instead of 50 per cent., so that our 9·1 per mille is easily reduced to the 7·9 that we find: with a higher correlation, the loss would be rather less than would reduce 50 per cent. to 35 per cent.

Turning back to the case of the general candidature of the city when compared with the country, it will be remembered that the latter gave about 16 per mille, the former for all eight years gave about 8·1 per mille. For the earlier years the city gave figures of 3·8, 6·1, 7·1, 8·0 and 10·0, when the proportion of candidates of the total field was at first about one-third but gradually increased over the years; for the last three years, when the candidature was about half the field, still increasing, it was 8·0, 7·0 and 11·7. Taking the correlation as about ·8 and the selection fraction as one-half, we have, from Table 3, that about 40 per cent. of pairs would survive, instead of 50 per cent. of individuals; when the fraction is one-third, the table gives about 25 per cent. of pairs instead of 33 per cent. of individuals. In both we lost about one-quarter of the cases, so that our 16 per mille is brought down to about 12 per mille.

If we allow for the lower average general level of the twins of, say, one third of an s.d., as found above, then we make the dichotomy for selection in every case at a point where the Pearson $h = k$ is about ·3 s.d. further along for the twins. Our last set of figures would then be, by interpolation in the table, about 27 per cent. in place of the 40 per cent. pairs, and 15 per cent. in place of 33 per cent. These results give, instead of 16 per mille, a figures of the order of 8 per mille and suggest that "lost" pairs in these cases can be fully explained by the effect of the differential selection rates. It is unfortunate that precise data are not available to enable us to make more precise determinations, but it seems clear that Mehrotra and Maxwell's conclusion that a "considerable number of twins have not been recorded as such" in their case is the result of many of the individuals having died, and of the survivors not having been reported as twins. In our cases the "shortage" we are now considering is due to the fact of differential death rates, and, in the city, to the fact that the selection has split the pairs, and the survivors have not been reported as such.

10. *Number of Twins in Grammar Schools*

Experienced grammar school teachers will be surprised when they read the figure of 16 per mille as that for the proportion of twins in a child population of secondary school age, for they do not see anything like this proportion in their classes. What is the explanation?

The work of the chief examiner finishes with the compilation of a ranked list of the various candidates—the later stages of admitting is an administrative matter depending on the parents, local policy, and so on. In the ordinary way, therefore, the chief examiner does not have any data about the number of twin pairs finally selected for the grammar schools. But in view of the interest of the question, the writer was able to obtain figures for the last two of the city examinations referred to above. The relevant figures are as follows:

Total candidature for Stage 2	14,251
Number of children admitted	5,759
Survival rate for ordinary children	·404
Twin Pairs, Like-Sex:	
Total number of pairs sitting	32
Number of pairs admitted	14
Survival rate of pairs: observed	·44
Twin Pairs, Unlike-Sex:	
Total number of pairs sitting	11
Number of pairs admitted	0
Survival rate of pairs: observed	0

For ordinary children selection was at about ·24 above the mean mark. The like-sex twin children were found to be about ·14 s.d. weaker than were ordinary children so that their selection was at about ·38 beyond the mean, corresponding to an individual survival rate of ·35. The average correlation coefficient for these like-sex children was about ·666, so that the survival rate for pairs of such twins is about ·22, giving as the expected number of pairs some 7 sets. We actually find more than this, 14.

Let us consider the unlike-sex cases. Here the mark deficit is about ·38 s.d. making the effective selection level at ·62 s.d., corresponding to an individual survival rate of ·27. The average correlation ratio is $-0{\cdot}442$. Extrapolating in Table 3 we find that the survival rate for pairs is less than ·03, so that we "expect" that, of the 11 pairs sitting, there will be none admitted. This is the case.

Why should there be such a discrepancy in the case of the like-sex twins? It may be a samplng effect. But there is an alternative suggestion which we should first consider. It is that the like-sex twins are not a homogeneous group. They consist of both fraternal and of identical twins. Let us then turn to consider this aspect of the problem.

11. *The Two Types of Twins*

The Registrar General reports that in 1954 there were born 2,539 like-sex pairs of boy twins, 2,479 like-sex pairs of girl twins, and 2,857 unlike-sex pairs. But though from the registration point of view there are these three kinds, yet genetically there are five kinds. These are the three kinds of fraternal twins (boy and boy, girl and girl, and boy and girl) from separately fertilized ova, and two kinds of identical twins (boy and boy, girl and girl) each from a divided single ovum. For the fraternal twins, bearing in mind that the sex ratio for all children at birth is roughly 1, the proportions expected of the three kinds are, using the traditional notation for proportionality, boy-boy: 1 : : boy and girl : 2 (boy-girl; 1, girl-boy; 1) : : and girl-girl : 1. For the identical twins the expected numbers of each of the two kinds are, for the same reason, approximately equal. Both these statements are only approximately true theoretically, as the detailed inter-uterine mechanisms may modify the first approximations. The proportion of identical and of fraternal twins cannot be derived on the same basis, but from the Registrar General's figures it seems that, approximately, the proportions are as follows: boy-boy (fraternal : 1 : : boy-boy (identical) : 1 : : girl-girl (fraternal) : 1 : : girl-girl (identical) : 1 : : boy-girl (necessarily fraternal) : 2.

Thus at birth about half the total of boy-boy twins are identical, and, similarly, about half the girl-girl twins are identical. As the determination of the genetic type is not to be done casually, there is little information about the relative numbers at any later stage after birth. But let us look at our figures for some evidence of the two genetic kinds.

The different values of the correlation within pairs for the like-sex and the unlike-sex twins indicate that the variance for the like-sex pairs is less than that for the unlike-sex pairs. We have found that there is little difference in either the averages or in the s.d.'s of the two sexes, so that the variance for the like-sex pairs seems to be built up of the variance due to the fraternal members of such pairs and the variance due to the identical members. This suggests a very small variance indeed for the latter type, whatever the proportion of the two kinds in the like-sex twins.

There is an alternative way of approaching the problem.

We can obtain a frequency distribution table for the difference of marks (standardized as above) within pairs of twins. Using the random sampling number procedure to designate each of a like-sex pair as either Castor or Pollux, we have distributions as in Table 5 for the pairs boy-boy, girl-girl, and boy-girl.

TABLE 5

Frequency Distribution Table for Difference of Standardized Marks within Pairs of Twins

Mark Diff.	*From*	− ∞	−1·50	−1·25	−1·0	−0·75	−0·50	−0·25	0	+0·25	+0·50	+0·75	+1·0	+1·25	+1·50	
	To	−1·5	−1·25	−1·0	−0·75	−0·50	−0·25	0	+0·25	+0·50	+0·75	+1·0	+1·25	+1·50	+ ∞	*Total*
Like-Sex	Boys .	3	0	6	6	10	20	28	18	14	10	3	4	3	3	129
	Girls .	5	3	1	8	13	29	22	32	16	7	5	2	2	2	147
Total .	.	8	10		14	23	49	50	50	30	17	8	6	5	5	276
Unlike-Sex	.	7	6	11	7	10	18	11	8	14	4	9	5	6	9	125
Contribution to χ^2 .	.	·1	2·9		7·0	—	—	—	—	—	—	·1	·2	·2	·4	10·9
Difference .	.	—	—		—	13	31	39	42	16	13	—	—	—	—	151

The distributions are somewhat asymmetrical. This is due, in the like-sex cases, to sampling effects, but it warns us that with such small total frequencies we shall find very large probable errors if we try to analyse either of the like-sex distributions into two normal distributions, one to represent the differences for the fraternal twins and those for the identical twins. But, even if we cannot get a precise analysis, we note that both the like-sex twin distributions are very sharply peaked (very leptokurtic), with a very great bulge in the middle of the distributions. The distributions could be made symmmetrical by reversing both frequencies, to get the difference each way instead of always Castor-Pollux, but as we do not propose to analyze in detail this hardly seems necessary. We shall proceed by an alternative method to obtain an approximation to the more precise analysis.

Let us look at the tails, and, for the employment of the χ^2 test, let us group into three arrays the data for differences less than − 0·75 s.d. and into four arrays the data for differences greater than + 0·75, for the distribution for the two like-sex cases together. Then, as shown in Table 5, we can apply the test to these seven groups and find that P is greater than ·10, so that the two tails, that for the like-sex pairs and that for the unlike-sex pairs, could well come from the same population. We take seven degrees of freedom, and not six, and by this means get some confirmation that the numbers of the fraternal twins in the two distributions (like-sex and unlike-sex) are the same. It is suggested therefore that the distributions for the like-sex twins can be regarded as together including that of samples of fraternal twins, the combined samples being equal in size to that of the unlike-sex twins, a fact which corresponds with the proportions suggested above as holding at birth. This leaves us with the differences between the two distributions, that for all like-sex twins and that for all unlike-sex twins, in the middle of the table. The numerical value of these differences in frequencies is shown in the last line of the table. If we regard this difference as due to the identical twin cases, we find that the s.d. of these middle six arrays is about 1·3 × ·25 times the original s.d. or, say, one-third of the original s.d. The frequency distribution for the fraternal twins gives, as the s.d. there, a value very nearly equal to that of the original s.d.

We can therefore expect the correlation between identical twins to be given by the relation (taking the correlation for fraternal twins as ·618)

$$\sqrt{(1 - r^2)} = \frac{1}{3}\sqrt{(1 - \cdot 618^2)}$$

This gives us a value of r, the correlation within pairs of identical twins, as about ·96.

This is very high: it is approximately that found for the correlation between any pair of long bones of the human body.

It may be noted that we can derive the value for the correlation within pairs of fraternal twins by an alternative route.

The s.d. of the mark difference within such pairs is about equal to the original s.d.

This mean deviation for such differences is therefore about ·8 times this s.d.

But for the original mark distribution, for which the s.d. has this value of 1, if we take samples of two from such a population we get an average difference of 1·13 times this s.d. (as can be seen from standard control chart tables).

We thus have that the correlation coefficient for pairs of unlike sex twins is given by

$$\sqrt{(1 - r^2)} = \cdot 8/1 \cdot 13$$

the solution of which is $r = \cdot 7$ (about).

Similarly we estimate the value of the correlation coefficient for the identical twins (using the value of ·25 s.d. given above) as about ·92.

12. *Application of the Identical Twin Data to our Observations*

The above analysis has overlooked any possible sex differences by assuming that the features appropriate to fraternal twins of unlike-sex are appropriate also to those of like-sex. It assumes that the difference between like-sex twins and unlike-sex twins is not to be explained as due to any superior vigour of the latter as such, as suggested earlier in this paper, but as due to the dilution of the like-sex cases by identical twins feebler, overall, than fraternal twins. Further, it cannot be applied, as it stands, to the problem that gave rise to this investigation, that of selecting for grammar school at the second stage of an examination, as the correlations are modified by previous selection. It is difficult to know how far, for example, the negative value of the correlation coefficient in the case for the unlike-sex twins can be taken as due to selection and not merely to sampling.

Suppose, however, that we make the following assumptions:

(i) about half the number of like-sex pairs are of identical twins, and half of fraternal twins,
(ii) the proportion of each of these two kinds of twins reaching the level of selection is well below that of ordinary children,
(iii) the correlation within pairs of identical twins is very high* and the correlation within pairs of fraternal twins is very low, even if not negative.

Then we should have that, of the 32 pairs of like-sex twins sitting, there are, say, 16 pairs of identical twins and 16 pairs of fraternal twins.

For the former, a survival rate of something less than ·404 (making with, say, $h = k = \cdot 15$ s.d. lower, the survival rate of individuals about ·35), with a high correlation would give a survival rate for pairs of about ·25, i.e. would give about 3 pairs.

For the latter, a survival rate of, say, the same ·35 for individuals and a low correlation would give us about ·1, say, as the survival rate for pairs, and thus we should find, say, 2 cases surviving. Thus we would get a total of about 5 pairs of like-sex twins surviving.

Actually there were 12 pairs: the previous treatment gave as our expected number the result of 7.

The result could not be expected to be precisely either of the expected values, for, as pointed out above, a special policy was adopted about twins, by which, when it was thought from various indications that they might be identical, attempts were made that they should be treated alike in their secondary school allocation. But it is unlikely that the extra number who thus were sent to grammar schools would so far exceed the expected number.

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* If we use the z transformation for r and assume that the average of the identical twin value of r equally weighted with the fraternal twin value of r gives us the like-sex value of r, we find for the City, 2nd Stage, data of Table 4, that the identical twin value of r is ·97.

Let us suppose the number of like-sex pairs assigned by examination mark alone to be 10, the balance of four being assigned after other considerations have been given weight. Then, if the expected number is 7, as we first found, a table for the Poisson distribution indicates that the chance of 10 (or more) cases is about ·17, so that it is quite likely that the observed number should be found. If the expected number is 5, as we have just given, then the probability is only ·03, and our attempted analysis of the like-sex twins into fraternal and identical pairs has not helped to solve the problem.

The assumptions, however, have been many so that it does not follow that part of the analysis may not represent the actual state of affairs. A long experience with nominal rolls of grammar schools suggests that the 14 pairs admitted on the examination considered, out of a total admission of 5,749, or, say, one pair in every 400 children, is an exceptionally high proportion from the schoolmaster's point of view.

But as it implies a proportion of about 5 twin children, admitted as such in pairs, for every thousand children, it is a proportion far less than any others of those that we have dealt with above.

13. *Summary and Conclusions*

(1) Examination experiences relating to 360 individuals of pairs of country twins and 628 individuals of pairs of town twins, all of them of age about ten years, have been reviewed.

(2) Among ordinary children taking the examinations, boys were found usually to do less well than girls, even though, when they were free to enter for the examination, they sat in relatively smaller numbers.

(3) Twins did markedly less well than did ordinary children, having examination marks averaging some half to two-thirds of the s.d. of the mark distribution less than those of ordinary children.

(4) Among twins, the boys did less well than did the girls, and the like-sex twins less well than the unlike-sex twins of the same sex: the correlations between individuals in like-sex pairs are greater than those in unlike-sex pairs.

(5) A measure of the mark defect among twins is found to be somewhat greater than that reported by other workers: the correlation within pairs of twins in their examination performances is also found to be somewhat greater than that reported elsewhere.

(6) A summary table is given to show how the survival rates for pairs of twins, as such, depend on the survival rates for the individual twins and on the correlation coefficients between the individuals of the pairs for the measure on which the survival is based, and the importance of the distinction between the two survival rates illustrated by several examples.

(7) An approximately linear relationship is found between the logarithms of these two survival rates for any one definite correlation coefficient. This property of data from Pearson's Table for the Volumes of Normal Bivariate Surfaces does not appear to have been noticed before.

(8) It is suggested that, at the age of ten, approximately half the surviving like-sex pairs of twins are identical, and that the correlation within pairs of such identical pairs of twins for their examination performance is of the order of ·96.

(9) Although the Registrar General reports that, at birth, about 24 twin children are born for every thousand ordinary children, yet death by the age of ten has reduced the number of children surviving as members of twin pairs to some 16 per mille.

(10) Self selection for examination candidature works, in the case of twin children who are, as stated, generally weaker than ordinary children and within pairs of which there is a correlation about their academic attainments, to reduce the number of twin children candidates known to be members of surviving twin pairs to about 10 twin children per mille candidates for a grammar school allocation examination in a particular city.

(11) A sequential procedure of a two-stage examination brings the proportion of twin children, members of pairs, down to about 8 per mille.

(12) The proportion of pairs of twin children found in grammar schools is considerably less than any of the above proportions.

(13) It is recognized that many of the conclusions are based on rather scanty material, but there does not seem to be any report on more extensive records.

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ALTERNATIVE PRESENTATIONS OF SOCIAL ACCOUNTING

By HARRY BURTON

THIS article consists of two sections dealing with two different, but related, ways of presenting systems of Keynesian social accounting (Burdett (1954), Burton (1956), Utting (1955)) so that the basic equations of the systems can readily be derived. Section I deals with the official system of the National Income and Expenditure Blue books, but modifies that system in the presentation of the capital and rest of the world accounts. Section II simplifies and re-casts the official statistics in a way which permits the derivation of equations approaching the simplicity of text-book accounts of Keynesian theory.

TABLE 1

Consolidated Social Accounts of the United Kingdom

Production Account

Receipts	1955 (£ *million*)	1956 (£ *million*)	*Payments*	1955 (£ *million*)	1956 (£ *million*)
1. (13) Consumption expenditure at market prices *(C)*	15,983	16,891	5. (9) Factor incomes (including residual error but net of stock appreciation and depreciation) *(F)*	15,658	16,703
2. (14) Subsidies *(Tn)*	346	369	6. (10) Taxes on expenditure *(Te)*	2,626	2,822
3. (21) Gross domestic capital formation *(Id)*	3,155	3,288	7. (19) Depreciation *(D)*	985	1,121
4. (27) Exports *(X)*	3,925	4,366	8. (23) Imports *(M)*	4,140	4,268
Total	23,409	24,914	Total	23,409	24,914

Appropriation Account

Receipts	1955	1956	*Payments*	1955	1956
9. (5) Factor incomes *(F)*	15,658	16,703	13. (1) Consumption expenditure *(C)*	15,983	16,891
10. (6) Taxes on expenditure *(Te)*	2,626	2,822	14. (2) Subsidies *(Tn)*	346	369
11. (28) Investment income from abroad *(Xi)*	628	670	15. (24) Transfers abroad *(Mt)*	84	78
12. (29) Transfers from abroad *(Xt)*	60	35	16. (18) Net saving *(Sd)*	2,091	2,400
			17. (25) Investment income paid abroad *(Mi)*	468	492
Total	18,972	20,230	Total	18,972	20,230

Capital Account

Receipts	1955	1956	*Payments*	1955	1956
18. (16) Net saving *(Sd)*	2,091	2,400	21. (3) Gross domestic capital formation *(Id)*	3,155	3,288
19. (7) Depreciation *(D)*	985	1,121	22. (26) Capital export (net lending abroad) *(If)*	—	233
Gross saving	3,076	3,521			
20. (30) Capital import *(Sf)*	79	—			
Total	3,155	3,521	Total	3,155	3,521

Rest of the World Account

Receipts	1955	1956	*Payments*	1955	1956
23. (8) U.K. imports *(M)*	4,140	4,268	27. (4) U.K. exports *(X)*	3,925	4,366
24. (15) Transfers from U.K. *(Mt)*	84	78	28. (11) Investment income to U.K. *(Xi)*	628	670
25. (17) Investment income from U.K. *(Mi)*	468	492	29. (12) Transfers to U.K. *(Xt)*	60	35
Total	4,692	4,838	Total	4,613	5,071
26. (22) U.K. capital export *(If)*	—	233	30. (2) U.K. capital import *(Sf)*	79	—
Total	4,692	5,071	Total	4,692	5,071

I

This section is concerned with presenting the social accounts of the United Kingdom for 1955 and 1956 (Table 1), and with showing how and why the treatment of the capital and rest of the world accounts differs from the official treatment. The argument involves a comparison of Tables 1 and 2, the latter being an adaptation in summary form of the official presentation of the capital and rest of the world accounts. The sources used are the National Income and Expenditure Blue Book (1956), *Preliminary Estimates of National Income and Expenditure* 1951–1956 (1957) and *National Income Statistics: Sources and Methods* (Central Statistical Office, 1956), hereafter referred to as *Sources and Methods*.

The Differences

1. *Depreciation.*—A minor difference of treatment concerns saving; this is a gross aggregate in the official statistics because of the lack of evidence on which to make reliable estimates of depreciation at current replacement cost. The prominent inclusion of depreciation in the present study is to enable a more complete presentation (on the lines of the national income statistics of the United States Survey of Current Business): it must not be taken as a rejection of the official warnings of the unsuitability of Blue book estimates of depreciation for practical purposes. It is to be hoped that, in the near future, the estimates of capital consumption, which now appear in the Blue books, will be integrated into the structure of the social accounts, bringing them nearer to the ideal system of pure theory.

TABLE 2

Capital Account

Receipts	1955 (£ *million*)	1956 (£ *million*)	*Payments*	1955 (£ *million*)	1956 (£ *million*)
1. Gross saving (including residual error) *(Sg)*	3,060	3,509	3. Gross domestic capital formation *(Id)*	3,155	3,288
2. (10) Grants from abroad *(Xct)*	16	12	4. (7) Net investment abroad *(If)*	−79	233
Total	3,076	3,521	Total	3,076	3,521

Rest of the World Account

Payments (*U.K. debits*)	1955	1956	*Receipts* (*U.K. credits*)	1955	1956
5. U.K. imports and investment income *(M+Mi)*	4,608	4,760	8. U.K. exports and investment income *(X+Xi)*	4,553	5,036
6. U.K. Transfers: *(Mt)*					
To persons	26	25	9. Transfers to U.K. *(Xt)*	44	23
To govts. and international organizations	58	53	10. (2) Other central government receipts *(Xct)*	16	12
7. (4) U.K. net investment *(If)*	−79	233			
Total	4,613	5,071	Total	4,613	5,071

2. *External transfers.*—The official treatment of external transfers (i) involves inconsistency of classification, (ii) exhibits inconsistency of accounting procedure in the rest of the world account, and (iii) distorts (though only slightly) the picture of the financing of investment expenditure.

(i) Oversea transactions may be divided into four main categories, (i) goods and services, (ii) investment income, (iii) transfers, (*a*) current and (*b*) capital, and (iv) capital. In the Balance of Payments White Papers items (iii*b*) and (iv) are dealt with in the capital account, and the remaining items in the current account. According to *Sources and Methods*, the distinction in the national accounts between current and capital grants follows the treatment of the Balance of Payments White Papers. This treatment "is based on the purpose of the grant in each case, as conceived by those responsible for policy, not on the objective characteristics or accounting features of the transaction". For instance, Defence Aid is treated as a current receipt because defence expenditure in the national accounts is a part of government current expenditure, but aid under the European

Recovery Programme is treated as a capital receipt of government because it is used to redeem debt. There is, however, inconsistency of application of this principle, since some of the component parts of two current items (12*a* and 12*b*) of the Balance of Payments White Papers become two items of capital receipts in the Central Government capital account. These are (*a*) "sale of surplus war stores held abroad" and (*b*) "net receipts from settlements, etc." These two items are aggregated into one capital receipt, "other Central Government receipts", in the rest of the world account. In *Sources and Methods* this item is explained as follows.

"This is a somewhat heterogeneous group of miscellaneous government receipts. It consists of item 12*b* in the White Papers *less* certain components which are sales of goods and services. A large part of this item, especially in the earlier years, consists of receipts in settlement of debt and claims arising from the last war. In addition, receipts under the 'Katz-Gaitskell' agreement (in item 12a in the White Papers), which provided that part of the United Kingdom gold and dollar payments to the European Payments Union should be refunded by the United States Government, are included here. In the Central Government accounts the whole item is treated as a transfer receipt on capital account (although it undoubtedly includes some current items)." (p. 345. For further discussion of this topic see also pages 199–200 and 206.)

This inconsistency of classification is evidently an act of conscious policy, but there appears to be no obvious need or justification for departing from the principle of consistent classification of items throughout the accounting system.

(ii) An item, once classified, should receive the same accounting treatment as other items of the same classification. This principle is also violated in terms of "other Central Government receipts" in the rest of the world account. Capital transfers from abroad consist of net capital grants from overseas governments and other Central Government receipts, and both appear with consistency as positive capital receipts in the combined Capital Account (Table 6 of the Blue Book and the Preliminary Estimates). But the two items receive inconsistent treatment in the rest of the world account; net capital grants from overseas governments appearing as a negative payment (U.K. debit) and other Central Government receipts appearing as a positive receipt (U.K. credit). From an accounting point of view the two items should appear on the same side and have the same sign. (A preference for the receipts side is stated below).

(iii) In an alternative approach it could be argued that the item other Central Government receipts is treated in the rest of the world account as a current item, which should, therefore, like current transfers from overseas governments, flow to the receipts side of the Central Government current account. With this treatment the item would be absorbed in the surplus on Central Government current account.

With the present official treatment a false impression is given of the domestic savings aggregate and the contribution to investment expenditures from external transfers. The official Combined Capital Account gives the impression that the full amount of other Central Government receipts (£16 million for 1955 and £12 million for 1956) is available for investment expenditure. This is misleading, since quite obviously the resources made available to the United Kingdom through external transfers can only have a value equal to the difference between all external transfers made and received. These values are £ −24 million (£60 million received—£84 million made) for 1955, and £ −43 million (£35 million received—£78 million made) for 1956. In effect, from an accounting point of view, a net transfer payment is the equivalent to an identical addition to expenditure abroad, reducing potential savings by the same amount and causing an equal increase in United Kingdom disinvestment or borrowing from abroad.

3. *The capital and rest of the world accounts.*—In Table 1, the rest of the world account is looked at from the point of view of the rest of the world in order to preserve uniformity of presentation (receipts on the left and payments on the right), and all negative items are removed to the other side of the accounts in order to secure positive flows. The latter change concerns net investment abroad and net capital grants from overseas governments. In the combined capital account of Table 1, net disinvestment abroad appears as borrowing from abroad to finance a part of gross investment expenditure, rather than as a reduction of gross investment expenditure as in Table 2. The capital account of Table 2 is, of course, both correct and meaningful, showing that gross domestic investment in 1955 of £3,155 million was only possible by disinvesting to the extent of £79 million, so that total investment of £3,076 million was equal to total domestic savings before

the provision for depreciation. But it is equally correct, and for some purposes more meaningful, to show as in Table 1, that gross domestic investment expenditure (and thus demand for resources) was £3,155 million and that this was financed by the sum of domestic savings, depreciation, and borrowing from abroad (the excess of payments due abroad over receipts due from abroad).

In the official rest of the world account the two main significant items are net investment abroad and total investment and financing, but the latter occupies the position of prime importance as the balancing item. Total investment and financing is equal to the balance of payments on current account and is, therefore, also equal to net investment abroad less net capital grants from overseas governments. In terms of 1951 (the last relevant year unless other Central Government receipts were treated as net capital grants from overseas governments), oversea transactions on current account produced a deficit of £403 million, hence disinvestment of this amount, but net capital grants from abroad of £43 million reduced actual disinvestment to £360 million. This presentation is, of course, informative; but a presentation is preferable which shows positive rather than negative flows, and which emphasizes net investment abroad as the significant balancing item. So modified, the rest of the world account, from the point of view of the rest of the world, becomes: Receipts of £4,457 million from U.K. imports, investment income and transfer payments, equal to the sum of Payments of £4,054 million for U.K. exports, investment income and transfer receipts, £43 million U.K. net capital grants receipt, and £360 million U.K. capital import.

II

The object of this section is to reduce to a minimum the complications due to oversea transactions in order to present the system for the open economy with a simplicity approaching that of the closed economy. The analysis is in terms of Table 3.

In the closed economy the familiar equations are: production, $Y = C + I$; appropriation, $Y = C + S$; and capital, $S = I$; when C stands for consumption, and Y, I and S stand for gross income, investment and saving. To obtain net income (Yn), net investment (In), and net saving (Sn), depreciation (D) must be deducted from each gross aggregate. With depreciation the basic equations are: production, $C + I = Yn + D$; appropriation, $Yn = C + Sn$; and capital, $Sn + D = I$.

The major difference of Table 3, compared with the official treatment, consists of following the U.S.A. practice of treating all oversea transactions as current items, and making the contribution of oversea transactions to the production account equal to net investment abroad. The M and X items of Table 1 are aggregated into items M and X of Table 3, so that M stands for all U.K. payments abroad other than loans, and X stands for all U.K. receipts from abroad other than loans. When $M > X$, the difference Sf supplements domestic saving and depreciation in the financing of gross domestic investment expenditure. When $X > M$, the difference If is U.K. net oversea investment financed by the excess domestic saving and depreciation over gross domestic capital formation. The X item is a single flow from the payments side of the rest of the world account to the receipts side of the production account, and the M item is a single flow from the payments side of the production account to the receipts side of the rest of the world account. With this procedure the contribution of oversea transactions to the production account is equal to net investment abroad (+ or −). (i) The oversea contribution to national income in the official treatment (e.g. Table 1 of the Blue books) is less than this by the amount of net transfers from abroad. (ii) For 1955 and 1956, net investment abroad is equal to the surplus or deficit on the balance of payments current account, because of the absence of net capital grants from overseas governments. On (i) above, the official treatment excludes net transfer receipts from the national income because it is "more appropriate to treat the national income as a measure of the goods and services becoming available to the nation through its own economic activity. This limitation allows the inclusion in national income of income from abroad, which is a measure of the yield of productive assets held abroad; it would not allow the inclusion of international transfers of which the value cannot be described as a measure of the receiving nation's economic activity". (*Sources and Methods*, pages 5 and 6.) This hardly seems an adequate justification of the inclusion of investment income. Investment income is a measure of productivity and economic activity abroad, not at home. More precisely, the amount of investment income depends on (i) the volume

TABLE 3

Simplified Consolidated Social Accounts of the United Kingdom

Production Account

Receipts	1955 (£ *million*)	1956 (£ *million*)	*Payments*	1955 (£ *million*)	1956 (£ *million*)
1. (8) Consumption expenditure at market prices (plus net transfers) . . (C) .	16,007	16,934	4. (7) Net national income at market prices . (Yn) .	18,098	19,334
2. (13) Gross domestic capital formation . (Id) .	3,155	3,288	5. (11) Depreciation (D) .	985	1,121
Gross domestic expenditure at market prices .	19,162	20,222	Gross national income at market prices . . .	19,083	20,455
3. (17) Exports plus income and transfers from abroad (X) .	4,613	5,071	6. (15) Imports plus income and transfers paid abroad (M) .	4,692	4,838
Total	23,775	25,293	Total	23,775	25,293

Appropriation Account

	1955	1956		1955	1956
7. (4) Net national income at market prices . (Yn) .	18,098	19,334	8. (1) Consumption expenditure at market prices (plus net transfers) . . (C) .	16,007	16,934
			9. (10) Net saving . (Sd) .	2,091	2,400
Total	18,098	19,334	Total	18,098	19,334

Capital Account

	1955	1956		1955	1956
10. (9) Net saving . (Sd) .	2,091	2,400	13. (2) Gross domestic capital formation . (Id) .	3,155	3,288
11. (5) Depreciation . (D) .	985	1,121	14. (16) Capital export (If) .	—	233
Gross saving . .	3,076	3,521			
12. (18) Capital import (Sf) .	79	—			
Total	3,155	3,521	Total	3,155	3,521

Rest of the World Account

	1955	1956		1955	1956
15. (6) U.K. imports plus income and transfers from U.K. (M) .	4,692	4,838	17. (3) U.K. exports plus income and transfers to U.K. (X) .	4,613	5,071
16. (14) U.K. capital export (If) .	—	233	18. (12) U.K. capital import (Sf) .	79	—
Total	4,692	5,071	Total	4,692	5,071

of assets owned abroad, which is a reflection of past (not present) economic activity at home, and (ii) the rate of yield on oversea assets, which is a reflection of economic activity abroad rather than at home. The treatment of Table 3, however, will result in the final aggregates of the production account failing to balance unless gross domestic expenditure is also adjusted for net transfers to or from abroad. As observed in Section I, net grants paid abroad in effect increase expenditure by an equal amount. On this basis consumption expenditure at market prices becomes £16,007 million (£15,983 million + £24 million) for 1955, and £16,934 million (£16,891 million + £43 million) for 1956.

From Table 3 the following equations are derived:

Production:

$$C + Id + X = Yn + D + M \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Appropriation:

$$Yn = C + Sd \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Rest of the world:

when $X > M$,

$$M + If = X \qquad (3)$$

and when $X < M$,

$$M = X + Sf \qquad (4)$$

Capital:

when $X > M$,

$$Sd + D = Id + If \qquad (5)$$

and when $X < M$,

$$Sd + D + Sf = Id \qquad (6)$$

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THE GENERAL FORM OF THE ADDING-UP CRITERION : A REJOINDER

By D. G. CHAMPERNOWNE

MR. J. L. NICHOLSON's statement of a "general" form of the adding-up criterion is that if

$$x_r' = a_r\varphi_1 + b_r\varphi_2 + c_r\varphi_3 \quad \ldots \quad + k_r\varphi_k \qquad (r = 1, 2 \quad \ldots \quad n)$$

$$X' = A\varphi_1 + B\varphi_2 + C\varphi_3 \quad \ldots \quad + K\varphi_k$$

are the regression equations of x_r (r = 1, 2 ... n) and $X = x_1 + x_2 \quad \ldots \quad x_n$ on independent variables $\varphi_1, \varphi_2 \quad \ldots \quad \varphi_k$ fitted so as to minimize the sums of squares of the deviations $(x_r' - x_r)^2$ and $(X' - X)^2$, then

$$\Sigma\, a_r = A_1\, \Sigma\, b_1 = B \quad \ldots \quad \Sigma\, k_r = K$$

and

$$\Sigma\, x_r' = X' \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

This is always undeniable, but there is a danger that the description of this result as an "adding-up" theorem may divert attention from the econometric problem to which the "adding-up" criterion should be relevant, and with which Mr. Worswick and I were concerned in the article to which Mr. Nicholson refers.

This problem is concerned with the case where the x_r represent expenditures on n individual commodities and X represents total expenditure ($X = x_1 + x_2 + \quad \ldots \quad x_n$ as above). Under what conditions will the total of the fitted regression functions $x_r' = a_r\varphi_1 + b_r\varphi_2 + \quad \ldots \quad + k_r\varphi_k$ $(r = 1, 2 \quad \ldots \quad n)$ exactly equal total expenditure?

The problem concerned not the identity

$$\Sigma\, x_r' = X' \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

but the identity

$$\sum_{r=1}^{n} x_r' = X \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Thus the condition $X \equiv X'$, which occurs in my own proof, far from being "a case of no practical consequence", as Mr. Nicholson puts it, is the kernel of the econometric adding-up problem.

The most general solution to *this* problem is given in my addendum to Mr. Worswick's article. "The necessary and sufficient condition that the least squares regression functions for expenditure in individual items should add up to the total expenditure exactly, is that some linear function of the independent variables should identically equal total expenditure."*

The proof there given was first to establish the identity (1) by the same algebraic argument as Nicholson later used independently, and then to observe that if, and only if, some linear function of the $\varphi_1 \quad \ldots \quad \varphi_k$ identically equalled X, would $X' \equiv X$ so that (1) would imply (2).

By a serious oversight the same symbol n was used in my note in referring to the number of individuals as had also been used for the number of commodities: in general the numbers are different, and this makes no difference to the proof. It is possible that this mistake may have misled Mr. Nicholson and put him on the wrong track concerning what it is that has to "add up". If so, I owe him a sincere apology.

Although my solution provides the most general form of the adding-up criterion, the "special case" proved in Mr. Worswick's note amounts for all practical purposes to the same thing: in referring to his result as a special case I was merely establishing that my proof led to his result. The purpose of including both his "rather lengthy" proof and my own as well was stated by Mr. Worswick: "I have left the above argument as it stands, because I believe that it may be somewhat easier to grasp by readers whose mathematical equipment is not large. Professor D. G. Champernowne has, however, put to me an alternative proof which is more succinct. . . ."

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* In the passage quoted the word income here appeared in place of expenditure. It was explained earlier that total income and total expenditure were to be taken as synonymous.

References

NICHOLSON, J. L. (1957), "The General form of the Adding-up Criterion", *J. R. Statist. Soc.* A, **120**, 84–85.

WORSWICK, G. D. N. & CHAMPERNOWNE, D. G. (1954), "Notes on the Adding-up Criterion", *Review of Economic Studies*, **22**, 57–60.

A REPLY—BY J. L. NICHOLSON

Mr. NICHOLSON replies as follows:

It is easy to say, once a theorem has been proved, that it is "always undeniable". Neither Professor Champernowne nor Mr. Worswick appears to have noticed that the proof of the theorem in form (2), to which they drew attention, had been given by me some time previously (*J. R. Statist Soc.*, 1949). The proof given by Worswick followed the same lines as mine and depended on the same condition—that total expenditure is one of the independent variables. Professor Champernowne added a further proof (which in fact involved less mathematical equipment than Worswick's) and pointed out that it was sufficient if total expenditure were identically equal to some linear function of the independent variables. Professor Champernowne admits that this slight extension of the theorem "amounts for all practical purposes to the same" as the previous version—which is why I thought it of no practical consequence.

The case where $\Sigma x_r' = X'$ is clearly more general than the case where $\Sigma x_r' = X' \equiv X$. I was led to the more general form by a consideration of a problem discussed by Barna (see *J. R. Statist. Soc.* A, **120**, p. 42), which is one possible application. Professor Champernowne gives no reason for thinking that only the less general form is relevant to econometric problems. To take the particular case with which he and Worswick were concerned: we are interested in obtaining regression equations expressing expenditure on each item and total expenditure as functions of one or more independent variables such as income. The fact that, if the same function is chosen for all items, the regression values for expenditure will add up to the regression value for total expenditure is surely of some interest. The regression equations are then mutually consistent. To insist that the regression values for total expenditure should exactly reproduce the actual figures of total expenditure would impose a much more severe restriction on the form of the regression functions. If the econometrician is only interested in functions which satisfy this condition, he is a poorer man than I thought.

REPORT OF THE COUNCIL

For the Financial Year *ended December* 31*st*, 1956, *and for the* Sessional Year *ended June* 19*th*, 1957, *presented at the* One Hundred and Twenty-third Annual General Meeting *of the* Royal Statistical Society, *held at the London School of Hygiene and Tropical Medicine, W.C.*1, *on June* 19*th*, 1957.

Number of Fellows

A further increase in the number of Fellows was recorded for the year, the total rising by December 31st, 1956, to 2,772. This represented a net gain of 153. During 1956, 254 Fellows were elected or restored to the roll, the highest number since 1947. Losses by death, resignation or default numbered 101. The end of the year total of ordinary Fellows included 120 representatives of corporate bodies.

Further elections in the early months of 1957 brought the total number of Fellows at the end of April to 2,800.

At the end of 1956 the Honorary Fellows numbered 31, of whom 7 were Presidents of other Societies.

Number of Fellows (excluding Honorary Fellows)

	Losses			
Calendar Year	*By Death*	*By Resignation or Default*	*Elected or Restored to the Roll*	*On the Roll at December 31st*
1945	18	11	177	1,417
1946	24	32	287	1,648
1947	14	61	263	1,836
1948	17	79	238	1,978
1949	16	87	189	2,064
1950	10	83	222	2,193
1951	21	93	228	2,307
1952	15	89	221	2,424
1953	10	91	165	2,488
1954	14	106	171	2,539
1955	19	83	182	2,619
1956	16	85	254	2,772

Losses by Death

The Council records with regret the loss by death during the year ended April 30th, 1957, of the undermentioned Fellows:

	Date of Election
Bernstein, Professor Felix (Honorary Fellow)	1954
Teixeira de Freitas, Mário Augusto (Honorary Fellow)	1954
Wagemann, Ernst Friedrich (Honorary Fellow)	1929
Adams, Arthur Avery, B.A.	1950
Beasley, Professor C. G.	1936
cp†*Bowley, Sir Arthur L., C.B.E., Sc.D., D.Litt.	1894
Dalal, Homi Pestonmi, A.S.A.A., A.C.I.S	1955
Hamilton, John Hyslop	1946
Perry of Stock Harvard, The Rt. Hon. Lord, K.B.E., LL.D.	1943
Rich, Cyril Douglas, M.A., F.I.A.	1952
Rothwell, Lionel Alan, F.S.A.A., F.I.M.T.A.	1937
Scott, Jesse	1940
Stevenson, Wing Cdr. J. A., A.S.A.A.	1950
Stone, Mrs. Feodora	1942
*Walton, Dudley William	1924
cp Wishart, John, M.A., D.Sc.	1929

* Life Fellow. *c* Served on Council. *p* Contributed to *Proceedings*. † Guy Medal.

Professor Felix Bernstein, who was elected an Honorary Fellow in 1954, was eminent in two fields, anatomy and mathematics. In the former he held positions in the Department of Anatomy of the Graduate School and the College of Dentistry, New York University, and was Professor Emeritus of the Department of Anatomy and the Graduate School of Arts and Sciences in that University. Earlier, he was Visiting Professor of the Department of Mathematics of Columbia University. It was for his contributions to mathematical statistics that he was chiefly known to statisticians and his articles in the *Annals of Mathematical Statistics* will be familiar to many.

Dr. Teixeira de Freitas was the acknowledged leader of statistics in South America and had many publications to his credit. His main work was in Brazil, where he was Director of the Statistical Service of the Ministry of Education and Public Health and also Secretary General of the Institute of Geography and Statistics. It was he who planned the organization of Brazilian official statistics during the 1930's. His interests were not confined to that country, however, and he had been Honorary President of the Inter American Statistical Institute. His election as Honorary Fellow took place in 1954.

Dr. E. F. Wagemann was an Honorary Fellow of long standing, having been elected in 1929. His election to Honorary Fellowship of the International Statistical Institute dated from three years earlier. Formerly Direktor des Instituts für Konjunkturforschung, Berlin, he wrote many books on Konjonktur. At the time of his death he was Director General de Instituto de Economia de la Universidad de Chile.

Mr. A. A. Adams, who died at the early age of 34, was elected a Fellow in 1950. Already he was well known for his research at the Department of Applied Economics in Cambridge, to which he brought both a high capacity for original thought and the most patient thoroughness in handling difficult statistical problems. For the last three years he had been statistician to the London and Cambridge Economic Service, overhauling the basic tables in the *Bulletin* and writing many articles on estimates of the national production in real terms.

Professor C. G. Beasley, a Fellow from 1936, worked chiefly overseas, first in Burma and later in the West Indies. Before going east in 1931, he had been Senior Lecturer in Geography at University College, Nottingham, and his first post in Rangoon was as Professor of Geography and Geology at the University there. Six years later he became Statistical Adviser to the Commerce Department of the Government of Burma and he continued to serve that Government both in Burma and later in India, until, in 1946, he was appointed Economic Adviser to the newly-formed Development and Welfare Organization for the West Indies. In that capacity and as permanent deputy Chairman of the Regional Economic Committee from its inception in 1951, he travelled continually from one colony to another, earning respect and affection throughout the Caribbean.

The Society sustained a grave loss by the death in January 1957 of Sir Arthur Bowley, one of its oldest and most distinguished Fellows. Elected a Fellow in 1894, he served on the Council for no less than six periods between 1898 and 1937. A Vice-President from 1907–09, and again 1913–14 and 1930–32, in 1938 he became President, holding that office till 1940. It is almost superfluous to write here of his fame as teacher and statistician; no Fellow can be ignorant of his celebrity or fail to recognize the influence he exerted. The Council would like specially to record its deep appreciation of the services he rendered to the Society throughout the many years of his Fellowship. The Guy Medal in Silver was conferred on him in 1895 for his paper on "Changes in Average Wages (nominal and real) in the United Kingdom between 1860 and 1891" which appeared in the *Journal* in that year. It was in 1935 that the Society paid him the highest honour in its power, the award of the Guy Medal in Gold. Less active perforce in later years, he had been a rare visitant to the Society's meetings since the War, but his interest and readiness to advise when approached with a problem remained strong.

Lord Perry of Stock Harvard, elected a Fellow in 1943, was an outstanding business man whose promotion of the motor trade, and in particular of the Ford organisation, made him widely known. His capacity for organization was also shown in his conversion of the Slough depôt of surplus mechanical transport into a flourishing industrial estate after the 1914–18 war. In both wars he freely gave his services in various government departments. His interest in statistics grew from his activities and he published several books on economic and financial subjects. Nor did the range of this lively and many-sided personality stop there—farming, literature and the arts all found in him a response.

Mrs. F. S. Stone became Secretary to the National Institute of Economic and Social Research in 1940, ensured its survival through the difficult war years and was able to see the Institute firmly established as a serious academic body with a large and growing volume of work. Becoming a Fellow of the Society in 1941 as representative of the Institute, her talent for administration led to her being invited to serve on a Committee set up by the Society to consider the constitutional problem of the method of electing its Council. In this Committee she played an active and constructive part and acted as rapporteur on behalf of a sub-committee consisting of the non-Council members. The present election arrangements are based on the Committee's recommendations and undoubtedly owe much to her.

Mr. D. W. Walton, whose election dated back to 1924, was one of the very early enthusiasts who sought to apply statistical methods to market research. A frequent attender at the Society's meetings as long as his health permitted, he was also a regular supporter of the Study Section in pre-war days.

Dr. J. Wishart was elected a Fellow in 1929 and from then onwards his interest in the Society never flagged. He served many times on the Council and was active in meetings of the Study Section during its early days. His professional life opened with his appointment as research assistant with Karl Pearson at the Galton Laboratory. In 1927 he moved to the Statistical Department at Rothamsted with Dr. (now Sir) Ronald Fisher. It was there that some of his most active research years were spent and where he derived the distribution, later given his name, which has formed the starting point of so much work in multivariate analysis. There too he shared in the pioneer work of testing out in the field the new and untried methods of analysis of variance. In 1931 he became Reader in Statistics at Cambridge and began his long struggle to get what he considered was sufficient recognition for statistics there. With the support of the Mathematics Faculty he was able to introduce mathematical undergraduates and biologists alike to statistical techniques and by 1948 a one-year Diploma course in mathematical statistics, initiated by him, became available. In 1953 he was appointed Director of a Statistical Laboratory under the Faculty of Mathematics. Among other distinctions, Wishart was a member of the International Statistical Institute, a Fellow of the Institute of Mathematical Statistics and an associate editor of *Biometrika*. One of the originators in 1933 of the Society's Industrial and Agricultural Research Section, in 1945 he became the first chairman of the newly-constituted Research Section.

Distinctions

The Council expressed its great pleasure on the election, during the past Session, to Honorary Membership of the Institute of Actuaries of the President, Professor E. S. Pearson, and of a past President, Professor A. Bradford Hill. This is a rare distinction—there had been only 13 such elections since the Institute was founded in 1848, the last having been made in 1894. It is of interest to recall that amongst those similarly honoured in the early years of the Institute were two distinguished Fellows of the Society whose names to-day continue to command respect, G. R. Porter and William Farr.

It was also with much pleasure that the Council conveyed its felicitations to Professor J. R. N. Stone on his election to Fellowship of the British Academy.

Honorary Fellowship

At the Ordinary Meeting on June 20th 1956, there were elected to Honorary Fellowship the following statisticians of international eminence :

Professor Oskar Anderson
Professor Ragnar Frisch
Professor Harold Hotelling
Professor Charles Jordan

Further consideration has been given to the Society's roll of Honorary Fellows and the Council has recommended for election four more statisticians of widespread recognition.

It was with great regret that the Council heard in December that Professor Charles Jordan, one of the Honorary Fellows, had lost the whole of his library and much of his personal property in the disturbances in Budapest in October. Professor Jordan's library of over 5,000 volumes contained a nearly complete collection of works on probability from 1496 to the present time and was one of the finest collections in Europe.

Vice-Presidents

For the Session 1956–57, the President appointed as Vice-Presidents of the Society Professor M. S. Bartlett, Professor D. G. Champernowne, Professor M. G. Kendall and Sir Robert Shone.

Meetings of the Society

The Board of Management and the Dean of the London School of Hygiene and Tropical Medicine have again placed at the disposal of the Society their lecture theatre for the Ordinary General Meetings and the meetings of the Research Section. As in former years, the Council gladly acknowledges its indebtedness for this privilege.

The following papers were read at the Ordinary General Meetings during the Session:

1956	
November 22nd	T. BARNA. The Replacement Cost of Fixed Assets in British Manufacturing Industry in 1955.
December 19th	M. S. BARTLETT. Measles Periodicity and Community Size.
1957	
January 16th	KATHLEEN GALES and M. G. KENDALL. An Inquiry concerning Interviewer Variability.
February 20th	G. HORSNELL. Economical Acceptance Sampling Schemes.
March 20th	M. J. FARRELL. The Measurement of Productive Efficiency.
April 17th	Discussion on the Use of Electronic Data Processing Equipment, opened by (1) J. H. H. MERRIMAN, (2) D. T. CAMINER, (3) H. L. BARMAN and D. EVANS, (4) H. DURANT.
May 15th	Joint Discussion with the Royal Meteorological Society on Analysis of Geophysical Time Series, opened by (1) J. M. CRADDOCK, (2) H. CHARNOCK, (3) J. NEUMANN and S. RUSHTON

The number of papers submitted for reading was rather less than in recent years. Fellows are reminded that offers of papers for reading are always welcome and that their likelihood of acceptance is increased if they are concisely written and sharply to the point.

The attendances at the meetings of the Society have ranged from 64 to 244.

The Research Section

Dr. H. E. Daniels was re-elected Chairman of the Research Section for the Session and Mr. D. V. Lindley was elected Honorary Secretary. Other members of the Committee were: Dr. P. Armitage, Mr. N. T. J. Bailey, Professor G. A. Barnard, Dr. N. L. Johnson, Dr. C. A. B. Smith, Mr. A. Stuart, Dr. S. Vajda, and Mr. A. M. Walker, with Professor M. S. Bartlett and Dr. G. E. P. Box appointed by the Council. During the Session Dr. Box resigned on going abroad and Dr. D. R. Cox was appointed in his stead. Dr. J. O. Irwin was an *ex officio* member of the Committee as Editor of the *Journal*, Series B.

The papers read during the Session have been as follows:

1956	
November 7th	Symposium on the Spectral Approach to Time Series: G. M. JENKINS and M. B. PRIESTLEY. The Spectral Analysis of Time Series. S. K. ZAREMBA and Z. A. LOMNICKI. On Estimating the Spectral Density Function of a Stochastic Process. P. WHITTLE. Curve and Periodogram Smoothing.
1957	
January 23rd	J. G. SKELLAM and L. R. SHENTON. Distributions Associated with Random Walk and Recurrent Events.
March 6th	J. GANI. Problems in the Probability Theory of Storage Systems. D. G. KENDALL. Some Problems in the Theory of Dams.
May 1st	F. YATES, M. J. R. HEALY and S. LIPTON. Routine Analysis of Replicated Experiments on an Electronic Computer.

The Committee of the Section has had several meetings, in addition to those normally held, at which the place of the Section in the Society has been considered. The Committee's report has been submitted to the Council and is now under consideration.

The Industrial Applications Section

The Section Committee which co-ordinates the functions of the separately organized Local Groups and links them with the Society has consisted of: (*a*) appointed by Council: Miss J. E. Keen and Professor E. S. Pearson; (*b*) nominated by the 1955–56 Section Committee: Mr. A. Stafford Beer, Mr. D. J. Page, Mr. R. B. Rowson, Mr. G. W. Sears, Mr. P. J. Stanley, Mr. A. W. Swan, Mr. A. F. Tout, Mr. M. Whyte; (*c*) representing Local Groups: Mr. M. A. Aczel, Mr. M. G. Dixon, Mr. D. A. Evans, Mr. W. D. Ewan, Mr. J. H. Hepburn, Mr. P. M. Hooper, Mr. D. A. Hopkins, Mr. A. N. James, Mr. W. N. Jessop, Mr. W. W. Jones, Mr. B. J. A. Martin, Mr. B. H. P. Rivett, Mr. J. P. L. Truesdale, Mr. C. S. Smith.

The Section Committee elected Mr. G. W. Sears Chairman for the Session. Mr. M. Whyte was re-elected Honorary Secretary.

During the Session a new Group was formed in Edinburgh, of which Mr. D. H. S. Forbes was appointed Honorary Secretary.

The Officers of the Local Groups were:

Group	*Chairman*	*Honorary Secretary*
Birmingham and District	Mr. B. J. A. Martin	Mr. W. N. Jessop
Edinburgh	Mr. J. Jones.	Mr. D. H. S. Forbes
Glasgow	Mr. J. H. Hepburn	Mr. W. R. Matheson
Leicester	Mr. D. S. Sirkin	Mr. P. M. Hooper
London	Mr. B. H. P. Rivett	Mr. A. N James
Merseyside	Mr. J. P. L Truesdale	Mr. N. Williams
North-Eastern	Mr. E. S. Page	Mr. D. J. Newell
Sheffield	Mr. D. A. Hopkins	Mr. M. A. Aczel
South Wales	Mr. W. D. Ewan	Mr. W. W. Jones

Group membership on December 31st, 1956 was as follows (comparative figures for 1955 are shown in brackets):

Group	*Fellows*	*Non-Fellows*	*Total*	*Average Attendance*
Birmingham	42 (42)	20 (21)	62 (63)	25 (25)
Edinburgh	6	17	23	20
Glasgow	8 (8)	28 (32)	36 (40)	15 (20)
Leicester	16 (10)	27 (31)	43 (41)	29 (18)
London	147 (135)	83 (74)	230 (209)	46 (50)
Merseyside	15 (17)	24 (27)	39 (44)	17 (17)
North-Eastern	36 (28)	6 (7)	42 (35)	22 (23)
Sheffield	35 (34)	34 (27)	69 (61)	21 (17)
South Wales	20 (20)	18 (11)	38 (31)	24 (21)
	325 (294)	257 (230)	582 (524)	

Fellows who are recorded as members of the Section are those who signify their wish to be attached to a particular group for the purpose of voting in its elections and being eligible to serve on its Committee.

The accounts of the Section and its Local Groups for the calendar year 1956, excluding expenditure by the Society in relation to the work of the Section, are summarized below:

Expenditure	£	*s.*	*d.*	£	*s.*	*d.*
Birmingham	23	8	5			
Edinburgh	7	7	10			
Glasgow	47	6	5			
Leicester	11	4	11			
London	26	18	10			
Merseyside	21	0	0			
North-Eastern	10	16	1			
Sheffield	20	17	7			
South Wales	13	17	3			
Section Committee	62	8	4			
				245	5	8
Carried forward to 1957				106	14	8
				£352	0	4

Income	£	*s.*	*d.*
Brought forward from 1955	115	0	4
Grants from Council	237	0	0
	£352	0	4

The total expenditure of £245 5*s*. 1*d*. in 1956 compares with £189 17*s*. 3*d*. in 1955.

The Section held a weekend Conference on "New Techniques and Applications of Statistics in Industry" at Florence Nightingale Hall, University of Nottingham, from July 20th to 23rd, 1956. The Conference was very successful and attendance totalled about 150. Dr. S. A. Wells, head of the Department of Industrial Economics, welcomed the Conference on behalf of the University and Professor E. S. Pearson, President of the Society, opened the proceedings. In his opening remarks the President stressed the need for improving communications between the mathematical statistician and his counterpart in industry. Many overseas visitors contributed to the discussions, among them Professor J. W. Tukey of Princeton University. The Conference concluded with a discussion of methods of teaching statistics within industry. Closely linked with this was a comprehensive exhibition of models, charts and other apparatus for use as teaching aids. The Conference closed on Monday with visits to the Boots Pharmaceutical Factories and to the Hosiery and Allied Trades Research Establishment.

Following the success of their Conference last year, the Birmingham Group organized a one-day Conference on "Statisical Methods in Stock Control" on May 8th, 1957. The Conference was well attended and again stressed the value of such Conferences as a means of stimulating interest in statistical methods in industry.

Fifty-eight meetings, listed below, were held by local Groups: 7 each in Birmingham, Glasgow, Liverpool and London; 6 each in Leicester, Newcastle-upon-Tyne and Sheffield; 3 in Cardiff, 2 in Newport, 1 in Crumlin and 6 in Edinburgh.

Birmingham and District Group

1956	
October 3rd	The Theory of Dams.—D. G. Kendall.
November 7th	Practical Difficulties in the Interpretation of the Analysis of Variance—E. A. G. Knowles.
December 5th	Practical Examples of Linear Programming—F. Benson.
1957	
January 11th	A Flexible System of Sampling Inspection—G. A. Barnard.
February 6th	Statistical Bases of a Study of the Effects of Television—W. A. Belson.
March 6th	The Application of Statistics in a Brewery—S. V. Cunliffe.
April 3rd	Systems of Stock Control—F. I. Musk.

Edinburgh Group

1956	
October 17th	Statistics in Industry—K. T. Boyd.
November 14th	Discussion on Statistical Methods in the Paper Trade—opened by I. F. Hendry.
December 12th	Validation of Personnel Procedure Selection—D. Macnahon.
1957	
February 22nd	(Joint Meeting with the Institute of Radio Engineers): Electronic Digital Manipulation and its Application to Statistics—J. Kyles. Statistical Evaluation of the Experimental Results of Field Trials—H. Holmes.
March 19th	Pricing Policy in Industry—L. C. Wright.
April 10th	Statistical Problems in a British Colony—W. Brass.

Glasgow Group

1956	
October 18th	The Control of Raw Materials Purchasing and the Optimum Use of Warehouse space—K. T. Boyd.
November 15th	Time Study and Statistics—A. W. Swan.
December 13th	The Statistics of Pass or Fail Tests—H. P. Stout.
1957	
January 24th	Labour Turnover—A. J. H. Morrell.
February 14th	Measuring the Development of Reliability in Machines—G. Morris.
March 21st	Population Problems of a New Town—W. R. Matheson.
April 25th	Discussion on Electronic Computers.

Leicester Group

1956	
October 4th	Market Research—D. H. Pickard.
November 1st	Stock Control—J. H. Hepburn.
December 6th	Statistical Calculations—A. Stafford Beer.

1957	
January 31st	The American Approach to Quality Control.—C. S. Smith.
February 28th	Electronics and the Automatic Office—J. C. Masters.
March 28th	A Review of Some New Techniques—C. H. Leigh-Dugmore.

London Group

1956	
October 5th	The Detection of Periodicities in Time Series: An application to Factory Sampling—R. C. Curnow.
November 2nd	The American Approach to Quality Control—C. S. Smith.
December 7th	Productivity Measurement at Plant Level—T. E. Easterfield.
1957	
February 1st	Experimental Design—M. J. Moroney.
March 1st	Elementary Games Theory—S. Vajda.
April 5th	Symposium on Electronic Computers: Economic Aspects—M. Hancock; Methodology—L. C. Payne; Recent Advances—S. L. H. Clarke.
May 3rd	Some Simple Experiments—D. R. Cox

Merseyside Group

1956	
October 2nd	Simplified Statistics—J. S. Ogilvie.
November 6th	Statistical Thinking in Industrial Control—A. Stafford Beer.
December 11th	Planning Information from Linear Programming—H. C. Mackenzie.
1957	
January 8th	Some Aspects of the Theory of Queues—H. F. Downton.
February 5th	The Latest Developments in Quality Control in the U.S.A.—C. S. Smith.
March 5th	A Statistical Approach to Stocktaking—E. S. Milner and D. Denman.
April 2nd	Marginal Cost—A. Muir.

North-Eastern Group

1956	
October 3rd	The Logic of Linear Programming—H. C. Mackenzie.
November 14th	Statistical Treatments in Advertising Research—N. J. Squirrell.
December 12th	Quality Control of a Continuous Process with Special Reference to Paper Manufacture—I. F. Hendry.
1957	
January 15th	Accidents and Road Layout—J. C. Tanner.
February 13th	Testing Taste and Smell—W. A. Pridmore.
March 13th	The Accuracy of Statistics Calculated from Non-Random Series, with Special Reference to Industrial Applications—G. H. Jowett.

Sheffield Group

1956	
October 25th	A Simplified Method of Regression Analysis—W. M. Gibson and G. H. Jowett.
November 29th	Linear Programming in Industry—M. A. Aczel.
1957	
January 31st	The Installation of a Quality Control System—G. H. Jowett.
February 28th	The Background to a Coal Sampling Specification—R. C. Tomlinson.
March 28th	Accident Control Charts—D. W. Widginton.
April 25th	Machine Translation and Other Uses of Electronic Computers—A. D. Booth.

South Wales Group

1956	
October 8th	Smoking and its Possible Effects on Lung Cancer—W. R. S. Doll.
November 12th	Microspores and the Identification of Coal Seams—R. C. Tomlinson.
December 10th	The Theory of Games—D. V. Lindley.
1957	
January 14th	Monte Carlo Methods and Industrial Problems—W. N. Jessop.
February 19th	Applications of Linear Programming—H. C. Mackenzie.
March 11th	Electronic Computers for Industrial Statistical Problems—D. G. Owen.

The Study Section

The Section Committee for the Session has been: Mr. C. A. Moser (Chairman), Miss S. V. Cunliffe (Honorary Secretary), Mr. B. P. Emmett, Mr. E. C. Fieller, Mr. H. W. G. Gearing, Mr. D. L. Lamberth, Mr. E. Shankleman, and Mr. L. T. Wilkins. The Council representatives were Mr. Moser and Mr. Fieller. Mr. G. M. Clarke represented the Bristol Group at Committee meetings.

Attendances at Study Section meetings in London have increased, the average for the Session having been 87.

The following papers were presented:

1956	
October 10th	The New Retail Price Index—R. G. D. Allen.
November 14th	Symposium on the Philosophy of Experimentation—A. Bradford Hill, M. J. R. Healy, and M. G. Kendall.
December 12th	Measuring the Effects of Advertising—D. H. Pickard.
1957	
January 9th	Economic Prospects for 1957—J. Ryan (in the Chair), P. Bareau, W. B. Reddaway, and H. Rose.
February 13th	The Statistical Aspects of Census Taking—B. Benjamin.
March 21st	Can the Statistician Help the Investor?—P. W. Freeman.
April 24th	The Pattern of Consumer Purchases—A. S. C. Ehrenberg.
May 8th	A Discussion on Some Problems of Statistical Inference—G. A. Barnard, M. G. Kendall and B. L. Welch.
June 12th	The Effect of Television on Children—H. T. Himmelweit and A. N. Oppenheim.

The Study Section and the Industrial Applications Section have again held their meetings at the Electric Lamp Manufacturers' Association Lighting Bureau, and the Council expresses its thanks to the Association for so kindly providing this accommodation.

A successful Conference on Economic Statistics was held at Queen's College, Cambridge, over the weekend July 6th–8th, 1956. Mr. W. B. Reddaway took the Chair at the Conference Sessions, at which the following papers were read:

Future Developments in National Income Statistics—C. T. Saunders.
Input Output Analysis—I. G. Stewart.
The Future of Production Statistics.—A. A. Adams.
Surveys of Investment Intentions and Other Surveys of Business Intentions and Expectations—J. C. R. Dow.
Retail Trade Statistics—J. Stafford.
What the Business Economist Requires from Short Term Economic Statistics—R. Brech.

Over 150 people attended the Conference. Mr. Campion was in the Chair at the Dinner on the Friday evening.

The Committee of the Bristol Group for the Session has been: Mr. G. M. Clarke (Chairman), Miss M. E. Holgate (Honorary Secretary), Mr. C. R. Hobbs, Mr. R. J. Pounce, and Mr. R. Savage.

Six meetings have been held, and the following papers read and discussed:

1956	
October 18th	The New Index of Retail Prices—H. S. Booker.
November 15th	How Statistics can help Investigations in the Factory—L. H. C. Tippett.
December 13th	The T.A.M. Service—P. Paine and A. Carden.
1957	
January 17th	Statistical Methods in Road Accident Research—F. Garwood.
February 21st	Some Uses of Economic Statistics in Industry—R. Savage.
March 21st	National Income Studies in the Colonies—A. R. Prest.

Attendances (ranging from 13 to 37) have averaged 20.

The Council makes grateful acknowledgment to the University of Bristol for providing a room in which to hold the meetings and for the service of tea.

The Medical Section

The Committee of the Section during the Session has been: Dr. D. D. Reid (Chairman), Mr. B. B. Bonner (Honorary Secretary), Dr. W. R. S. Doll, Dr. W. Hartston, Mr. J. A. Heady, and Mr. S. Rosenbaum, with Mr. M. P. Curwen and Dr. W. P. D. Logan as Council representatives.

The following meetings were held at which attendances have ranged from 24 to 39 :

1956	
October 30th	An Association between Smoking and Respiratory Tuberculosis—C. R. Lowe.
November 27th	The Aetiology of Malignant Disease in Children—D. Hewitt.
December 4th	Medical Statistics at the National Institutes of Health, U.S.A.—H. F. Dorn.
1957	
January 29th	Statistical Problems of the National Coal Board Medical Service—F. D. K. Liddell.
February 26th	The Provision of Hospital Beds—D. J. Newell and A. T. Elder.
March 26th	Arterial Blood Pressure in Population Samples—P. D. Oldham.
April 30th	Studying the Ravages of Time—R. G. Carpenter.
May 28th	Some Aspects of Cigarette Poisoning—J. H. Edwards.

The Council again expresses its thanks to the Dean of the Westminster Medical School for providing accommodation for the meetings.

The Section held a successful one-day Conference at County Hall, London, on September 28th, 1956. Professor Bradford Hill gave the opening address, and the following papers were read and discussed:

The Use of Statistics in Clinical Obstetrics—Professor Dugald Baird.
Genetical Effects of Radiation in Human Populations—Professor L. S. Penrose.
Statistical Studies of Cancer in Relation to Environment—Dr. Percy Stocks.
Ionising Radiations and Human Leukaemia—Dr. Michael Court Brown.

A film was shown on " X-raying a Complete Community: The Vale of Glamorgan Survey ".

The Conference was attended by about 140 people. The Council expresses its gratitude to the Medical Officer of Health of the London County Council for granting the necessary facilities for the Conference to be held at County Hall.

Award of the Guy Medal in Silver

The Council has much pleasure in awarding the Guy Medal in Silver to Dr. H. E. Daniels, with particular reference to his contributions to the *Journal*, Series B. His early papers were the beginning of an extended series which can be conveniently grouped under the title of " Components of Variance Analysis''. Dr. Daniels has taken a very active part in the affairs of the Society both as a member of Council and as Chairman of the Research Section. The Council is glad of this opportunity to acknowledge his services to the Society.

The Society's Examinations

Twenty-five candidates for all or part of the Certificate and two for the Diploma presented themselves for the examinations held on March 20th, 21st and 22nd, 1957.

Certificates have been awarded to the following Fellows:

Bamfield, Clifford
Brown, William Alexander
Coggins, Josephine Elizabeth
Caudwell, Maude
Davies, Hilda Margaret
Dick, Sylvia
Munden, John Major
Outlaw, John Eric
Smith, Diana Margaret
Wyatt, Audrey Florence

The Council again records its thanks to the London School of Hygiene and Tropical Medicine for allowing the examinations to be held in its Department of Medical Statistics and Epidemiology, to those Fellows who acted as invigilators, and to the Examinations Committee and others who have helped the Committee in its work. The Committee has consisted of: Mr. J. Durbin (Chairman), Professor R. G. D. Allen, Mr. N. T. J. Bailey, Mr. B. Benjamin, Professor D. G. Champernowne,

Mr. W. J. Corlett, Dr. D. R. Cox, Dr. H. E. Daniels, Dr. N. L. Johnson, Miss R. J. Maurice, Mr. F. A. A. Menzler, Dr. P. G. Moore, Mr. H. D. Patterson, Dr. S. Rushton, and Mr. A. Stuart.

The Journals

Consideration was given during the Session to the prices charged to non-Fellows and subscribers for current volumes and for back issues of the Society's Journals. The price of current volumes of Series A had been fixed in 1946 at £3 and that of Series B in 1948 at 45*s*. The increase in printing costs alone necessitated reconsideration of these prices and it was decided that beginning with the volumes for 1957 the price of Series A should be increased to £4 and of Series B to £3. The prices of back issues were also carefully reviewed and for Series A the price of those volumes separately available up to 1945 (Vols. 1–108) was increased to £3 per volume, and of issues subsequent to 1945 (Vols. 109 and onwards) to £4 per volume. Similarly, volumes of Series B up to 1940–41 (Vols. 1–7) and from 1947 (Vols. 8 and onwards) were increased in price to £3 and £4 respectively.

In November the Council received the report of a special committee appointed to consider the printing and related aspects of the Journals. This Committee consisted of: Professor E. S. Pearson (Chairman), Mr. F. J. Anscombe, Dr. H. E. Daniels, Mr. E. C. Fieller, Mr. R. F. Fowler, and Dr. J. O. Irwin. It was decided that, from 1958, a rather larger type, wider spacing and a better quality paper should be used for both series A and B. It is hoped that these improvements will make the Journals easier to read and more attractive in appearance. It was also agreed that the Council should appoint as one of its standing Committees a Journal Committee to keep under review questions of stock renewal, prices for back and current numbers, and the size of issue of current volumes. Because of the annual growth in Fellowship and the steadily increasing number of subscribers, this last question is referred to the Council at least once a year; the print order in 1956 was 3,850 for Series A and 2,500 for Series B. As recently as five years ago, these numbers were 3,350 and 1,750 respectively.

The four parts of the *Journal* Series A for 1956 contained 508 pages, compared with 517 in 1955 and 516 in 1954. There still appears some evidence to suggest that authors are reluctant to reduce their papers to a minimum although in a number of instances the Editors gladly acknowledge the response to their appeals for brevity. If the *Journal* is to be kept at a reasonable size, long papers can be accepted only at the expense of other material submitted for publication.

Professor Kendall reports that with the publication in May of the second volume of the series of *The Sources and Nature of the Statistics of the United Kingdom* the project which he has sponsored for the past nine years has been completed. The price of this second volume is thirty shillings and the price of the first volume was raised to this figure on the appearance of Volume II. The Council again records its appreciation of the tenacity and care with which Professor Kendall has guided this operation through to its conclusion. The Council is also very appreciative of the interest taken by Messrs. Oliver and Boyd in the enterprise. The publication of these two volumes is not a highly attractive undertaking from the commercial point of view and the publishers are to be congratulated on their generous attitude throughout the negotiations leading to the issue of the two collections of essays.

The two parts of the *Journal* Series B for 1956 contained 304 pages; there were 280 pages in 1955 and 301 in 1954. There is an increasing demand for space in Series B owing to the continually growing interest in new techniques and their applications. As it is clearly desirable that there should be no arrears in publication when the new journal form is introduced in 1958, the Council has authorised an increase in size for the 1957 volume which will contain about 350 pages.

Mr. Norman Bailey and Dr. D. R. Cox have been appointed Associate Editors of Series B, while Dr. J. O. Irwin continues to act as before. The Editorial Panel has been as follows: Dr. J. O. Irwin (Editor), Mr. Norman Bailey and Dr. D. R. Cox (Associate Editors), Mr. F. J. Anscombe, Dr. P. Armitage, Professor G. A. Barnard, Professor M. S. Bartlett, Dr. H. E. Daniels, Dr. O. L. Davies, Mr. E. C. Fieller, Mr. J. M. Hammersley, Mr. D. G. Kendall, Professor M. G. Kendall, Mr. D. V. Lindley, Dr. R. L. Plackett, Dr. C. A. B. Smith, Professor J. R. N. Stone, Dr. K. D. Tocher, Dr. S. Vajda, and Dr. F. Yates.

Applied Statistics

At its meeting in July, 1956, the Council accepted with great regret Mr. Tippett's resignation from the joint Editorship of *Applied Statistics*. The Council is glad to acknowledge its indebtedness to Mr. Tippett who has done much to ensure the success of that journal. It is largely because of his capacity to present technical material to general readers that *Applied Statistics* has achieved an established place in statistical literature. On his resignation, the Council appointed Mr. B. T. Ramm as Assistant Editor to Dr. D. G. Beech.

During the year, a system of representation of Sections on the *Applied Statistics* Committee, with some changes every year, was adopted, and in consequence Mr. C. A. Moser ceased to be a member. In addition to the Editors the present members of the Committee are: Mr. A. Stafford Beer, Miss S. V. Cunliffe, Dr. O. L. Davies, Dr. W. R. S. Doll, Mr. R. F. Fowler, Dr. J. O. Irwin, Dr. G. H. Jowett, Dr. C. Nuttall, Mr. P. D. Oldham, Mr. E. D. van Rest, Mr. E. Shankleman, Mr. L. H. C. Tippett and Mr. L. T. Wilkins.

The volume of *Applied Statistics* for 1956 contained 216 pages. Dr. Beech reports that a steady supply of suitable articles has slightly exceeded the space available and some have consequently had to be held over. The subject matter has been well varied and the articles have ranged from the short note to the fairly lengthy survey.

It was decided during the year to increase the price of the journal from the beginning of 1957, and to permit a small increase in size if the supply of articles continued to warrant it.

The numbers of subscribers have been as follows:

	Fellows	*Non-Fellows*	*Total*
1954 . . .	462	923	1,385
1955 . . .	467	992	1,459
1956 . . .	508	1,035	1,543

The increase in the number of subscribers is welcome, particularly in the case of subscribing Fellows.

The Library

The regular work of the Library continued throughout the year without any outstanding event to report. As the following table shows, increasing use is being made by Fellows of the facilities provided and recovery from the inevitable decrease of usage occasioned by the removal to Bentinck Street now seems complete.

The table presents an analysis of requests made for books by Fellows during each of the years 1952–56 inclusive. The analysis does not include books consulted for reference.

	1952	1953	1954	1955	1956
Number of Fellows who made requests to borrow books	391	343	339	380	450
Number of applications made	1,423	1,319	1 032	1,370	1,744
Number of volumes borrowed	2,419	2,110	1,509	2,339	2,832
Number of volumes "on loan" (unavailable at time of request)	195	185	181	292	329
Number of volumes not possessed by the Society at time of request	241	174	137	266	333
Total number of volumes asked for	2,855	2,469	1,827	2,897	3,494
Non-serial works added to the Library	594	516	695	930	719

The Investments Policy Committee

During the Session the Council gave careful consideration to the nature of the Society's investments. At the end of 1955 the accumulated fund alone amounted to over £10,000 and, with other funds, the sums invested represented nearly £30,000. It had traditionally been the policy to confine the investments of the Society to trustee securities although none of the funds was in fact held in trust. Other organizations with investments of a similar character had in recent years widened the nature of their investment portfolio and the Council felt that the time was opportune to review the Society's investment policy. A committee consisting of Dr. D. Heron (Chairman), Professor R. G. D. Allen, Mr. F. A. A. Menzler and Mr. A. Stuart, was therefore appointed to advise the Council on the question. The Council accepted the recommendations of this committee as follows:

(*a*) That, as a general principle, out of the total funds of the Society, taken at book value, 35 per cent. be invested in preference shares and equities, 15 per cent. in debenture and loan stock and 50 per cent. in trustee securities.

(*b*) That the Council appoint a standing Committee to meet at least once annually after the close of each year to review the action taken over the year and to consider future policy.

(*c*) That the Council appoint two Fellows to advise the Honorary Treasurer on specific investments from time to time, the Honorary Treasurer and these two Fellows constituting an Investment Sub-Committee, authorized to act within the investment policy approved by the Council.

Finance

Abstracts of the Honorary Treasurer's Accounts, *viz.*, the Statement of Income and Expenditure for the year 1956 and the Balance Sheet as at 31st December, 1956, together with the Auditor's Report thereon are in Appendices B and C. A Schedule of the Society's Investments is in Appendix D.

Total Expenditure increased from £12,985 in 1955 to £16,362 in 1956. Although there were some small savings owing to the non-recurrence of certain special expenses in 1955, almost every other item increased. The main increase was in the Publication and Distribution Expenses of the Society's Journals which rose from £6,006 to £9,349. There were substantial increases in printing charges and the full effect of these has not yet been felt. Further, three parts, instead of two, of Series B were issued in the year, thus bringing Series B up to date for the first time since the war at an additional cost of £1,487, and four parts of Series B which had been sold out were reprinted at a cost of £868.

Total income rose from £14,357 to £15,257. Fellowship subscriptions showed a further increase of £333, to £6,856. Journal sales increased by £1,809 to £6,486 but £730 of this was due to the issue of the third part of Series B referred to above. The Grant from the Rockefeller Foundation, which amounted to £1,115 in 1955, was completed at the end of that year.

The result was that in place of a surplus of £1,372 in 1955 there was a deficit of £1,105 in 1956.

In the Balance Sheet, the Accumulated Fund of the Society met the deficit of the year but was increased by the Udny Yule Bequest of £2,600 and at £11,653 showed a net increase of £1,495.

Life Composition Fees of £233 received during the year were added to the Life Composition Fee Fund, and after transferring to the Income and Expenditure Account the fees of compounders who died during the year or were presumed to have died in previous years (£110) the Fund, which is maintained at the total of the Composition Fees received from Fellows still living, increased from £7,799 to £7,922.

Among the Assets, the Balance at Banks and Cash in hand, £6,558, included the Udny Yule Bequest of £2,600 which was temporarily placed on deposit at the end of the year.

The Council and Officers

During the Session, Dr. G. E. P. Box resigned from the Council on his departure for the United States and Dr. D. R. Cox was appointed to fill the vacancy.

Professor E. S. Pearson has completed two consecutive years of office as President and is not therefore eligible for re-election. The Council gratefully acknowledges his furtherance of the Society's interests and his very helpful guidance in the conduct of its affairs. As President for the forthcoming Session the Council has pleasure in nominating Mr. H. Campion.

In addition to the elected members, the Council includes the three most recent Past Presidents, who, being continuing Fellows, are willing to serve. With the election of a new President, Professor Bradford Hill accordingly retires from the Council, at any rate for the time being, and this event cannot be allowed to pass unnoticed. With the exception of the one Session 1936–37, Professor Bradford Hill has served on the Council continuously since 1931–32 and thus without a break for the past 20 years—first as an ordinary member and subsequently as Honorary Secretary, President and Past President. There can be few instances in the Society's history of such service. The value of his wise counsel has been felt throughout and the Council is glad to know that it can continue to rely on his interest and advice while he is not a member of it.

The ordinary members who retire at the close of the current Session are Mr. R. G. Glenday, Professor D. G. Champernowne, Professor G. A. Barnard, Dr. D. J. Finney, Mr. C. A. Moser, and Dr. P. Armitage. Thus, with the nomination of Mr. Campion as President, there are seven vacancies. Having observed the requirements of the Bye-Laws and having considered the suggestions put forward by Fellows, the Council recommended that the seven vacancies for ordinary members be filled by the election of Mr. N. T. J. Bailey, Mr. J. Durbin, Mr. W. N. Jessop, Mr. L. Moss, Dr. D. D. Reid, Mr. A. Stuart and Mr. H. Tetley. No alternative proposals were received and the Fellows named below will be announced at the Annual General Meeting on June 19th, 1957, as having been elected President, other officers and other members of Council for the Session 1957–58.

Those whose names are marked * have never served on the Council.

President

Mr. H. Campion, C.B., C.B.E.

Council

*N. T. J. Bailey.
M. S. Bartlett.
D. G. Beech.
D. R. Cox.
Stella V. Cunliffe.
M. P. Curwen.
H. E. Daniels.
*J. Durbin.
E. C. Fieller.
D. Heron.
*W. N. Jessop.
J. O. Irwin.
Joan E. Keen.
M. G. Kendall.
F. A. A. Menzler.
*L. Moss.
*D. D. Reid.
W. F. Searle.
G. W. Sears.
Sir Robert Shone.
C. A. B. Smith.
J. Stafford.
J. R. N. Stone.
*A. Stuart.
*H. Tetley.
L. H. C. Tippett.

Honorary Treasurer

R. F. Fowler.

Honorary Secretaries

R. F. George. Iris Douglas. B. Benjamin.

On behalf of the Council,

E. S. Pearson, *President*.

R. F. George
Iris Douglas
B. Benjamin
} *Honorary Secretaries*.

APPENDIX A

From June, 1956 to May, 1957 inclusive, the candidates named below were elected Fellows of the Society:—

Ablewhite, Victor Charles.
Ainscough, John Ralph.
Ahmed, Nazir.
Almond, Joyce.
Andrews, Alan Charles John.
Anwar, Pamela Nurallah.
Aram, Terence Stoneleigh.
Arnold, Jane Frances.
Avadhani, Tangirala Venkata.
Aylott, Ronald Frederick William.

Backett, Edward Maurice.
Baker, Frederick Harold.
Baker, Henry Moore.
Baker, Stanley Harold.
Bamberg, Ronald.
Banerjee, Kali Shankar.
Bannister, Peter James.
Barry, Harold Edward.
Barton, David Elliott.
Bates, Grace E.
Beaumont-Dark, Anthony Michael.
Beeston, Derek Thomas.
Bell, Fenwick Emerson.
Bell, Wilfred.
Benson, Denis Charles.
Benton, William John Fox.
Bergin, Patrick.
Berry, Norman Charles
Binet, Francis Emeric.
Birnbaum, Allan.
Bishop, Bernard Cyril.
Bonner, Frederick Ernest.
Bonnett, Ronald Llewellyn.
Bradley, Robert George.
Breny, Henri.
Briggs, Richard John.
Britt, Kenneth James.
Brown, Leslie.
Browne, Philip William Peter.
Bumstead, Ronald Salthouse.
Burbridge, Tony Reginald.

Callaway, Sidney.
Callus, Francis.
Campbell, John Colin.
Carter, Donald McGregor.
Carter, Richard Leston.
Caudwell, Maude.
Chadda, Rosham Lal.
Chambers, Michael Leonard.
Chambers, William James.
Chapman, John Stanley.
Chapman, William Horace.
Chew, Victor.
Christian, Norman Spencer.
Clarke, John Welham.
Clough, Keith Raymond.
Cohen, Sheila.
Coles, William Dennis.
Collings, Howard.
Cook, Derek Edward.
Cooper, John Withrington.
Cormack, Richard Melville.
Cornell, Merriss.
Cornwall, Colin John.
Coultate, John Henderson.
Crabbe, Reginald James Williams.
Critchley, Douglas.
Cropper, Peter John.
Cumming, Oliver Douglas.

Dada, Paul O. A.
Davies, David Henry.
Davison, Mary.
Deam, Robert Joseph.
Deans, Alexander David Thriepland.
Dodds, John Jeffrey.
Doig, Alison Grant.
Dow, Robert.

Edwards, Derlwyn Howard.
Evans, Herbert John.
Evans, Iowerth Glyn.

Farlie, Dennis John Gerald.
Fenn, Cecil Geoffrey.
Firmin, Victor Edward.
Fowling, John Richard.
Francis, Peter Raphael.
Fraser, Donald Alexander Stuart.
Frimpong-Ansah, Jonathan Herbert.

Gardner, Alan.
Giddings, Geoffrey Donald.
Gilchrist, Joseph Bertold.
Gilley, Dennis Frank.
Graham, Anila.
Graneek, Jacob Jack.
Grassau, Erika.
Gray, William George.
Gregory, Ernest.
Gurland, John.
Guaschi, Francis Eugene.
Gunewardena, Olga Elaine Barbara.

Hall, Walter Bruce.
Hamilton-Jones, Joseph.
Hammon, Philip Horace.
Hardie, Thomas Ian.
Hazell, Robert Charles.
Hemingway, Dennis.
Henning, George Wallace.
Herbert, Carola.
Hewson, Joan Mary.
Hickox, Walter Ernest Hindmarsh.
Hill, Colin Barclay.
Hitching, Claude Herbert.
Hooker, Percy Francis.
Hutchings, Kenneth Richard.

Jaafar, Mohamed Ali.
John, Edward Francis.
Jotikabukkana, Supan.

Kabunda, Kironde.

Kay, Peter.
Keir, Ronald Smart.
Kenny, Peter Brian.
Kemp, Kenneth Walter.
Kerridge, David Frank.
Kessler, Sidney.
Kilpatrick, Samuel James.
Kraus, Hanus.

Latham, Francis Roy.
Lawrence, Douglas James.
Leaver, Rex Anthony.
Lepere, Jean M.
Lillistone, Janet Elizabeth.
Linnell, John Hyde.
Long, Eric Selby.

McIntosh, Peter Thomas.
Mack, Cornelius.
Mackowiecki, Zbigniew Konstanty.
Mann, Stella Elaine.
Marr, Charles Alexander.
Maynard, Christopher William.
Messenger, Henry William.
Moore, Brendan.
Morrison, Stuart Love.
Morton, Rosemary Janet.
Muir, Donald Stuart.
Munday, Arthur Alfred.

Nag, Himansu.
Naor, Pinhas Paul.
Newbery, Richard Geoffrey.

Oliver, Francis Richard.
Omaboe, Emmanuel.
Oyakhire, George Bennett Lazarus.

Paige, Wyndham Somerville.
Parakh, Kershi Dossabhoy.
Peake, Robert Ernest.
Penny, Kenneth Eifion.
Pereira, Peter.
Perry, Colin Albert.
Pettet, Kenneth Arthur John.
Plant, Norman.
Please, Neil William.
Pokora, Antoni Konrad.
Posener, Ludwig Nahman.
Prabhu, Narahari Umanath.
Pugh, Derek Salman.
Puri, Mansa Ram.

Rao, A. Gopal.
Read, Ewart Kingsley.
Rendell, David Edward.
Robarts, Basil.
Roberts, Wilbert Douglas.
Rogers, Alan Rowland.
Roseman, Nathan Stephen.

Sagar, Brian Frederick.
Sauer, Lothar.
Scullion, Henry James.
Shaljean, Jack Arshavir.
Shadrack, Ronold Ernest.
Shankar, Prasad Sheo.
Shears, Norman Havelock.
Shodeinde, Zainu.
Simmons, Martin.
Simons, Arthur Gribble.
Sinclair, Edward.
Smith, Diana Margaret.
Soul, Victor Frank.
Sparrow, John.
Spurrell, David John.
Stein, Bevan B.
Sundaresan, T. K.
Sutton, Rupert Leo.
Swan, Peter Harrison.
Symon, Evelyn Margaret MacDonald.

Taylor, John Kirby.
Taylor, Joseph Andrew George.
Teoh, Gim Hock.
Thomas, John Ethelbert.
Thomas, John William Ivor.
Thompson, Eric John.
Thompson, Glyn.
Thorpe, Harry Lingard.
Thursfield, Peter.
Tomlins, William Edward.
Tripathy, Sridhar.
Trustam, Charles Frederick.
Turner, Anthony McCallum.

Verity, Arthur James.
Vincent, Anthony Peyton.

Wales, Francis Herbert.
Walker, Peter.
Weatherall, Josephine Alice Coreen.
White, Colin.
Whitwell, Alan Walter.
Wilkinson, Eric Brian.
Williams, Leonard Ainslie.
Woodall, Arthur David.
Wyatt, Audrey Florence.
Wyatt, John Keith.

Corporate Representatives

Baines, Rita Mary, *representing* The Research Techniques Unit, London School of Economics.
Beswick, Kenneth, *representing* Henry Simon (Holdings) Ltd.
Glassborow, David Waters, *representing* the British Transport Commission.
Hepworth, George Anthony, *representing* Hepworth and Grandage Ltd.
Ross, Ian Gray, *representing* British Nylon Spinners Ltd.
Sando, Frank D., *representing* Albert E. Reed and Co.
Swann, John Roger, *representing* Production-Engineering Ltd.
Swift, Reginald Stanley, *representing* the Ministry of Pensions and National Insurance.
Wall, William Douglas, *representing* the National Foundation for Educational Research in England and Wales.

APPEN

STATEMENT OF INCOME AND EXPENDITURE

1955		**EXPENDITURE**	1956	
£	£		£	£
	101	Rents payable *less* rent received		109
	437	House expenses		491
	2,936	Salaries and wages (including contribution to staff superannuation scheme)		3,310
	250	Pension		250
	63	Insurance		14
	155	Office and Library equipment...		55
	123	Postage and telephone		173
	504	Stationery and miscellaneous printing		602
		Publication and distribution expenses:		
3,844		Journal, Series A (General) and reprints	4,579	
2,162		Journal, Series B (Methodological) and reprints	4,770	
6,006			9,349	
228		*Journal Index*	—	
253		*Fellows' List*	—	
	6,487			9,349
	—	*Applied Statistics*, loss for year		15
		Library:		
300		Books	390	
132		Binding	330	
	432			720
	181	General Meetings—ordinary and annual		182
	22	Council and committee travelling expenses		51
		Expenses of sections:		
40		Research	26	
222		Industrial Applications	243	
34		Study	48	
—		Medical	7	
	296			324
	55	*Guy Medals*		—
		Examination expenses:		
67		Printing, stationery, postage etc.	74	
93		Examiners' fees	98	
	160			172
	53	Auditor's fee (1955)		53
	28	Miscellaneous expenses		67
	425	Amortisation of leasehold premises		425
	277	*Cost of alterations to comply with fire regulations*		—
	12,985			
	1,372	*Surplus carried to Accumulated Fund*		—
	14,357			16,362
	242	Amount carried to Life Composition Fee Fund		233
	£14,599			£16,595

DIX B

FOR THE YEAR ENDED 31ST DECEMBER, 1956

1955 £	£	INCOME	1956 £	£
		Annual subscriptions:		
6,523		Fellowship	6,856	
12		Research Section	10	
93		Industrial Applications Section	87	
22		Study Section...	11	
—		Medical Section	6	
——	*6,650*		——	6,970
	50	Contribution from Royal Economic Society		50
	802	Dividends and interest (gross)		877
		Sales of Journal:		
2,646		Journal, Series A (General) and reprints...	3,371	
2,031		Journal, Series B (Methodological) and reprints	3,115	
——	*4,677*		——	6,486
	186	*Applied Statistics — Surplus for year*		—
	11	Sales of other publications		25
	180	Examination receipts: Fees and Sale of papers		237
	1,115	*Grant from Rockefeller Foundation — expenditure appropriated thereto*		—
	33	Surplus from Conferences		95
	89	Transfer from Life Composition Fee Fund, being fees of compounders known to have died during year and of those presumed to have died in previous years		110
	384	1950 Benefaction Fund—interest on investment		387
	180	*Refund of Schedule 'A' tax paid in previous years*		—
	—	Profit on sale of investment		20
	14,357			15,257
	—	Deficit carried to Accumulated Fund		1,105
	14,357			16,362
	242	Life Composition Fees received during year		233
	£14,599			£16,595

APPEN
BALANCE SHEET AT

FUNDS AND LIABILITIES

1955 £	£		1956 £	£
		Accumulated Fund:		
8,786		Balance at 31st December, 1955 …	10,158	
—		*Less:* Balance per Statement of Income and Expenditure …	1,105	
1,372		*Add: Balance per Statement of Income and Expenditure* …	—	
10,158			9,053	
—		*Add:* Bequest by Mr. Udny Yule …	2,600	
	10,158			11,653
		Life Composition Fee Fund:		
7,646		Balance at 31st December, 1955 …	7,799	
242		*Add:* Life Composition Fees received during year …	233	
7,888			8,032	
89		*Less:* Transfer to Statement of Income and Expenditure of fees of compounders known to have died during year, and of those presumed to have died in previous years …	110	
	7,799			7,922
		Building Fund (per contra):		
1,542		Balance at 31st December, 1955 …	1,600	
58		*Add:* Income for year …	61	
	1,600			1,661
		1950 Benefaction Fund (per contra):		
	30,971	Balance at 31st December, 1955 …		30,971
		Liabilities and Income held in suspense:		
3,190		Sundry Creditors …	5,061	
1,663		Amounts received in advance …	1,084	
	4,853			6,145
	55,381			58,352
		Frances Wood Memorial Fund (per contra):		
571		Balance at 31st December, 1955 …	594	
23		*Add:* Income for year …	23	
	594			617
	£55,975			£58,969

REPORT OF

I have examined the above balance sheet and annexed statement of income and expenditure
December, 1956, and of its income and expenditure for the year ended on that date according to

5, London Wall Buildings,
London, E.C.2.

24th May, 1957.

DIX C
31ST DECEMBER, 1956

ASSETS

1955 £	£		1956 £	£
	17,968	*Investments*, per schedule attached		17,503
		(Market Value, 31/12/55 £17,221, 31/12/56 £15,619)		
		Building Fund:		
	1,600	Investment, per schedule attached		1,661
		(Market Value, 31/12/55 £1,418, 31/12/56 £1,275)		
		1950 Benefaction Fund:		
		Leasehold Premises:		
21,368		At cost	21,368	
1,561		*Less:* Amortisation to 31st December, 1956	2,046	
19,807			19,322	
1,561		Sinking Fund Investment, per schedule attached	2,046	
		(Market Value, 31/12/55 £1,408, 31/12/56 £1,802)		
21,368			21,368	
9,603		Investment, per schedule attached	9,603	
		(Market Value, 31/12/55 £9,095, 31/12/56 £8,527)		
	30,971			30,971
		Current Assets:		
873		Debtors and amounts paid in advance	1,312	
100		Arrears of subscriptions recoverable (estimated)	100	
231		Interest accrued on investments (gross)	227	
—		Income tax recoverable	20	
3,638		Balances at banks and cash in hand	6,558	
	4,842			8,217
	55,381			58,352
		Frances Wood Memorial Fund:		
300		Investment, per schedule attached	300	
		(Market Value, 31/12/55 £377, 31/12/56 £351)		
294		Post Office Savings Bank Deposit	317	
	594			617

NOTE.—

No value is placed in the Accounts on—

(a) Journals and other publications in stock.

(b) Books in library.

(c) Pictures, furniture and equipment.

E. S. PEARSON, *President*.

R. F. FOWLER, *Honorary Treasurer*.

	£55,975			£58,969

THE AUDITOR
which, in my opinion, respectively give a true and fair view of the Society's affairs as at 31st the best of my information and the explanations given to me and as shown by the books.

W. G. DENSEM, *Chartered Accountant*.
(*Deloitte, Plender, Griffiths & Co.*)

APPENDIX D

SCHEDULE OF INVESTMENTS—31st DECEMBER, 1956

1955			*1956*		
Market Value £	*Book Value* £		*Nominal* £	*Book Value* £	*Market Value* £
5,948	*5,580*	2½% Consols (Guy Bequest)	10,528	5,580	5,527
1,264	*1,185*	2½% Consols	2,237	1,185	1,174
1,410	*1,324*	3½% Conversion Loan	1,865	1,324	1,298
381	*490*	3½% War Loan	500	490	351
2,328	*2,992*	3% Savings Bonds, 1965–75...	2,970	2,992	2,179
2,000	*2,000*	3% Defence Bonds	—	—	—
163	*100*	British Transport 3% Guaranteed Stock, 1978–88	195	100	133
3,727	*4,297*	British Electricity 3½% Guaranteed Stock, 1976-79	3,909	3,812	3,013
—	—	British Electricity 4¼% Guaranteed Stock, 1974-79	2,246	2,020	1,944
£17,221	*£17,968*			£17,503	£15,619
		Building Fund:			
£1,418	*£1,600*	3½% Conversion Loan	1,809	£1,661	£1,275
		1950 Benefaction Sinking Fund:			
£1,408	*£1,561*	British Electricity 3½% Guaranteed Stock, 1976–79	2,338	£2,046	£1,802
		1950 Benefaction Fund:			
£9,095	*£9,603*	British Electricity 3½% Guaranteed Stock, 1976–79	11,066	£9,603	£8,527
		Frances Wood Memorial Fund:			
£377	*£300*	British Transport 3% Guaranteed Stock, 1978–88	513	£300	£351

PROCEEDINGS OF THE ONE HUNDRED AND TWENTY-THIRD ANNUAL GENERAL MEETING OF THE ROYAL STATISTICAL SOCIETY held at the London School of Hygiene and Tropical Medicine on Wednesday, June 19th, 1957, at 5.30 p.m.

The Chair was taken by the President, Professor E. S. Pearson, C.B.E.

The President presented the Report of the Council for the financial year 1956 and the session 1956–57, and moved that it be received.

During his submission of the Report the President presented the Guy Medal in Silver to Dr. H. E. Daniels, and Certificates to the successful candidates in the 1957 examination who were able to be present at the meeting.

The Honorary Treasurer, Mr. R. F. Fowler, presented the accounts and seconded the motion that the Report be received.

On being put to the meeting the Report was formally received.

Dr. David Heron moved that Mr. W. G. Densem be elected Auditor of the Society for the session 1957–58, at a fee of fifty guineas; and that his appointment as Auditor by the Council during the current session on the retirement from that office of Sir Alan Rae Smith be confirmed.

The proposal was seconded by Professor M. G. Kendall and carried unanimously.

The President announced that as no alternative nominations had been received the President, Council and Officers for the session 1957–58, nominated as shown on the list already circulated, were duly elected.

The meeting then terminated.

The Annual General Meeting was preceded by an Ordinary Meeting at which the following statisticians were elected Honorary Fellows of the Society:

Dr. Gertrude M. Cox	United States.
Professor Carlos E. Dieulefait	Argentine.
Dr. Robert C. Geary	Eire.
Professor René Roy	France.

The candidates named below were elected Fellows of the Society:

Victor Lawrence Budge
George Walter Collett
John Maybin Henley
James Thomas Dennis Holt
Alfred Howarth
Fathi Mohamed Ibrahim
Eric Eustace Canram Lavallette
Albert Walter Morling
Robert George Morton
Vera Pearlman
Edwin Jacques Reinger
Zygmunt Antoni Sterminski
John James Thomas
Sam Wainwright
Louis Stuart White

Corporate Representative

Ailsa Horton Land, *representing* The Research Techniques Unit, London School of Economics.

REVIEWS OF STATISTICAL AND ECONOMIC BOOKS

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1.—*Nonparametric Methods in Statistics*. By D. A. S. Fraser. New York, Wiley, 1957. London, Chapman and Hall. x, 299 pp. 9″. 68*s.*

Anyone giving a course in advanced mathematical statistics is conscious of the lack of an adequate accompanying text. The standard books, of Cramér, Wilks and Kendall, for example, reflect the prewar development of the subject and do not, because of the time when they were written, include accounts of advances made during and after the Second World War. This period has been one in which mathematical statistics has flourished exceedingly. More powerful mathematical tools have been used to extend the theory; a unity has been introduced into the field by the decision function ideas of Wald, and the concepts of admissibility and of Bayes' solutions have come to the fore; the importance of sufficient statistics has been more thoroughly understood and the idea of invariance introduced explicitly. These are but a few of the developments that reflect the rapid progress and make the earlier books appear so dated. The omission of a modern text has been felt more in this country than the States. Over there the two excellent sets of lecture notes by E. Lehmann on "Theory of Testing Hypotheses" and "Theory of Estimation" have circulated, either in their mimeographed form, or through their teachers own adaptations of them in lectures, to most students. Consequently the modern attitude is familiar to them. In this country we have adopted a much more pragmatic (some might say practical) approach and are in danger of being left behind in the mathematical theory.

For these reasons the book under review should be of especial value to members of the Society who are interested in the mathematical side of our subject. In it they will see a subject which is recognizably statistics as they understand the term (in contrast, for example, to Blackwell and Girshick's treatment), and which is expounded using the post-war ideas. Certainly the most curious, not to say ridiculous, feature of the book is its title. 125 pages (being chapters one and two) are devoted to a general account of mathematical statistics without specific attention to nonparametric ideas. Furthermore the book is throughout concerned with theory: methods receive little or no attention. The first two words of the title are therefore misleading: a clumsier, but more adequate, title would be "The Theory of Mathematical Statistics, with special reference to Nonparametric Theory".

The first two chapters are obviously taken almost directly from the Lehmann notes already referred to. The treatment assumes a previous knowledge of statistics, and is rather too condensed even then. Examples of topics discussed are: measure theory, sufficiency and completeness, unbiased and invariant estimation, unbiased and invariant tests of hypotheses, confidence regions, tolerance regions. Some subjects, discussed in detail by Lehmann (e.g. tests of linear hypotheses) are relegated by Fraser to a large collection of problems. A brief chapter three outlines the sort of problems the author chooses to consider as nonparametric, and it is not until the fourth chapter that the book proper may be said to begin. The topics therein are the estimation of real parameters and tolerance regions. The first may look out of place, but what is meant is, for example, the estimation of a parameter such as the mean, without restriction of the class of distributions to a parametric class. A typical result (p. 144) is that the k statistics are the minimum-variance estimators of the cumulants in the class of unbiased estimators. This is presumably the reply Karl Pearson should have made to Fisher instead of the classic "Wasting your time fitting curves by moments, eh?": not that the modern reply is much better. Hypothesis testing is considered in the fifth chapter: the invariance theory is used to reduce several nonparametric problems to ones based on ranks. Chapter six opens with the statements (without proofs) of several theorems of probability, in particular limit theorems. These are extended and used to prove limit theorems about statistics introduced in the earlier chapters. The final chapter deals with large-sample properties of tests, such as consistency, relative efficiency and large sample efficiencies of some conditional and rank tests.

Your reviewer is not aware who was responsible for introducing the idea of restricting attention in a statistical problem to a class of distributions depending on a finite number of parameters, but it must surely rank as one of the most important ideas in statistics. In most problems the class of all distributions is far too wide and it seems natural to search, like Karl Pearson did, for a subfamily which would be adequate for practical applications. His set of curves was not satisfactory because he had not considered the ideas of sufficiency and efficiency. A possible family to consider is the generalized exponential family which, for all random samples, possess a k-dimensional sufficient statistic for n parameters. A theory confined to this family ought to be adequate for most applications. However that may be, the fact that the class of distributions is parametric introduces mathematical simplicity, and it is therefore surprising to find that the first textbook on the modern material should pay most attention to the more difficult nonparametric field. I would have thought that the mathematical development of nonparametric theory was not in a sufficiently advanced stable form for a satisfactory book to be written at the present time. This seems to be the main defect of Fraser's work: the book is unsatisfactory not because of the author's writing but because now is not the time to write a book in this specialized field. It is even doubtful if a textbook on advanced parametric theory could be written now. Statistics stands at a cross-roads; shall we follow the signpost "Bayes' theorem" and go with Jeffreys (the use of Bayes' solutions makes this more attractive than it did a few years ago), or shall we continue with "Significance levels" and confidence intervals. Or should we use both, are they tools for different situations? Until such questions are resolved it will be difficult, if not impossible, to write a text which will stand the test of time.

In points of detail Fraser's book suffers from a high number of misprints, some obscurities of expression, particularly in non-mathematical remarks such as those about random variables (p. 9) and average risk (p. 38), and some errors—for example a wrong statement of the Radon-Nikodym theorem on p. 13. But I would strongly recommend the book to the attention of mathematical statisticians in this country, despite these defects, because, especially in the first two chapters, it presents a development which will probably be strange to them but will certainly repay study. The use of this material in the rest of the book may not be worthy of the tools developed but the tools are important for all that.

D. V. LINDLEY.

2.—*The Lognormal Distribution with special reference to its uses in Economics*. By J. Aitchison and J. A. C. Brown. No. 5 in the monograph series of the Department of Applied Economics, University of Cambridge. Cambridge University Press, 1957. xviii, 176 pp. 9½". 35*s*.

The Department of Applied Economics at Cambridge has during the past decade made a series of valuable contributions to statistical analysis to which this latest monograph is a useful addition. The authors were led to their study of the lognormal distribution by their experience in handling economic data. In particular they found the probit transformation, a special application of lognormal theory, to be a powerful tool for the analysis of family budget data. A more familiar application is that by Gibrat and others to the study of income distributions. In fact many of the variables studied in economics have highly-skewed distributions of "the-higher-the-fewer" kind for the analysis of which the lognormal hypothesis appears to offer a firmer foundation than that of normality, on which much of standard statistical methodology is based. Examples are expenditure on a particular commodity, size of business enterprises, household size. It is scarcely necessary to add that applications of the distribution are by no means confined to economics. For this reason the authors have sought to give a general treatment and they list examples from the fields of small-particle statistics, sociology, biology, anthropology, ecology, physics, industry, astronomy, demography and philology. No doubt many others could be found.

In view of the wide applications of the distribution it is surprising that it has occupied such a small place in standard text-books and orthodox courses of statistics. In this sense the authors are justified in referring to it as the Cinderella of distributions. However the neglect certainly does does not arise from a lack of research into the properties of the distribution. Indeed the bibliography at the end of the monograph lists no less than 217 items most of which are concerned directly or indirectly with the distribution. Possibly it arises from an impression among statisticians at large that the lognormal distribution presents no essentially new problems which cannot be dealt with by making a simple transformation to normality. This misapprehension the authors are at some pains to disperse.

The body of the book starts with a clear account of the basic theory of lognormality and goes on to discuss a number of theoretical processes by which the distribution may be generated. Many of the results are of course simply derived in virtue of the fact that if x is lognormal, log x is normal. Thus the simplest type of generation process is that in which x is the product of a large number of independent random variables. Consequently log x is the sum of independent random variables and hence is normal by the Central Limit theorem. Thus x is lognormally distributed. There are many phenomena for which effects tend to be multiplicative rather than additive and hence give rise to distributions which are approximately lognormal. Personal income is an example.

After disposing of the theory of the ordinary two-parameter distribution, for which x ranges from zero to infinity, the authors consider three- and four-parameter families to deal with cases in which the lower, upper, or both ends of the range are terminated at unknown points. Further developments are given for truncated and censored distributions. Methods of estimation are developed for distributions of each type and these are compared empirically for a series of artificial samples derived by transformation from Wold's tables of random normal deviates. One of the drawbacks of handling lognormal data is the complexity of the equations required for efficient estimation of the parameters. For the present study the computations were performed on EDSAC and the authors take a good deal of trouble to emphasise the ways in which the development of electronic methods of computation lightens the arithmetical burden.

Perhaps the most interesting and novel part of the book is that dealing with the analysis of consumer's behaviour. Work in this field has traditionally been based on classical economic theory and linear regression analysis, both of which have frequently been found deficient for the purpose. The theory developed here is based on the concept of a tolerance distribution for a set of consumers akin to that postulated in probit analysis. Given a fixed-price situation it is supposed that each consumer decides what is the least income he must have before he will purchase, say, a television set. This income is called the tolerance income. Its distribution for a population of consumers is the tolerance distribution and is supposed to be lognormal. The parameters of the distribution can be estimated from family budget data and hence the effects of income-changes can be predicted. Extensions are given for commodities for which expenditure is permitted to vary continuously and for the effects of price-changes and variations in household composition.

The comprehensive account of the lognormal distribution given in this book will be valuable to all statisticians. Econometricians will find in addition suggestions for promising lines of further research.

J. DURBIN.

3.—*Probability: An Intermediate Text-book*. By M. T. L. Bizley. Cambridge University Press (for the Institute of Actuaries and the Faculty of Actuaries), 1957. viii, 230 pp. 8½". 20*s.*

The actuarial student, at an early stage in his studies, has to sit for an examination in "Probability" in which he is asked a number of questions, of varying degrees of difficulty, of what may be loosely termed the "coin, dice and playing card" variety. There has long been a need for a suitable text-book for students preparing for this examination; the Institute and Faculty of Actuaries therefore commissioned Mr. Bizley to prepare such a work and he has produced the present volume which, in addition to meeting the need to which it was directed, will be found of value by many who have no intention of embarking on an actuarial career.

It is important to make clear at the outset the limitations within which the author, by reason of the circumstances in which the book was written, has confined himself. The reader cannot expect to find a full discussion of the various theories of probability, though he will find a clear statement of the difficulties implicit in the classical or unitary definition which, in a somewhat modified form, the author has adopted for this book and he will find very brief descriptions of some other definitions and theories of probability. He will discover also that there is no attempt to extend the study of probability theory to cover the elements of statistics, since this ground is covered in a separate text-book in this series. The book is, in fact, a primer setting out, with the utmost clarity, the various methods that can be used in the solution of problems in probability and the circumstances in which each is likely to be appropriate. A number of these methods and the results derived from them do not appear to have been given in any previous text-book.

The book opens with a brief discussion of the meaning of probability to which reference has already been made. This leads up to what the author calls a "working definition of probability" in terms of the ratio of the number of the favourable sub-class to the number of the reference class and the first chapter concludes with examples which can be solved by direct calculation of these two numbers. The author here assumes that his reader already has quite an extensive knowledge of the theory of permutations and combinations. There follows a chapter on problems where more than one event (dependent or independent) is involved, the usual addition and multiplication rules being derived, and there is a much more complete treatment than is usual in this type of book of the requirements that must be met before a number of events can be said to form a completely independent set. Next, a proof is given of Waring's theorem concerning the probability that exactly t of r events happen and the theorem is extended to the probability that at least t events occur; several examples are given which demonstrate its great power. The author then turns to Bayes's theorem; he confines himself strictly to applications where the prior probabilities are precisely known, with only a passing reference to the vexed question of indifference rules.

The following chapter is almost certainly the best in the book; it deals first with repeated trials, including simple limited and unlimited games, and then goes on to consider expectations: the reviewer knows of no other work in which the varied methods of calculating an expected value are so well set out. The author then develops indirect methods, leading to difference equations, which can be employed when the direct approach breaks down; it does not seem to be well known that these indirect methods are also applicable to the calculation of expectations, but here their use for this purpose is fully brought out. The next chapter concerns the occurrence of various sequences in a series of repeated (Bernoulli) trials, and it is here in particular that several of the methods and applications are both novel and valuable. Lastly, there is a chapter on continuous variables. Both the methods of the integral calculus and geometrical methods are shown, as well as the treatment of certain continuous problems as limiting cases of analogous discrete problems.

The mathematical standard demanded of the reader is roughly that of the Advanced Level of the General Certificate of Education, though some of those who have reached that general standard might find it necessary to acquire rather more knowledge than they already possess of such subjects as permutations and combinations, distributions and difference equations. The standard of the book is thus nicely gauged to meet the needs of the mathematically-minded school-leaver who wishes to acquire a knowledge of the less advanced aspects of probability as a first step in a course of statistical or actuarial studies.

Though by no means all the subject matter is easy, Mr. Bizley has taken great care throughout to make his exposition as simple as possible. His style is clear and direct and the argument is never hard to follow; he avoids the mistake of proceeding by such large strides that the student has to stop to fill in the intervening steps. In addition, every principle introduced, every theorem proved and every method proposed is at once followed by an illustrative example which helps to clarify the point at issue. The happy choice of these examples and their detailed working out is perhaps the best feature of the book.

Mr. Bizley's text-book can thus be heartily recommended to anyone commencing the study of this subject, which so many find to be full of difficulties and pitfalls. The student who has mastered this work will have acquired both a thorough grounding in the principles of probability and a knowledge of how best to attempt the solution of a variety of practical problems.

LESLIE V. MARTIN.

4.—*Random Processes in Automatic Control.* By J. H. Laning and R. H. Battin. New York and London, McGraw-Hill, 1956. ix, 434 pp. 9″. 75*s*.

This book is the outcome of a series of lectures that were given at the Massachusetts Institute of Technology with the object of providing the basic knowledge necessary for the application of random processes to complex instrument systems. The first third of the book deals with the basic notions of probability and random processes. Binomial, Poisson and Normal distributions are described followed by the properties of the distributions of different types of random process. Having dealt with these basic topics in sufficient detail for anyone with a reasonable mathematical equipment to be able to follow, the book goes on to consider some particular random processes in detail. This portion starts with the Gaussian random process and the time invariant control systems. Special attention is given to the case where there are one or more stationary random processes as input including an investigation of the correlation and spectral densities that relate the inputs and outputs. Next the book turns to non-stationary systems. Non-stationary systems arise both when the responses of the system vary with time and when the input itself is of a non-stationary form. Solutions to such complicated systems are less elegant mathematically than for stationary forms and the approach is to find the response that occurs for a prescribed linear system rather than to attempt the breakdown of the final system. Following this there is a short chapter dealing with optimum stationary processes and this leads on to the last chapter which deals with the question of limited data. The treatment here is not entirely rigorous and there are some gaps as yet unfilled, but without some discussion of the optimum operations that can be expected with a finite set of data it is difficult to see how far a system under study differs from the optimum obtainable.

There are a number of appendices that deal mainly with purely mathematical points that have occurred in the text such as inversion of matrices and types of orthogonal functions. These are followed by a good bibliography which is divided up by chapters. There are quite a number of interesting examples scattered throughout the text and a few problems for the reader. The standard of printing is high and the book seems remarkably free from misprints. It can safely be recommended to the mathematician, probabilist or engineer who wishes to learn something about the applications of random processes to the theory of complex instrument systems.

P. G. MOORE.

5.—*Contributions on Partially Balanced Incomplete Block Designs with Two Associate Classes.* By W. H. Clatworthy. National Bureau of Standards, Washington, 1956. iv, 70 pp. 10¼″. 45*c*. (Appl. Math. Ser. 47).

The design and analysis of incomplete block experiments has tended to become the specialist concern of the Indian School of Statistics and this volume is a welcome indication that the study of these designs is now spreading beyond the bounds of that country. The partially balanced incomplete block designs invented by Bose and Nair give, in those cases where exact equality of precision of treatment estimates is not possible, a design in which only a limited number of precisions are involved. Of all the designs which might have this property the PBIB designs are the only ones in which the simplicity of the variance structure is maintained under an interblock analysis (i.e. using the technique of recovering all information from block totals).

In studying this volume one alternates between admiration at the perseverance and ingenuity shown in developing these designs, and a suspicion that the intrinsic interest in their development has swept the author along far past the point where the designs have any practical application.

Even if the design is restricted to the simplest possible case of two associate classes, which mean there are at most three distinct variances, the problem of a complete enumeration of all possible designs has not yet been solved. Clatworthy starts his analysis by determining the sets of algebraic restraints of PBIB designs and he shows for certain cases how most of the algebraically possible cases exist combinatorily. This is in marked contrast to the balanced incomplete block designs.

He then considers group divisible designs (which are designs in which variance structure is defined as v_1 say for any treatments in the same group and v_2 ($\neq v_1$) for any treatments in different groups) that can be constructed by augmenting three complete block designs in independent treatments. He describes an alternative method of generating group divisible designs by noticing that the complement of a group divisible design is group divisible, that a set of independent balanced incomplete block designs is a degenerate group divisible design and taking the complement of this.

The next paper describes methods of design using the principle of duality (i.e. interchanging treatments and blocks) and shows how the duals of replicated designs can give rise to further designs.

The types of PBIB can be classified according to an array called the association scheme. The cells of this array are the treatments, and the association between any two treatments is of the first kind if they lie in the same row or same column and otherwise are of the second kind. The association scheme can be extended by classifying cells as first associates if in addition they lie in the same orthogonal set defined by a Latin square or a Graeco-Latin square. Clatworthy considers various special association schemes such as the triangular.

The volume finishes with a very large set of tables of various designs which are not included in the very comprehensive list of designs given by Bose, Clatworthy and Shrikhande.

K. D. TOCHER.

6.—*Proceedings of the First International Congress on the Application of the Theory of Probability in Telephone Engineering and Administration* 1955. (Vol. I No. 1 of *Teleteknik*) Copenhagen: 1957. 130 pp. 11¾″.

This volume contains some introductory matter on the genesis of the congress, a list of participants and twenty-six contributed papers. Of the latter, twelve are summaries only. The first (and longest) paper is historical—*The Beginnings of Switching Theory in the United States* (by R. J. Wilkinson)—and is followed by another historical paper on European developments in the application of the theory of probability in telephone traffic engineering, by L. Kosten.

The scientific papers are concerned, for the most part ,with the application of statistical methods to problems of telephone traffic, following the lines developed by Erlang and Palm. The problems arising are similar to those occurring in the recently developed "queueing theory", but the papers presented do not bring out this similarity. They are, in general, more concerned with specific technical problems than with general methodological questions.

The most substantial of the papers are the following:

(i) *Probabilités de perte en selection conjuguée* (By R. Fortet and B. Cancelli). This considers a situation in which calls from a number of different exchanges are arriving at a common switchboard. Assuming each sequence of calls to be of fixed lengths, with initial instants which are distributed as samples from independent Poisson processes, various limiting distributions (e.g. of the number of occupied positions on the switchboard) are obtained.

(ii) *Statistical Equilibrium Equations for Overflow Systems* (By N. I. Bech). An outline of a general method, with no numerical results shown.

(iii) *A Survey of Traffic-Measuring Methods in the Copenhagen Telephone Exchanges* (By E. Brockmeyer). This paper is rather less specialized than most of the others, and contains an interesting account of methods of measuring duration of call, congestion time, holding times, etc. The use of statistical theory in determining adequate sample sizes is particularly emphasized.

(iv) *On a Special Use of the Erlang Methods in Industry* (By K. Rander Buch). The methods developed by Erlang and Palm are applied to a special case of the problem of "idle time" for a number of machines serviced by a single worker.

(v) *Ein Analysator für den Fernsprechverkehrs* (By S. A. Karlsson). An interesting paper describing the construction of an electrical analogue machine for investigating telephone traffic problems by (effectively) Monte Carlo methods.

(vi) *Über die Bedeutung des Stichprobenverfahrens beim Messen des Fernsprechverkehrs* (By K. Kohde). On the application of sampling methods, and especially of sequential sampling, to telephone traffic problems.

(vii) *The Applicability of Decision Theory in the Planning and Operation of Telephone Plant* (By A. Jensen). This paper is very general in nature, but may well be found of interest as an ingenious example of application of the ideas of decision theory in a fresh field.

N. L. JOHNSON.

7.—*Teach Yourself Statistics.* By Richard Goodman. London, English Universities Press, 1957. 239 pp. 7″. 7*s.* 6*d.*

Statistics is the latest branch of knowledge to join the new formidable and diverse list of subjects covered by the "Teach Yourself" series. Like companion volumes, it is produced in a style that will be familiar to most readers.

Although small in size (there are 239 pages), the author has managed to survey a wide range of topics. Chapters are devoted to frequencies and probabilities; statistical models-binomial, poisson and normal; bivariate distributions; sampling; analysis of variance; regressions and correlation and the chi-square test. The aim of the book is to help those who have to teach themselves some statistics to an understanding of some of the fundamental ideas and mathematics involved. This it does; and it is no criticism to say that the reader would need to be equipped with a working knowledge of calculus and algebra.

The text is liberally interspersed with worked samples which help to bring home the points and method. Except in the first chapter, which is introductory, each chapter is followed by a series of exercises (with answers) partly taken from past examination papers of the Royal Statistical Society, the Association of Incorporated Statisticians and the London University.

Students of statistics will find this book a useful introduction to the subject. It presents the general basic principles in a terse and understandable form that leads naturally to more advanced and specialized study.

A. BARR.

8.—*National Income Statistics: Sources and Methods*, Central Statistical Office. London, H.M. Stationery Office, 1956. viii, 387 pp. 9½″. 25*s.*

The days when Bowley, Stamp or Colin Clark could publish national income estimates, in a slim volume, together with a very satisfactory explanation of the concepts, sources and methods used, are gone. Since the war, when the government assumed responsibility for annual estimates of national income and expenditure, the subject has grown in complexity both at home and abroad.

We now have international conventions on the conceptual framework and even an international association of specialists. To-day the annual blue book giving the figures alone contains 72 pages.

The volume under review has the straightforward task of explaining how the official statisticians produced the estimates given in the blue books and, formerly, the white papers on national income; the main conceptual framework, which is the subject of numerous text-books and learned articles, and which is confirmed by the expert recommendations of United Nations and the O.E.E.C., is taken for granted. In the past few years, with additions to and changes in the well-known pre-war sources and methods, the general user of the national income estimates has tended to become less sure of his ground, although the main sources have always been obvious. This volume must be welcomed without reservation as one which fills a long-felt want.

To estimate the national income is an art as well as a science, which by its nature cannot be reduced to administrative routine. There was never any danger, at any rate in this country, that this would be attempted. The band of government statisticians who were recruited by Keynes in 1940, and their successors, continued and maintained the academic tradition of the pioneers. This is evident from the present volume which gives the frankest explanation of official estimates to be found in any country (with the possible exception of Sweden). It is a pity that the names of those who built up the estimates—well-known to experts all over the world—are not mentioned and acknowledged.

In general the authors give due warning on margins of error but they appear over-sanguine about the realiability of the estimates of personal savings. There are also points on which we would like to have fuller information and more details of calculation. An instance is the lack of adequate discussion of the consequences of tax evasion and avoidance on the estimates. It is admitted (p. 96) "that some understatement of income occurs in tax returns is borne out by the figures published by the Inland Revenue on taxes and penalties charged in cases that have come to light. There must obviously be other cases so far undetected and the figures published in the Blue Book include an arbitrary addition on this account". How much is added, is it the same for each year, and what basis is there for an estimate? Nor are we told how business expenditure on consumer goods is estimated. The Central Statistical Office thinks that its estimate for spirits and wines (5 per cent. of total consumption) "is probably too small" (p. 112). For tobacco no estimate is given.

As these instances illustrate, the Central Statistical Office does not always give an adequate explanation in terms of primary sources when another department is responsible for the estimates, even when such an explanation is not published elsewhere. On occasion, when it is stated that some calculation cannot be done (as for instance on p. 142, *n*.1. or p. 287, *l*.26), the primary data for making the necessary estimates do exist but they have not been analyzed by some other department.

In places the exposition has been made difficult by the authors' apparent lack of certainty as to whether they should explain the current estimates or the whole series since 1940. This difficulty might have been avoided by a separate publication, giving the definitive estimates and the explanation of sources and methods for the war and early post-war years, in continuation of the series of monographs edited by Professor Stone which cover the pre-war period.

A volume on this scale is naturally the work of several hands. In a revised version certain inconsistencies in tone and emphasis could perhaps be eliminated and careful editing could make the book shorter without loss of information. References to published work do not appear to be systematic and in a work of this nature terms like "now", "at present", and "more recent" should be avoided—especially if there is a considerable lapse of time between writing and publication.

However, in spite of these criticisms, which are directed at relatively minor issues, the main requirements of a work of reference are fulfilled. One may perhaps end with a suggestion concerning the future of the estimates. With its growth in size and complexity, the subject of national income estimates is to-day tackled in a number of specialized compartments. We are not given a comprehensive picture of the statistical independence of the elements that make up the estimates (and indeed in places administrative consistency appears to be preferred to independence of estimation). The national income estimates could provide a framework, as they do for instance in Norway, for statistical co-ordination and for showing the fields in which progress in sources and methods is most urgently required, and an advisory committee might help the official statisticians to review the system and to suggest further improvements.

T. BARNA.

9.—*The Pattern of Financial Asset Ownership: Wisconsin Individuals*, 1949. By Thomas R. Atkinson. Princeton, University Press, 1956. London: Oxford University Press. xviii, 176 pp. 9″. 30*s*.

Information about the forms of personal savings and the factors that influence their choice is so restricted, in America no less than in Britain, that any new study is likely to be of interest. This one, as the title of the book indicates, is limited very much in scope for it is concerned only with a single American state and with a single year. Its interest, therefore, is not so much in the figures that emerge as in the techniques used and whether these offer encouraging prospects for further work along the same lines.

The basis of this whole study, sponsored jointly by the University of Wisconsin and the National Bureau of Economic Research, is a sample of personal state income tax returns filed in Wisconsin in 1949. These returns show the reported income from certain financial assets, but noticeable omissions are cash, demand deposits, insurance and federal government obligations. The survey covers only one-half of all financial assets held by individuals—a large restriction at the outset. Further, as in all savings surveys, there is under-reporting of income in which respect there seems no reason to believe that the residents of Wisconsin are any better or worse than others. In that case probably 60 per cent. of all non-federal cash-interest payments and 34 per cent. of corporate dividend payments will have gone undisclosed. But exactly how this deficiency is spread through the income groups cannot be estimated and consequently Mr. Atkinson decided not to attempt any correction for it, although it obviously detracts from his evidence about the distribution of assets among different income groups. Moreover the calculation of the value of the assets from which the income has been derived involves some major assumptions: a moment's reflection will see the difficulties in assessing the value of stock, even if it is quoted, from reported annual income while for stock which is seldom traded, there must be a large element of guesswork.

Mr. Atkinson is well aware of the inadequacy of his survey and exposes the errors in simple statistical terms. His justification for it is that it adds something to the understanding of the factors affecting asset-holding and that, as far as interest and dividend receipts go, the personal interview methods seem in the past to have provided even less complete information. Nevertheless readers of this book are likely to feel that the prospects of improving the interview method are greater than those of overcoming the problems of this more unusual approach.

MARJORIE DEANE.

10.—*Economic Issues: a Financial and Economic Debate in the Critical Years* 1954–57. Edited by Stephen Frowen and H. C. Hillmann. London, Waterlow, 1957. xi, 231 pp. 9½". 21*s.* 6*d.*

In this volume are reprinted some thirty articles that appeared in *The Banker's Magazine* during the years 1954 to 1956, with a few postscripts added in 1957. They make an interesting collection of some current thinking on inflation and monetary policy although they cannot claim to form a balanced study. But here we find, for instance, Professor Shackle examining the liberal tradition in economics as argued by Professor Robbins, Mr. Roy Harrod pleading for early action in management of Bank rate, and Sir Ralph Hawtrey giving his views on the mechanism of the banking system.

Of most interest to statisticians will be the articles that review the modern tools of economic analysis. Mr. Colin Clark adds spice to the proceedings by taking the Victorian statisticians to task for their preoccupation with national wealth rather than with national income and suggests that there is danger of a revival today of confused thinking about real capital and money claims. He uses Mr. Redfern's valuation of capital stock to discourse on the relationships between national capital and national output, a discourse, however, which needs modification in the light of Dr. Barna's more recent estimates which fairly conclusively demonstrate that for manufacturing anyway, Mr. Redfern has under-valued the stock of capital. In an admirable paper on the development of national income statistics, Mr. Saunders gives some space to the new system of accounting known as "flow of funds" and to speculation on the uses of input-output analysis in such problems as how different forms of final demand affect different industries. In another essay Dr. Klein provides an interesting comparison between the spending and saving habits of British and American consumers and it is perhaps in the sphere of demand analysis that these papers on economic statistics suggest hope of most development. MARJORIE DEANE.

11.—*The Demand and Supply of Scientific Personnel.* By David M. Blank and George J. Stigler. New York, National Bureau of Economic Research. London, Oxford University Press, 1957. xix, 200 pp. 9". $4.

This monograph is a study of methods by which movements in the supply and demand for scientific personnel can be explained. The methods examined are applied to the recent situation, up to 1955, in the United States. The technical professions are defined as those whose subject matter is natural science and its applications. Of these, much the largest is engineering and much the second largest consists of the chemists. The remainder, which collectively are smaller in number than the chemists, include physicists, mathematicians, biologists and geologists. An examination of the population census of engineers and chemists from 1870 to 1950 shows that the number grew 17 times as fast as the labour force. The authors consider, however, that the figures are an underestimate since engineers and chemists cannot be defined uniformly throughout the period, partly owing to the absence of a clear line between skilled workers and academically trained technicians. Another picture of the general pattern of growth from 1900 to 1955 is given by the numbers of Bachelors' and Doctoral Degrees in natural sciences. Further evidence of the growth of the technical professions is the extent to which organized research has developed. Figures on this subject are quoted.

On the question as to whether there has been a shortage of engineers and natural scientists it is argued that a shortage would have been reflected in relatively high salaries. Accordingly the movement in the salaries of such persons is compared with that in wages and salaries generally. The conclusion is that up to 1955 there had been an increasingly ample supply of engineers. The authors adhere to this conclusion in spite of opinions to the contrary but they express the wish that more adequate data were available. Predictions by the Bureau of Labor and by the Engineers' Joint Council as to future demands are examined.

In view of the fact that the number of engineers and chemists rose from 50,000 in 1900 to 625,000 in 1950, from 0.18 to 1.11 per cent. of the labour force, a chapter is devoted to the factors influencing this growth. Part of the growth is attributable to the expansion of industries in which engineers and chemists are employed and a contraction in others. Another factor is the rapidly expanding programme of research by the Federal Government mainly in national defence. There has also been some substitution of highly trained personnel for ordinary skilled workers. Moreover, changes in the technology of production have increased the demand for highly trained men.

As regards the supply of engineers the number has, in the past, been able to grow at a high rate with an increasingly high educational level. The primary cause of this has been the fact that it has been highly remunerative for a young man to get college training. In other words, the excess of income for a qualified engineer over that of a man with a high school training has been more than sufficient to cover the additional cost of education. Entrants to the engineering profession are of two main types. There are those with four or more years college training and holding at least a first professional degree and the others who have had less than this period at college and may have graduated from a technical institute or who may simply have been trained on the job. In the first category are rather more than half of the total number in the profession and the influx on the average in 1951 to 1955 was over 28,000 a year. If the same rate of increase continues, the authors estimate that the influx may be doubled or even trebled by 1970. As regards the non-graduate engineers little appears to be known about the ways they enter the profession or the functions they perform but such information as is available is examined.

The numbers of mathematicians and physicists are stated to be not large—7,350 and 11,520 in 1950. Amongst those with the degree of Ph.D. in 1951 87 per cent. of mathematicians and 58 per cent. of physicists were college teachers. The demand factor, therefore, is largely determined by the average number of college students per teacher and the level of enrolments. The available material on this subject in recent years is examined. The general conclusion is that for mathematicians and physicists there will be at least an adequate proportion of doctorates in the future.

The above is a very brief summary of the methods by which the trend in the supply and demand for scientific personnel can be examined on the basis of statistics available in the United States and conclusions drawn as to the future position. In view of the importance of this subject in British economy the treatise provides a useful indication as to how the position can be assessed and illustrates the kind of statistics which need to be initiated or developed. Above all, perhaps, it shows the necessity, for the benefit of future statisticians working on the subject, of devising adequate definitions of various types of scientific personnel, particularly of those in engineering, which can be maintained over a period of years.

R. B. Ainsworth.

12.—*The Growth of Public Employment in Great Britain.* By Moses Abramovitz and Vera Eliasberg. Princeton: University Press, 1957. London: Oxford University Press. xiii, 151 pp. 9″. 30*s.*

This study entirely lives up to the very high standards of precision and thoroughness established by its sponsors, the National Bureau of Economic Research in New York. Census material from 1891 to 1931 is skilfully welded on to more recent information from government returns to show the growth of employment, distinguishing the requirements of public administration from the Forces, Post Office, public utilities, and teaching.

In a remarkable feat of statistical comparison, the authors give comparable figures for Britain and the United States at decennial intervals since 1900. The labour requirements of public administration have also grown rapidly in the United States, but their comparable figure remains well below ours. The proportion of the labour force required to operate communication services.

on the other hand, (including in the United States privately-owned telephone and telegraphic services as well as the post office) is lower in Britain than in the United States.

The authors also draw attention to the important point that since 1900 the hours worked by public officials have only been slightly reduced in the United States, and definitely increased in Britain, while the hours of other workers have considerably fallen. The rise in the proportion of our total *effort* which we devote to public administration has therefore been even more marked than appears at first sight.

COLIN CLARK.

13.—*Family and Neighbourhood.* By J. M. Mogey. London, Oxford University Press. 1956. xvi, 181 pp. $8\frac{1}{2}''$. 30*s.*

It was intended that a substantial survey should be made of the relationship between "Family" and "Neighbourhood", how substantial is not clearly indicated, but the funds necessary to support this project had not been forthcoming, at least not by the time this book was published. As a preliminary to this larger study, a small pilot survey was carried out under the general direction of the Oxford Pilot Social Survey Committee and this volume records the results of this pilot.

It seems clear that the particular aspect of neighbouhood influence which was under consideration was the effect of moving from a well-established old neighbourhood to a new housing estate. Ideally, as the author himself points out, the only conclusive evidence of such influences requires a study of families in an old neighbourhood and, later of the same families after their removal to a new estate. In fact in this pilot survey no attempt was made to ensure this close correlation between the families involved and the subjects for study were provided purely as random families on an old and a new estate. Nevertheless the reader cannot fail to gain the impression that many of the differences found between the two classes of family are in fact derived from the environment and indeed the author frequently points out that this may well be so, whilst reminding the reader that the survey design in fact prevents this being proved.

Both the areas chosen are in Oxford, the one a decaying area in the centre of the town, the other a mainly post-war council estate on the perimeter. Both, therefore, are set in the changing picture of the University-cum-Market Town turning over to motor car manufacture, with all the implications of immigration from depressed areas and the changing values of employment opportunity turning from the University servant to the factory worker.

Background information is provided from the 1951 Census—the General Register Office created, for this project, Census Tracts similar to those employed in the U.S.A. The survey proper was not in the form of completing questionnaires, but in free interview of families and their individual members. The families were selected by random area sampling. The author takes no pains to hide the fact that, from its very nature, no irrefutable conclusions can be drawn. Since the object of the survey was exploratory, as a pilot to a larger survey, this is only to be expected. Nevertheless this survey undoubtedly points to some interesting differences between old- and new-estate families. For instance there is the non-acceptance of the children of other families in the new-estates, leading to parents keeping their children playing in the house or back garden. On the old estate the children must of necessity play together on the street since the houses have no garden and but a small backyard. Social intercourse between the new-estate dwellers is also restricted by lack of proximity of relations, lack of the old friendly public house and the old-established church. They are thus driven by the need for some contact with society to "knowing" their immediate neighbours on each side, and thus chain contacts along (but not across) streets are established.

Of no less interest are the common features of the two sets, suggesting deeper features of the working class, at least in Oxford. The attitude to employment, for instance, is that it is best forgotten at home.

In spite of the limited objectives of this pilot study, it has clearly been made most energetically and the author has marshalled his facts in a clear orderly fashion and is to be congratulated on his work.

This book is well written, with adequate but not superfluous footnotes and technical appendices. Thus it is eminently readable. For anyone intending to research further into the field considered, it is approaching the indispensable and for others interested in Society it will prove most interesting.

N. H. CARRIER.

14.—*American Housing and its Use: the Demand for Shelter Space.* By Louis Winnick ... for the Social Science Research Council. New York, J. Wiley, 1957 (London, Chapman & Hall). xiv, 143 pp. 9″. 44*s.*

This one of a series of monographs devoted to a detailed examination of the 1950 United States Census. The field covered is rather narrower than the title implies: it is the density at which "non-farm" households occupy their dwellings, expressed as the average number of persons per room (the PPR ratio). Whilst the pathological effects of overcrowding have been of interest mainly to sociologists, Mr. Winnick is concerned with elucidating the economic and social factors affecting this density at all levels. The manifold information produced by the U.S. Census makes it possible to relate density to rent, house values, income, the age of heads of households, family sizes and other factors. Extensive use is also made of material from the Bureau of Labor Statistics, the National Health Survey 1935–36 and various public and private surveys for supporting and supplementary material. The tabular material is plentiful and well presented and a great deal of information is packed into this slim volume. Occasionally, however, conciseness is achieved at the expense of clarity; e.g. "the lowest rent/value-income ratio class".

Mr. Winnick starts from the argument that, though the United States is the best housed nation, its superiority with regard to housing density is not as great as its lead in income and wealth would imply. Whilst 85·5 per cent. of its urban households live at less than one person per room, the comparable figures for Canada and Britain are 83·5 per cent. and 81 per cent. respectively. The reasons why American households are not housed more spaciously are examined in turn and a multiple regression analysis has been worked out for the three variables that appear to have affected density most closely; namely for the percentage changes from 1940–50 in rents per room, household sizes and the median income of the "experienced labor force". The correlations are based on data from 94 cities of more than 100,000 inhabitants.

Too much is perhaps made of the results. The wartime decade was one of great upheaval in the United States as elsewhere; the cities selected may be representative but no one could call them homogeneous; Census errors leading to the under-representation of small and rented houses are pointed out; and there are serious limitations to the concepts employed. For instance, in a general census one room is as good as another, irrespective of quality and size. Mr. Winnick points out that in some of the Western States high densities are partly due to the fact that a large proportion of houses are of modern design, in which a few large multi-purpose rooms replace more and smaller rooms. These and other limitations to which the data are subject receive some attention. Yet conclusions are stated and the coefficients are applied with a degree of confidence which not everyone will share. For instance, on the basis of the regression analysis an estimate is hazarded at the number of rooms and dwellings which would have been vacated if there had been no rent control in the United States. The figure is surprisingly small and one cannot help feeling that such guessing, unverifiable as it is, can only be of use to irresponsible people. It is also somewhat difficult to believe in a negative correlation between rent and income. Mr. Winnick attributes it to the fact that families are larger in the higher income groups and that other necessary expenditure depresses rent. There is a sharp increase in rent as the size of dwellings increases and a 3-person household would have to pay about ten times as much rent as a one-person household to achieve a low PPR ratio.

Considerable attention is devoted to an analysis of changes in household sizes since 1940 and it is shown that the demand for new houses in the U.S.A. does not come mainly from newly married couples. These apparently live with their in-laws at least as commonly as in this country. New households tend to be set up when their children reach school age.

Comparisons with other countries are not pursued; many will suggest themselves to the reader familiar with the British censuses. It is, for instance, interesting to find that very large households are housed more spaciously in this country than in the U.S.A. This partly accounts for the fact that what Mr. Winnick calls "severe overcrowding"—more than 2 persons per room; more than 1 is merely "overcrowding"—is found in 1·8 per cent. of urban households in the U.S.A. but only in 0·9–1·2 per cent. of households in the urban areas of England and Wales. Interesting parallels can also be drawn regarding the effects of rent control in the two countries.

E. H. RUTLAND.

15.—*London Travel Survey*, 1954. London Transport Executive, 1956. [vi] 63 pp. 9″. 5*s.*

Undertaken by the Social Survey on behalf of the London Transport Executive, the British Transport Commission and the Road Research Laboratory of the Department of Scientific and Industrial Research, the enquiry whose main results are reported in this booklet was a repetition of the first London Travel Survey of 1949. In broad outline—though with a wider scope and an improved presentation of the results—it follows in the steps of the earlier survey. A sample of households—some 5,500 as against 3,000 in 1949—was interviewed in and around Greater London as to the number and character of the journeys made during the previous week by each person; and these data were analysed in conjunction with background information on such factors as age, sex, status—distinguishing earners, housewives and children—household income, ownership of motor vehicles and bicycles, and distance from public transport.

In important respects, the present survey represents a considerable advance on the earlier one. Sufficient interviews were carried out in six peripheral "country towns"—including Hemel Hempstead New Town—for results to be presented for each of these towns separately, though regrettably all too briefly. More information was obtained on "casual" as distinct from "regular" journeys and on the use of private as well as public transport—though journeys on foot were still, in the main, excluded. Analysis of the results presented in the report in terms of the geographical origin and destination of travel is a particularly welcome improvement on the 1949 survey, whose published data lost in value from the absence of any information on the direction of journeys. Especially useful is the major distinction between travel to workplaces in Central London and to workplaces elsewhere (even if this distinction would have been still more fruitful and striking, had the Centre been so defined as also to include St. Marylebone and possibly St. Pancras, both boroughs on balance importing large numbers of commuters every day).

Interest may in fact centre mainly on the information presented on journeys to work—which, according to the report, account for the majority of all journeys by public and private transport in the London area: for no less than two thirds—and generally more—of journeys by train, by motor-cycle and by bicycle; for half of those by bus; though for only two-fifths of journeys by car. (Cars, it seems, are often used rather for casual journeys, outings and shopping expeditions than for regular journeys to work—especially if the workplace is in the Central Area, though even so the absolute number of people with jobs in the Centre arriving there by car was estimated at 32,000 already in 1954.) The particular value of the survey here lies in the way in which its results can supplement the basic data on movements between homes and workplaces available from the Census. Broadly, the picture obtained by analysis of the Census material is confirmed and in some directions extended. The general pattern of a multitude of fairly short distance, local and near-local journeys, overlaid by a heavy volume of long-distance travel to the Centre, emerges again from analysis of the flows. And it can be seen also, for instance, in the information given on travel costs: in 1954 there was little difference in average expenditure on travel to work by public transport, between the worker living in the "Inner Zone", whether working in the Centre or elsewhere, and the worker living in the "Outer Zone" and working anywhere but in the Centre—the average in these cases was between 4*s.* 8*d.* and 5*s.* 8*d.* per week; but the worker living in the suburban "Outer Zone" and working in the Centre spent on average about twice as much on fares to work. Again, it is significant that among male workers with homes in the suburban ring and earning under £10 a week—comparatively few of whom would have jobs in the Centre—no less than a third were able to walk or bicycle to work every day.

It is, however, a pity that the brevity of the report—while presumably helping to keep the price agreeably low—has involved the exclusion of much detailed material, which must—as is also often suggested by hints in the text—have been available to the authors, and which if published would have enhanced its value. It would, for instance, have meant a gain in interest—and also, one suspects, in clarity of presentation—if analysis in terms of the origin and destination of journeys had been more consistently adopted and, in particular, extended to cover, not only travel to work and all travel irrespective of purpose—as in the chapter on traffic flows and the accompanying tables and charts—but also journeys for such other specific purposes as shopping and entertainment. Again, it would be valuable to know more about differences in the character and destination of journeys to work and for other purposes made by people at different levels of income, living in different areas. In this connection, one might perhaps venture to suggest that, in a possible third London Travel Survey, background information should be sought not only on household income, but on occupation, industry and, if possible, also on the length of time which the household has spent at its present address and the district from which it moved there.

These comments arise mainly out of the limitations which the authorities concerned and the authors of the report have imposed on themselves, and only emphasize the value, present and potential, of such enquiries which, by supplementing Census and similar material, can help to fill the still far too wide gaps in our knowledge of the structure of our towns.

J. H. WESTERGAARD.

16.—*A National Survey of Readership of Newspapers and Magazines on a Continuing Basis.* London, The Institute of Practitioners in Advertising, 1956. 8″ × 13¾″. 210*s.*

This survey of press readership has certain novel features that distinguish it from other readership surveys. One of the most important is its continuity; field work was started in April 1956 and three quarterly investigations were carried out during 1956. The first volume to be published contained the results for the two middle quarters and an additional set of tables subsequently brought the information up to the end of the year. A 1957 survey is being undertaken in like manner with the intention of providing new material approximately every three months.

Altogether, in the nine months of 1956, 15,551 effective interviews were made with people aged 16 and over. The field work was carried out by Research Services Limited (the subsidiary of the London Press Exchange). The sample was designed by Professor Kendall and was drawn from the electoral register, using a three-stage sampling design making special provisions for picking up a sample of non-electors. In the event the number of non-electors fell short of the anticipated number and it was agreed that some attempt should be made to improve on this in future.

But the improvements that can be made in the sampling plan are likely to be only marginal. The technical points which will arouse most interest surround the problems of defining readership, of questionnaire design and of interview methods. A new feature in this survey aimed at eliminating bias due to fatigue in replying by rotating the order of the list of publications presented to the informant. In the end the Institute has produced larger readership figures than any previous survey. These may indeed be inflated since freer play has been given to the suspicion that some respondents may have been influenced in their replies by what they feel they ought to read as distinct from what they really do. But the very concept of readership is a nebulous one and there is no doubt that the modifications made in this survey are an improvement. Some reservations there will always be about this type of social survey but this one should provide a reasonably accurate picture of what kind of people read different publications. The readership of over a hundred newspapers and magazines is analysed by age, sex, social class, region and size of town.

For those who want a fuller explanation than is provided by the notes accompanying the tables, a more detailed explanatory manual has been prepared by Dr. Mark Abrams and is published by the IPA. This also gives some interesting general information obtained from the survey such as the ownership of durable consumer goods by social class and the social status rating of various occupations.

MARJORIE DEANE.

17.—*Company Reports and their Presentation.* Society of Investment Analysts, Ltd. [London, 1957]. 20 pp. 9½″. 5*s.*

The Society of Investment Analysts, 10 Old Broad Street, London, E.C.2 has published a helpful booklet on *Company Reports and their Presentation.* It may be remarked that the Society, whose members are drawn in the main from banks, insurance companies, investment trusts, pension funds and stock exchange firms, is concerned *inter alia* "to improve the general quality of investment analysis and analytical techniques". Since the war, the idea has been gaining ground that shareholders are entitled to be kept better informed than in pre-war days of the activities of the companies of which they are in law the proprietors. Following the Report of the Cohen Committee the statutory requirements on the accounting side were materially increased by the Companies Act of 1948, and many companies have gone further. In addition, attractively printed brochures of the "glossy" type, describing the plant, processes and development of the larger concerns have become familiar, while picturesque diagrams, graphs and tables frequently support the statutory forms of accounts.

The information which companies are required by law to publish is generally regarded as a minimum, and the object of the booklet is to suggest how such statutory information should be presented and what further facts and comment should be available, if a balanced long-term view of a company is to be taken. Much prominence is given by the compilers to the "Chairman's Address", the desirable content of which is indicated in some detail. It is suggested that the address should cater for the needs of laymen rather than of the initiated reader, and that it should ensure that shareholders who cannot understand accounts are not at too great a disadvantage compared with those who can. Shareholders, it is remarked, who cannot make head or tail of a balance sheet automatically seek information in the accompanying narrative instead.

In view of accounting controversies on historical cost versus replacement value as a basis for depreciation provision, it is interesting to note that the authors think that the profit and loss account should show: (1) the depreciation based on the cost to the company of the assets; and (2) "if appropriate, as a separate item, the additional amount set aside to bring the combined depreciation charge to a figure such that the company retains, during the life of the assets, sufficient to finance replacement at current prices." It must be remarked that (2) is still a matter of controversy. It is a counsel of perfection that few companies adopt in so precise a form. The amount, if any, so appropriated will, of course, depend in practice on the year's profits There are other matters of controversy in the Society's recommendations, e.g. should future tax reserves be regarded as part of the capital invested in the business? The booklet draws attention to what is described as a major defect in both law and practice, viz.:—that no statutory provision is made for informing shareholders who were not present in person of what actually took place at the annual general meeting. The authors have had much in mind the difficulties that those without "a certain amount of accounting training" may have in interpreting balance sheets and profit and loss accounts. Perusal of the booklet may often serve as an intense and usually understandable short course for shareholders who may be uninitiated in accounting matters.

F. A. A. MENZLER.

STATISTICAL NOTES

(1) British Official Statistics

The index of retail prices compiled by the Ministry of Labour and National Service which was 105 (January 17th, 1956 = 100) in May rose to 106 in June and 107 in July. Between mid-May and mid-June there were increases in the average prices of potatoes, bacon and eggs. In the following four weeks increases for eggs, potatoes, milk and coal were partly offset by some reductions in price. The detailed figures of the weights used in calculating the index and the indices for the different commodity groups were as follows:

	Food	*Alcoholic-Drinks*	*Tobacco*	*Housing*	*Fuel and Light*	*Durable Household Goods*	*Clothing and Footwear*	*Transport and Vehicles*	*Miscellaneous*	*Services*	*All Items*
Weights:	350	71	80	87	55	66	106	68	59	58	1,000
May 14th, 1957 .	103·9	103·9	105·3	109·4	101·8	100·5	102·0	108·7	106·5	107·7	104·6
June 18th, 1957 .	106·7	103·9	105·3	109·7	102·9	100·5	102·1	108·4	106·6	107·9	105·7
July 16th, 1957 .	108·4	104·1	105·3	110·0	106·3	100·6	102·1	109·0	107·0	108·4	106·6

The Ministry of Labour index of weekly wage rates, calculated on the basis of January 31st, 1956 = 100, showed that in May, 1957, the level was 110·6. It rose to 110·7 in June and to 111·0 in July. For manufacturing industries alone the figures were 110·7, 110·9, and 111·2. The principal increases in June affected engineering and shipbuilding, rubber manufacture, and the wholesale grocery trade. In July the main increases were in general printing, the motor vehicle retail and repairing trade, company-owned and municipal omnibus undertakings, merchant shipping and cinemas.

The total working population and the numbers in civil employment in the three months ended June 1957, were as follows:

(*Thousands*)

	Total Working Population			*Numbers in Civil Employment*		
Date	*Males*	*Females*	*Total*	*Males*	*Females*	*Total*
April 1957 . .	16,148	7,852	24,000	15,231	7,741	22,972
May 1957 . .	16,124	7,854	23,978	15,242	7,756	22,998
June 1957 . .	16,111	7,858	23,969	15,253	7,771	23,026

The number of persons on the unemployment registers of the Employment Exchanges fell by 48,700 in June and by a further 20,400 in July. The total for July represented 1·1 per cent. of the total number of employees in Great Britain. The percentages in the different Regions ranged from 0·7 in London and the South-East to 2·1 in Wales and 2·2 in Scotland.

The following is a sex analysis of the figures:

Number of Unemployed Persons on the Registers of Employment Exchanges

Date	*Men and Boys*	*Women and Girls*	*Total*
May 13th, 1957 . . .	214,978	98,494	313,472
June 17th, 1957 . . .	182,246	82,477	264,723
July 15th, 1957 . . .	171,446	72,860	244,306

Of the total of 244,306 in July, 57,938 had been unemployed for not more than two weeks, 57,007, for two to eight weeks, and 115,136 for more than eight weeks, while 14,225 were temporarily stopped. In the four weeks ended July 10th, 150,535 vacancies were filled by the Employment Exchanges. The number of vacancies unfilled at this date was 331,757.

The numbers of insured persons absent from work owing to sickness, including self-employed as well as employed, were 859,400 in May, 834,700 in June and 793,200 in July. The numbers of employed persons absent owing to industrial injuries were 56,500, 56,700 and 56,000.

A study of wages and related elements of labour costs in industry in some European countries has recently been undertaken by the International Labour Office. The first part of the survey relates to the cost to employers of national security schemes and other social and welfare services. A summary of the results of the survey covering iron and steel, shipbuilding, machine tools, boot and shoe, cotton, coal mining and railways in Great Britain is published in the August issue of the *Ministry of Labour Gazette*. The figures show that the average expenditure by employers per employee in 1955 on social security contributions, subsidies and other payments related to labour costs in the seven industries named, ranged from a little under £25 to £39, except in coal mining, where it was £63.

The latter relatively high figure was largely due to heavy payments from a reserve fund to victims of accidents and diseases contracted before July 1948. In addition to the payments covered by the statistics between 6 and 7 per cent. of the total man-hours worked in these industries were man-hours paid for but not worked, i.e. annual and public holidays.

The Fourth Conference of British Commonwealth Statisticians, a report of which has now been published (H.M. Stationery Office, 2*s*.) was held in London during September 1956, the Third Conference having been held in Canberra, Australia in 1951.

Delegates from the following countries and colonial governments attended the Fourth Conference: Australia, Canada, Ceylon, India, New Zealand, Pakistan, Federation of Rhodesia and Nyasaland, Union of South Africa, United Kingdom of Great Britain and Northern Ireland, Barbados, East Africa High Commission, Gold Coast, Jamaica, Federation of Malaya, Nigeria, Singapore, and Trinidad. Official observers were present from the Irish Republic, the Statistical Office of the United Nations and the Commonwealth Economic Committee.

The Conference was opened by the Earl of Selkirk, O.B.E., A.F.C., Chancellor of the Duchy of Lancaster, who welcomed delegates to London on behalf of Her Majesty's Government. Mr. H. Campion of the United Kingdom was elected Chairman of the Conference.

The following subjects were discussed: the statistical activities of Commonwealth countries, national income statistics, capital formation, stock statistics, retail prices, agricultural statistics, industrial statistics, wholesale prices, population statistics, external trade and balance of payments, electronic aids to statistical calculation and the general application of sampling methods.

The recruiting, training and exchange of personnel was discussed with particular reference to the needs of Colonial territories, and the possibilities of keeping countries informed between Conferences by some systematic exchange of information about statistical experience and developments were also considered. It is intended to arrange a further Conference within the next five years.

The Air Ministry Meteorological Office has issued in *Professional Notes* No. 121 (H.M. Stationery Office) a statistical study of the variation of wind with height. This variation is examined by means of vector correlation coefficients in a similar manner to the variation of wind with distance and time published in *Geophysical Memoirs* No. 93.

The correlation coefficients are given for the British Isles up to 100,000 ft., for Habbaniya up to 40,000 ft. as indicating the conditions in middle latitudes, for Nairobi indicating conditions in the tropics and for Barrow, Alaska, indicating conditions in the far north.

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(2) OTHER STATISTICS

World Wheat Statistics (International Wheat Council, London, 20*s*., pp. 83) gives figures of production, trade, prices of wheat and flour, in the various countries, for the period 1949–50 to 1956. Quantities are now given throughout in metric tons, and prices in U.S. dollars both per metric ton and per bushel. Special tables analyze the supply and distribution of wheat and flour in the six principal wheat exporting countries. Tables of ocean freight rates, and for converting the various units used into each other are given at the end of the volume.

Company Income and Finance 1949–53. (Printed for private circulation by the National Institute of Economic and Social Research, price 10*s.*) The Institute produced in 1955 the first fruits of its study of company finance, using company accounts published since the Companies Act of 1948. This took the form of "A classified list of large companies engaged in British Industry" and is now followed by a series of tables showing, for each of 21 industry groups and for the whole body of 3,025 companies tabulated, a summary appropriation account, balance sheet and statement of sources and uses of capital funds. A short accompanying text explains the form and scope of the tables, relates the group of companies covered to the whole economy and illustrates some of the ways in which the figures can be used in studying economic trends and relations.

The work on published company accounts begun by the Institute is being continued by the Board of Trade for the years after 1953. The Institute intends to publish in book form a complete account of its studies of the 1949–53 series, and this may be expected in about a year's time.

A mathematical conference for schoolteachers and industrialists was held at Trinity College, Oxford, in April 1957, and the abbreviated proceedings have now been issued (published by Technology, The Times Publishing Co. Ltd.). The conference was designed to broaden school teaching by providing contact between teachers and those who develop science. This was done by arranging for representatives from schools, universities, technical colleges, industry and research organizations to meet and talk together for a sufficiently prolonged period to understand each other's work. The conference confirms the need for more and continuing liaison between schools and industry. It demonstrated that even the best qualified schoolteachers can grow rapidly out of date and that a regular and efficient means should be sought for keeping teachers up-to-date. The subjects covered included, among others, electronic computors, problems in physics and engineering, linear programming, mathematics in the Royal Aircraft Establishment, mathematics in the textile, chemical and steel industries, and the mathematical education of scientists.

CURRENT NOTES

Messrs. Charles Griffin and Co. Ltd., the publishers of many leading statistical works, are publishing a new series of statistical monographs and courses, under the editorship of Professor M. G. Kendall. The purpose of the series is to make available specialized studies particularly in newly developed fields in which the subject has not reached the stage where a full-scale text book would be justified. The first of these studies is the *Analysis of Multiple Time Series* by Mr. Quenouille. The second, *A Course in Multivariate Analysis* is by Professor Kendall.

The Trustees of the Houblon-Norman Fund invite applications for Fellowships or Grants in aid of Research into the working of industry and finance in Great Britain and elsewhere and the economic conditions affecting them.

Fellowships, which are intended for experienced research workers, are awarded for full-time research for one year which may be renewed for a second year. Grants are awarded for either full-time or part-time work. The amount of the awards, which will take effect from October 1st 1958, will depend upon the circumstances of the candidate and the probable expenses of his work.

Candidates must be British subjects normally resident in the United Kingdom.

Forms of application, which may be obtained from the Secretary of the Fund, c/o the Bank of England, London, E.C.2, should be returned not later than March 1st, 1958.

The Trustees of the Houblon-Norman Fund, on the recommendation of their Advisory Committee, have made the following awards for 1957–58.

Research Grants.—Mr. J. D. Bailey, Research Student, Nuffield College, Oxford (Renewal), "British Investment in Australasian Financial Companies, 1870–1900"; Mr. G. E. Clayton, Lecturer in Economics, University of Liverpool, "Banking in Spain and in Portugal"; Mr. A. B. Cramp, Research Student, London School of Economics, "Operation of Bank Rate, 1822–60"; Mr. R. Davis, Lecturer, University of Hull (Renewal), "English Shipping Industry, 1660–1702"; Mr. A. G. E. Jones, Teacher, Bristol College of Commerce, "Growth of Banking in Suffolk, 1700–1900"; Mr. R. V. Lennard, Historian (Renewal), "Rural England in the Middle Ages";

Miss C. A. P. Leubuscher (Renewal), "Shipping Facilities available to the overseas trade of Colonial Territories"; Mr. J. W. McCarty, Research Student, Cambridge University, "British Investment Overseas in Non-Ferrous Metal Mining, 1870–1914"; Miss J. E. Mitchell, Lecturer, University of Nottingham (Renewal), "The Balance of Payments—Crisis of 1951"; Mrs. C. S. Orwin (Renewal), "A History of Modern British Agriculture, 1840–1913"; Dr. L. S. Pressnell, Lecturer, University College, "Money in the Eighteenth Century"; Mr. P. Robson, Lecturer, Queen's University, Belfast, "Fiscal and Financial Aspects of Post-war Capital Levies"; Professor R. S. Sayers, F.B.A., London School of Economics, "A Study of American Banking"; Mr. J. S. G. Wilson, Reader, London School of Economics, "Banking in France and the Netherlands"; Mr. G. A. Yewdall, Research Student, University of Leeds, "Population movements in certain northern towns during the early part of the nineteenth century".

STATISTICAL AND ECONOMIC ARTICLES IN RECENT PERIODICALS

UNITED KINGDOM—

Accounting Research—

July 1957—Depreciation, income taxes and growth: *S. Davidson.* Dividend declarations and inventory valuations: *A. R. Cerf.* The reliability of national income statistics: *H. Burton.* Improving accounting measures for management: the concept of homogeneity in accounting data: *E. H. Weinwurm.*

Advancement of Science—

June 1957—The social effectiveness of the mentally handicapped: *N. O'Connor*. The economic effects of automation: *H. D. Dickinson.*

Anglo-Soviet Journal—

Summer 1957—Statistics of social change in the U.S.S.R.: *V. S. Nemchinov.*

Annals of Human Genetics—

June 1957—Body size and weight in relation to varicocoele and hernia: *S. M. Smith.* On the estimation of intraclass correlation: *C. A. B. Smith.* Le nombre de dermatoglyphes dans un échantillon de jumeaux: *M. Lamy*, *J. Frézal*, *J. de Grouchy* and *J. Kelley.* The log likelihood ratio test (the *G*-test): methods and tables for tests of heterogeneity in contingency tables: *B. Woolf.*

British Journal of Preventive and Social Medicine—

April 1957—Stature and weight of men from England and Wales in 1941: *E. M. B. Clements* and *K. G. Pickett.* Random selection in the therapeutic trial: *R. F. Wrighton.* A note on the practical interpretation of 2 × 2 tables: *J. H. Edwards.* Recent trends of mortality from violence: *W. J. Martin.*

Cambridge Philosophical Society, Proceedings—

July 1957—Some applications of Mellin transforms to the theory of bivariate statistical distributions: *C. Fox.* Percolation processes. I. Crystals and mazes: *S. R. Broadbent* and *J. M. Hammersley.* II. The connective constant: *J. M. Hammersley.*

Economica—

August 1957—The British national debt. Part I: *E. G. Jones* and *E. Nevin.* Tariffs, subsidies and the terms of trade: *W. M. Corden.*

Institute of Actuaries Students' Society, Journal—

July 1957—Some aspects of policy reserves and premiums under the new mortality tables: *E. M. Thomas.* Economic policy: *H. C. Cottrell.* The practical application of statistics in the business world—how much mathematics does it require? *B. T. Ramm.* The population of the British Caribbean: *C. M. Stewart.* A further table of binomial coefficients: *M. T. L. Bizley.*

Journal of Agricultural Economics—

June 1957—The significance of variations in annual farm profits: *A. W. Tansey.* Some short-term forecasting methods: *F. Holme.* A note on cereal prices and acreages: *T. W. Gardner.*

Manchester School of Economic and Social Studies—

May 1957—The growth of life assurance in U.K. since 1880: *J. Johnston* and *G. W. Murphy.* An Engel-type curve for cash: *J. Spraos.* Private company finance in the post-war period: *F. R. Jervis.*

Operational Research Quarterly—

June 1957—Management in action: *T. Burns*. The effectiveness of present-day crash helmets for motorcyclists: *K. N. Chandler* and *J. K. L. Thompson*. The consequences of economic stabilization: *A. Tustin*. A comparison of operational research in the U.S.A. and in Great Britain: *R. L. Ackoff*.

Oxford Economic Papers—

June 1957—Anglo-American economic relations in war-time: *R. Opie*. Classical growth: *I. M. D. Little*. An international comparison of "over-employment" trends in money wages: *C. Clark*.

Oxford University Institute of Statistics, Bulletin—

May 1957—Savings behaviour: a symposium.

August 1957—Introducing a new wage structure into coal-mining: *W. H. Sales* and *J. L. Davies*. On measuring business concentration: *P. E. Hart*. Dr. Nevin and the Welsh economy: *J. Parry Lewis*. Fresh fruit and vegetables statistics in Britain: *L. G. Bennett*. The economic consequences of overseas trade concessions: *P. Streeten*. A "human needs" diet, spring 1957: *T. Schulz*.

Population Studies—

July 1957—A demographic study of the British ducal families: *T. H. Hollingsworth*. The measurement of population distribution: *O. D. Duncan*. Death rates, age distribution and average income in stationary populations: *L. Johansen*. The fertility of teachers in England and Wales: *W. Scott*. A new method for calculating Lotka's r: the intrinsic rate of growth in a stable population: *A. J. Coale*.

Royal Meteorological Society, Quarterly Journal—

July 1957—On a distribution of weather cycles by length: *K. R. Gabriel* and *J. Neumann*.

Royal Society of Edinburgh, Proceedings, Section A—

Vol. 64, Part 4—Index polynomials and bifurcating root-trees: *H. Minc*.

Sociological Review—

July 1957—The distribution of social defects in Liverpool: *I. M. Castle* and *E. Gittus*. The group discussion as an interview technique: *J. A. Banks*. Family or sibship position and scholastic ability: *J. P. Lees* and *A. H. Stewart*.

AUSTRALIA—

Statistical Society of New South Wales Bulletin—

April 1957—A brief survey of teaching and research in statistics in Australian institutions. Part I: universities. Confidence, prediction and tolerance intervals and regions in regression: *H. S. Konijn*. Mortality surveys: *H. O. Lancaster*.

INDIA—

Calcutta Statistical Association Bulletin—

July 1957—On some optimum compound decision procedures: *H. K. Nandi*. On the distribution of varieties of $p \times (p-1)$, n-ple latinized rectangular lattices and weighed average variances: *P. M. Roy*. On measures of location and dispersion and tests of hypotheses in a Pareto population: *A. N. M. Muniruzzaman*. Analysis of a 3^3 factorial design: *V. N. Murty*.

Sankhyā—

February 1957—Some observations on input-output analysis: *O. Lange*. The use of a short-term econometric model for Indian economic policy: *J. Tinbergen*. Approximate distribution of certain linear function of order statistics: *K. C. Seal*. On the unboundedness of infinitely divisible laws: *S. D. Chatterjee* and *R. P. Pakshirajan*. A note on the construction of orthogonal Latin squares: *N. Bhattacharya*. A new discrete distribution: *A. Prasad*. An experimental method for obtaining random digits and permutations: *J. E. Walsh*.

On estimating parametric functions in stratified sampling designs: *D. Raj.* A note on estimation of variance components in multistage sampling with varying probabilities: *J. Roy.* A note on two stage sampling: *R. Rangarajan.* Method of matching used for the estimation of test reliability: *P. K. Bose* and *S. B. Chaudhuri.* Recommendations for personnel selection in India based on the British selection methods in the civil service and industry: *R. S. Das.* Isolation of some morale dimensions by factor analysis: *H. C. Ganguli.* Inversion of 25 × 25 matrix on 602A calculating punch: *D. K. Bose* and *A. K. Roy.*

UNION OF SOUTH AFRICA—

South African Journal of Economics—

June 1957—Scientific method and measurable utility: *C. S. Soper.* Ghana in economic transition: *R. M. Lawson.* Railway expansion during the post-war period: *J. C. Laight.*

UNITED STATES—

American Economic Review—

May 1957—Papers and proceedings of the sixty-ninth annual meeting of the American Economic Association, Cleveland, Ohio, December 27–29, 1956.

June 1957—Entrepreneurial income, saving and investment: *I. Friend* and *I. B. Kravis.* Principles of debt management: *E. R. Rolph.*

American Statistical Association, Journal—

June 1957—Errors of the third kind in statistical consulting: *A. W. Kimball.* Sampling in a nutshell: *M. J. Slonim.* National income statistics of underdeveloped countries: *H. T. Oshima.* Estimating the service life of household goods by actuarial methods: *J. L. Pennock* and *C. M. Jaeger.* An application of multivariate quality control to photographic processing: *J. E. Jackson* and *R. H. Morris.* Maximum likelihood estimates for a multivariate normal distribution when some observations are missing: *T. W. Anderson.* The analysis of incomplete block designs: *M. Zelen.* Non-additivities in a Latin square design: *M. B. Wilk* and *O. Kempthorne.* Transformations to normality using fractional powers of the variable: *P. G. Moore.* Linear transformation to a set of stochastically dependent normal variables: *W. J. Moonan.* The regions of unimodality and positivity in the abbreviated Edgeworth and Gram-Charlier series: *G. D. Berndt.* The President's economic report: *A. Russell.*

American Statistician—

June 1957—Nonparametric statistics: *S. Siegel.*

Annals of Mathematical Statistics—

June 1957—Pólya type distributions. II: *S. Karlin.* On minimizing and maximizing a certain integral with statistical applications: *J. S. Rustagi.* On consistent estimates of the spectrum of a stationary time series: *E. Parzen.* Intra-block analysis for factorials in two-associate class group divisible designs: *C. Y. Kramer* and *R. A. Bradley.* Statistical properties of inverse Gaussian distributions. I: *M. C. K. Tweedie.* Variances of variance components. III. Third moments in a balanced single classification: *J. W. Tukey.* Some remarks on systematic sampling: *W. Gautschi.* Tightened multi-level continuous sampling plans: *C. Derman, S. Littauer* and *H. Solomon.* On some characterization problems connected with linear structural relations: *R. G. Laha.* Random orthogonal transformations and their use in some classical distribution problems in multivariate analysis: *R. A. Wijsman.* On the distribution of ranks and of certain rank order statistics: *M. Dwass.* Estimating future from past in life testing: *J. E. Walsh.* Probability distributions of random variables associated with a structure of the sample space of sociometric investigations: *L. Katz* and *J. H. Powell.* Design for the control of selection bias: *D. Blackwell* and *J. L. Hodges, Jr.* On infinitely divisible random vectors: *M. Dwass* and *H. Teicher.* A moving single server problem: *B. McMillan* and *J. Riordan.* Some further methods of constructing regular group divisible incomplete block designs: *G. H. Freeman.* On partially balanced linked block designs: *J. Roy* and *R. G. Laha.* On Borel fields over finite sets: *G. Szekeres* and *F. E. Binet.* On transient Markov chains with application to the uniqueness problem for Markov processes: *L. Breiman.* An approximate formula for the cumulative z-distribution: *J. Wishart.* The mixture of normal distributions with different variances: *D. Teichroew.* Metrics and norms on spaces of random variables: *A. J. Thomasian*. Divergent time-homogeneous birth and death processes: *P. W. M. John*. A regression analysis using the invariance method: *D. A. S. Fraser*. On discrete variables whose sum is absolutely continuous: *D. Blackwell*. A proof that the sequential probability ratio test (S.P.R.T.) of the general linear hypothesis terminates with probability unity: *W. D. Ray*.

Bell System Technical Journal—

July 1957—A sufficient set of statistics for a simple telephone exchange model: *V. E. Beneš*. Fluctuations of telephone traffic: *V. E. Beneš*. Coincidences in Poisson patterns: *E. N. Gilbert* and *H. O. Pollak*.

Econometrica—

April 1957—Production functions for Indian industry: *V. N. Murti* and *V. K. Sastry*. The theory of inventory decisions: *E. S. Mills*. Automobile consumption, 1940-1950: *R. A. Bandeen*. An econometric investigation of the technology of agricultural production functions: *E. O. Heady*. Engineering data and statistical techniques in the analysis of production and technological change: fuel requirements of the trucking industry: *V. L. Smith*. On linear models and the graphs of Minkowski-Leontief matrices: *D. Rosenblatt*. Surrogates for uncertain decision problems: minimal information for decision making: *S. Reiter*. A note on certainty equivalence in dynamic planning: *H. Theil*.

Harvard Business Review—

July–August 1957—The statistically designed experiment: *D. Shainin*. High-speed operations research: *M. E. Salveson*. Profitability index for investments: *R. I. Reul*.

September–October 1957—Marketing costs and mathematical programming: *W. J. Baumol* and *C. H. Sevin*. Never overestimate the power of a computer. *R. F. Lewis*. New dimensions in top executive reading: *E. C. Bursk*. Strategies for diversification: *H. I. Ansoff*. Annual report on executive compensation: *A. Patton*.

Industrial Quality Control—

June 1957—Significance tests and the dollar sign: *F. C. Leone*, *R. B. Nottingham* and *J. Zucker*. Control charts in multi-stage batch processes: *R. S. Bingham, Jr*.

July 1957—The practical calculation of Hotelling's T: *T. W. Bauer*. Dimensions of diversity: *M. E. Wescott*.

August 1957—Trends for quality control: *L. R. Hafstad*. Statistical quality control methods at Rome Cable Corporation: *C. K. Hamlin*. Logical rules for writing and interpreting tolerance specifications: *F. Proschan*. Statistical quality control applied to precision manufacture of weapon barrel: *N. H. Zimmerman*. Application of statistical quality control in glass fabrication: *T. R. Meyer*, *J. H. Zambone* and *F. L. Curcio*.

Journal of Experimental Education—

June 1957—Testing the assumptions underlying multiple regression: *F. A. C. Sevier*. A quick and dirty method for estimating multiple correlations: *W. J. Moonan*.

Journal of Political Economy—

June 1957—Structure and policy in postal rates: *J. Kennedy*. A note on the cost of children's mortality: *W. L. Hansen*.

Management Science—

April 1957—Air Force progress in logistics planning: *W. Jacobs*. Limitations of the scientific method in management science: *E. H. Weinwurm*. A linear programming solution to dynamic Leontief type models: *H. M. Wagner*. The elimination form of the inverse and its application to linear programming: *H. M. Markowitz*. On a dynamic programming approach to the caterer problem. I: *R. Bellman*. The trim problem: *K. Eisemann*.

Milbank Memorial Fund Quarterly—

July 1957—The age for neuroses: *M. Shepherd* and *E. M. Gruenberg*. The differential fertility of the negro population, Houston, Texas, 1940–1950: *J. E. Dodson*. Median age at first marriage in Sweden, 1881–1953: *R. Chambliss*.

Operations Research—

June 1957—Some civil defense problems in the nation's capital following widespread thermonuclear attack: *R. Bentz* and others. Evaluating health screening procedures: *M. S. Blumberg*. A comparison of the original and revised simplex methods: *H. M. Wagner*. An operations-research study of sales response to advertising: *M. L. Vidale* and *H. B. Wolfe*. Distribution of seasonal inventory of the Hawaiian Pineapple Company: *A. R. Eagle*. A Monte Carlo analysis of the ground controlled approach system: *A. Blumstein*. Computational aspects of dynamic programming: *S. E. Dreyfus*. A hand-computation determination of kill probability for weapons having spherical lethal volume: *H. W. Lilliefors*. The mechanical blackboard: *R. E. Machol*. Linear programming isn't always the answer: *D. E. Debeau*.

Psychometrika—

June 1957—Theory of learning with constant, variable, or contingent probabilities of reinforcement: *W. K. Estes*. A component model for stimulus variables in discrimination learning: *C. J. Burke* and *W. K. Estes*. Simple proofs of relations between the communality problem and multiple correlation: *L. Guttman*. A general least squares solution for successive intervals: *G. W. Diederich*, *S. J. Messick* and *L. R. Tucker*. On the applications of the method of absolute scaling: *C.-T. Fan*. A method of score conversion through item statistics: *F. Swineford* and *C.-T. Fan*. A revised law of comparative judgment: *W. P. Harris*. A fast approximate algebraic factor rotation method to maximize agreement between loadings and predetermined weights: *D. A. Rodgers*.

Review of Economics and Statistics—

May, 1957—A new type of socio-economic system: *G. H. Orcutt*. Can speculators forecast prices? *H. S. Houthakker*. Inter-relations of state income and industrial structure: *H. S. Perloff*. Government debt as a generator of economic growth: *F. Gehrels*. Liabilities of business failures as a business indicator: *P. B. Simpson* and *P. S. Anderson*.

Smithsonian Contributions to Astrophysics—

Vol. 2, Nos. 2–4—Studies of solar granulation. I. The statistical interpretation of granule structure from one-dimensional microphotometer tracings: *G. Wlérick*. Variations in the thermodynamic state of the chromosphere over the sunspot cycle: *R. G. Athay*, *D. H. Menzel* and *F. Q. Orrall*.

AUSTRIA—

Statistisches Vierteljahresschrift—

Vol. 9, Nos. 3–4—Wehrstatistik: *W. Winkler*. Statistik und Ökonometrie: *G. Tintner*. Probleme und Methoden einer Betriebsökonometrie: *A. Adam*. Die Identifikation struktureller Beziehungen: *F. Ferschl*. Ein Beitrag zur kollektiven Risikotheorie: *K. H. Wolff*.

CZECHOSLOVAKIA—

Aplikace Matematiky—

Vol. 2, No. 3—A simple nonparametric test of the difference of location of two populations: *Z. Šidák* and *J. Vondráček*. (Czech text, English summary). A note on negative binomial distribution: *O. Hanš*. (Czech text, English summary).

EGYPT—

L'Egypte Contemporaine—

July 1957—La politique cotonnière égyptienne: *F. N. Gabali*. Competitive uses of land for recreation and food production: *O. Wahby*.

FRANCE—

Etudes Statistiques—

April–June 1957—L'élasticité des dépenses alimentaires d'après deux enquêtes sur les budgets familiaux (1951–1953).

Revue d'Economie Politique—

May–June 1957—L'autofinancement de la grande unité interterritoriale et les dimensions temporelles de son plan: *M. Byé.* L'économie française vue à travers le tableau économique: essai d'analyse et de mesure des interdépendances: *P. Maillet.*

July–October 1957—La France économique en 1956.

Revue de Statistique Appliquée—

Vol. 5, No. 2—Analyse d'un plan factoriel d'expérimentation dont les résultats sont qualitatifs: *J. Ulmo* and *F. Bastenaire.* Courbes d'efficacité de la carte de contrôle par mesures en fonction du pourcentage de pièces défectueuses: *G. Rouzet.* La reproductibilité des mesures de degré de polymérisation dans la cupriéthylènediamine et de gonflement sur la fibrane viscose: interprétation statistique des résultats: *J. P. Fleury* and *Th. Reitzer.* Aspect statistique des mesures de grain: *A. H. Bazin.* Méthodes graphiques pour l'analyse des débits de crues: *E. J. Gumbel.* Sur l'application des diverses lois limites des valeurs extrêmes au problème des débits de crues: *M. J. Bernier.*

Société de Statistique de Paris, Journal—

January–March 1957—Les enquêtes de conjoncture auprès des chefs d'entreprises industrielles et commerciales: *R. Joly.* La prévision à l'Institut National de la Statistique et des Etudes Economiques: *J. Dumontier.*

GERMANY—

Weltwirtschaftliches Archiv—

Vol. 78, No. 2—Der Lohnanteil am Gesamteinkommen: einige Bemerkungen zu einem umstrittenen Problem: *K. W. Rothschild.* Growth, output and input of Soviet timber: *M. Lamer.*

ITALY—

Giornale degli Economisti e Annali di Economia—

March–April 1957—Alcuni riflessi economici della diminuzione della mortalità in Italia: *G. Mortara.* Sulle economie bipartite: *S. Cherubino.* Progresso tecnico e teoria matematica della lotta per l'esistenza (con alcune applicazioni): *E. Moroni.*

May–June 1957—Sistema dei prezzi e concorrenza nel mercato comune: *G. Demaria.* Prezzi relativi e programmi di sviluppo: *P. S. Labini.*

Rivista Italiana di Economia, Demografia e Statistica—

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LIST OF ADDITIONS TO THE LIBRARY

Since the issue of Part III, 1957, the Society has received the publications enumerated below.

I.—OFFICIAL PUBLICATIONS

United Kingdom. *Air Ministry. Meteorological Office.* A statistical study of the variation of wind with height; by C. S. Durst. London, H.M.S.O., 1957. 10p. 9¾″. 8*d.* (Prof. Notes Met. Off., **8**, No. 121)

Atomic Energy Research Establishment. List of unclassified A.E.R.E. reports, January 1952 to December 1955; by L. J. Anthony and Miss P. M. Harris. Harwell, 1957. [ii], 23p. 11″. 3*s.* 9*d.* (A.E.R.E. Inf/Bib.110)

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REVENUE OF THE UNITED KINGDOM

Net Produce in Quarters of 1956, *and the Financial Years ended March* 31, 1956/57, 1955/56, 1954/55, 1953/54

£ thousand

	QUARTERS ended				Total for Calendar Year 1956
	March 31, 1956	June 30, 1956	Sept. 30, 1956	Dec. 31, 1956	
Ordinary Revenue—					
Income tax	1,222,754	263,006	289,446	248,550	2,023,756
Surtax	90,400	23,500	11,500	18,300	143,700
Death duties	43,600	44,700	38,100	37,900	164,300
Stamps	16,800	15,200	16,500	14,200	62,700
Profits tax and excess profits tax	41,200	34,100	61,800	53,250	190,350
Excess profits levy	2,450	1,700	1,600	550	6,300
Other Inland Revenue duties	420	110	100	—	630
Total Inland Revenue	1,417,624	382,316	419,046	372,750	2,591,736
Customs	270,465	296,906	283,920	328,430	1,179,721
Excise	235,311	196,230	231,485	247,620	910,646
Total Customs and Excise	505,776	493,136	515,405	576,050	2,090,367
Motor vehicles duties	62,809	10,257	8,531	7,629	89,226
Post Office (net receipt)	—	—	—	5,200	5,200
Broadcast receiving licences	8,000	4,700	5,100	9,800	27,600
Receipts from sundry loans	2,415	8,669	15,736	5,909	32,729
Miscellaneous receipts	54,454	12,738	57,063	60,638	184,893
Total Ordinary Revenue	2,051,078	911,816	1,020,881	1,037,976	5,021,751
Self-balancing Revenue —					
Post Office	69,100	70,600	74,000	75,200	288,900
Income tax deducted from excess profits tax, post-war refunds	397	181	191	140	909

£ thousand

	Year 1956/57	Year 1955/56	1956/57 (compared with 1955/56)		Corresponding Years	
			Increase	Decrease	1954/55	1953/54
Ordinary Revenue—						
Income tax	2,114,173	1,942,902	171,271	—	1,892,965	1,730,991
Surtax	158,000	138,600	19,400	—	134,700	132,400
Death duties	169,000	175,700	—	6,700	187,900	164,500
Stamps	63,000	70,600	—	7,600	75,100	56,600
Profits tax and excess profits tax	195,000	192,500	2,500	—	172,800	188,400
Excess profits levy	5,000	18,450	—	13,450	76,500	65,850
Other Inland Revenue duties	640	770	—	130	1,280	1,390
Total Inland Revenue	2,704,813	2,539,522	165,291	—	2,541,245	2,340,131
Customs	1,198,882	1,148,598	50,284	—	1,100,049	1,042,396
Excise	901,735	864,511	37,224	—	771,637	721,975
Total Customs and Excise	2,100,617	2,013,109	87,508	—	1,871,686	1,764,371
Motor vehicle duties	90,610	86,987	3,623	—	78,980	73,115
Post Office (net receipt)	4,464	—	4,464	—	5,100	—
Broadcast receiving licences	28,400	25,800	2,600	—	22,300	16,400
Receipts from sundry loans	32,714	29,858	2,856	—	23,023	38,014
Miscellaneous receipts	196,195	197,867	—	1,672	195,558	136,099
Total Ordinary Revenue	5,157,813	4,893,143	264,670	—	4,737,892	4,368,130
Self-balancing Revenue—						
Post Office	304,036	264,900	39,136	—	243,600	232,000
Income tax deducted from excess profits tax, post-war refunds	599	2,365	—	1,766	5,186	5,940

REGISTRATION OF THE UNITED KINGDOM AND EIRE

No. I.—ENGLAND AND WALES

Births, Deaths *and* **Marriages** *registered in the Calendar years* 1952–1956 *and in the Quarters of those years. Numbers, Annual and Quarterly Rates*† *per* 1,000 *persons living.* (*Deaths under* 1 *year of age; rate per* 1,000 *related Live Births. Stillbirths per* 1,000 *births*).

Years	1952		1953		1954		1955†		1956‡	
Estimated Mid-year Home Popn. in thousands*	43,955		44,109		44,274		44,441		44,667	
	Number	Rate	Number	Rate	Number	Rate	Number	Rate	Number	Rate
Live births	673,286	*15·3*	681,964	*15·5*	672,635	*15·2*	664,954	*15·0*	699,059	*15·7*
Stillbirths	15,647	*22·7*	15,683	*22·5*	16,202	*23·5*	15,815	*23·2*	16,431	*23·0*
Deaths	497,484	*11·3*	503,529	*11·4*	501,896	*11·3*	518,864	*11·7*	521,402	*11·7*
Marriages	349,308	*8·0*	344,998	*7·8*	341,731	*7·7*	357,918	*8·1*	352,901	*7·9*
Infant Mortality	18,555	*27·6*	18,324	*26·8*	17,160	*52·4*	16,613	*24·9*	16,471	*23·8*
Net Reproduction Rate ..	*1·008*		*1·032*		*1·031*		Not yet available		Not yet available	
Quarters	Live Births in the Quarters of each Calendar Year									
Jan.–March	173,338	*15·9*	171,649	*15·8*	173,547	*15·9*	169,190	*15·4*	179,446	*16·2*
April–June	174,159	*15·9*	179,477	*16·3*	175,290	*15·9*	171,234	*15·5*	182,593	*16·4*
July–Sept.	168,030	*15·2*	172,943	*15·6*	166,938	*15·0*	166,890	*14·9*	173,586	*15·5*
Oct.–Dec.	157,759	*14·3*	157,895	*14·2*	156,860	*14·1*	157,640	*14·1*	163,434	*14·6*
	Stillbirths									
Jan.–March	4,174	*23·5*	3,975	*22·6*	4,236	*23·8*	4,032	*23·3*	4,291	*23·4*
April–June	4,004	*22·5*	4,220	*23·0*	4,171	*23·2*	4,023	*23·0*	4,229	*22·6*
July–Sept.	3,714	*21·6*	3,711	*21·0*	3,939	*23·1*	3,910	*23·0*	4,007	*22·6*
Oct.–Dec.	3,755	*23·2*	3,777	*23·4*	3,856	*24·0*	3,850	*23·8*	3,904	*23·3*
	Deaths (excluding Stillbirths)									
Jan.–March	146,948	*13·4*	171,350	*15·8*	153,120	*14·0*	168,645	*15·4*	170,293	*15·3*
April–June	115,369	*10·6*	114,644	*10·4*	117,205	*10·6*	124,230	*11·2*	120,063	*10·8*
July–Sept.	98,613	*8·9*	98,545	*8·9*	104,114	*9·3*	101,677	*9·1*	104,135	*9·3*
Oct.–Dec.	136,554	*12·4*	118,990	*10·7*	127,457	*11·4*	124,312	*11·1*	126,911	*11·3*
	Marriages									
Jan.–March	106,629	*9·8*	93,610	*8·6*	95,922	*8·8*	100,696	*9·2*	107,122	*9·7*
April–June	69,489	*6·4*	77,123	*7·0*	74,364	*6·8*	74,763	*6·8*	68,821	*6·2*
July–Sept.	103,411	*9·4*	105,614	*9·5*	104,505	*9·4*	106,691	*9·5*	106,923	*9·5*
Oct.–Dec.	69,779	*6·3*	68,651	*6·2*	66,940	*6·0*	75,768	*6·8*	70,035	*6·3*
	Infant Mortality									
Jan.–March	5,629	*33·6*	5,709	*34·0*	5,256	*31·0*	4,852	*29·2*	4,873	*28·4*
April–June	4,332	*25·1*	4,448	*25·2*	4,304	*24·7*	4,155	*24·4*	4,081	*23·0*
July–Sept.	3,863	*22·8*	3,862	*22·2*	3,543	*21·0*	3,605	*21·6*	3,538	*20·3*
Oct.–Dec.	4,731	*29·0*	4,305	*26·2*	4,057	*25·1*	4,001	*24·5*	3,979	*23·9*

* Home population comprises the resident civilian population plus any British, Commonwealth or Allied Armed Forces stationed in the country and the mid-year estimates are based upon final figures from the full tabulation of the 1951 Census.

† Birth, death and marriage rates are based on the relevant Home population.

‡ Provisional figures.

No. II.—SCOTLAND

BIRTHS, DEATHS *and* MARRIAGES *registered in the Calendar years* 1952–1956 *and in the Quarters of those years. Numbers, Annual and Quarterly Rates* per* 1,000 *persons living.* (*Deaths under* 1 *year of age, rate per* 1,000 *Live Births; Stillbirths per* 1,000 *Births*).

Years	1952		1953		1954		1955		1956†	
Estimated Mid-year Popn. in thousands*	5,114		5,118		5,123		5,133		5,145	
	Number	Rate	Number	Rate	Number	Rate	Number	Rate	Number	Rate
Live Births	90,422	17·7	90,913	17·8	92,315	18·0	92,539	18·0	95,313	18·5
Stillbirths	2,430	26	2,308	25	2,401	25	2,330	25	2,329	24
Deaths	61,510	12·0	58,878	11·5	61,380	12·0	61,645	12·0	61,792	12·0
Marriages	41,154	8·0	40,902	8·0	41,975	8·2	43,199	8·4	43,969	8·5
Infant Mortality	3,181	35	2,800	31	2,861	31	2,811	30	2,726	29
Quarters	Live Births in the Quarters of each Calendar Year									
Jan.–March	22,892	18·0	22,860	18·1	23,456	18·6	23,111	18·3	24,594	19·2
April–June	23,531	18·5	23,791	18·6	24,097	18·9	23,942	18·7	24,889	19·5
July–Sept.	22,161	17·2	22,166	17·2	22 823	17·7	22,734	17·6	22,654	17·5
Oct.–Dec.	21,838	17·0	22,096	17·1	21,939	17·0	22,752	17·6	23,176	17·9
	Stillbirths									
Jan.–March	677	29	602	26	612	25	598	25	617	24
April–June	598	25	586	24	628	25	566	23	613	24
July–Sept.	541	24	561	25	601	26	597	26	536	23
Oct.–Dec.	614	27	559	25	560	25	569	24	563	24
	Deaths (excluding Stillbirths)									
Jan.–March	18,394	14·5	17,264	13·7	18,003	14·3	19,078	15·1	19,156	15·0
April–June	14,270	11·2	14,345	11·2	14,638	11·5	14,998	11·7	14,562	11·4
July–Sept.	12,813	10·0	12,722	9·9	13,327	10·3	12,733	9·8	13,088	10·1
Oct.–Dec.	16,033	12·5	14,547	11·3	15,412	11·9	14,836	11·5	14,986	11·6
	Marriages									
Jan.–March	12,420	9·8	12,085	9·6	12,371	9·8	12,420	9·8	11,744	9·2
April–June	8,156	6·4	8,212	6·4	8,553	6·7	8,662	6·8	9,845	7·7
July–Sept	11,894	9·3	12,069	9·4	12,490	9·7	12,966	10·0	12,959	10·0
Oct.–Dec.	8,684	6·8	8,536	6·6	8,561	6·6	9,151	7·1	9,421	7·3
	Infant Mortality									
Jan.–March	872	38	776	34	892	38	829	36	763	31
April–June	801	34	696	29	728	30	692	29	656	26
July–Sept.	663	30	614	28	555	24	588	26	602	27
Oct.–Dec.	845	39	714	32	686	31	702	31	705	30

† Provisional figures.

* The population is the civilian population (including members of the Mercantile Marine for the time being in Scotland, and members of the Armed Forces stationed in Scotland). Birth, Death and Marriage Rates are calculated accordingly.

No. III.—NORTHERN IRELAND

BIRTHS, DEATHS *and* MARRIAGES *registered in the Calendar years* 1952–1956 *and in the Quarters of those years. Numbers, Annual and Quarterly Rates* per* 1,000 *persons living.* (*Deaths under* 1 *year of age, rate per* 1,000 *Live Births*).

Years	1952		1953		1954		1955		1956†	
Estimated Mid-year Home Popn. in thousands*	1,375		1,384		1,387		1,394		1,397	
	Number	Rate	Number	Rate	Number	Rate	Number	Rate	Number	Rate
Live births	28,760	*20·9*	28,984	*20·9*	28,803	*20·8*	28,965	*20·8*	29,489	*21·1*
Stillbirths[1]			Not registered							
Deaths	14,812	*10·8*	14,813	*10·7*	15,124	*10·9*	15,407	*11·1*	14,858	*10·6*
Marriages	9,300	*6·8*	9,416	*6·8*	9,154	*6·6*	9,510	*6·8*	9,371	*6·7*
Infant Mortality	1,117	*39*	1,090	*38*	951	*33*	937	*32*	850	*29*
Quarters	Live Births in the Quarters of each Calendar Year									
Jan.–March	7,095	*20·8*	7,212	*21·3*	7,253	*21·3*	6,876	*20·2*	7,216	*20·8*
April–June	7,589	*22·2*	7,902	*22·9*	7,704	*22·3*	7,679	*22·1*	7,912	*22·8*
July–Sept.	7,215	*20·9*	7,254	*20·8*	7,089	*20·3*	7,638	*21·7*	7,379	*21·0*
Oct.–Dec.	6,861	*19·9*	6,616	*19·0*	6,757	*19·3*	6,772	*19·3*	6,982	*19·9*
	Deaths (excluding Stillbirths)									
Jan.–March	4,450	*13·0*	4,542	*13·4*	4,311	*12·6*	5,083	*14·9*	4,880	*14·1*
April–June	3,671	*10·7*	3,733	*10·8*	3,985	*11·5*	3,867	*11·1*	3,485	*10·0*
July–Sept.	3,093	*9·0*	3,075	*8·8*	3,197	*9·1*	3,126	*8·9*	2,972	*8·5*
Oct.–Dec.	3,598	*10·4*	3,463	*9·9*	3,631	*10·4*	3,331	*9·5*	3,521	*10·0*
	Marriages									
Jan.–March	2,272	*6·7*	2,022	*6·0*	2,067	*6·1*	2,185	*6·4*	2,071	*6·0*
April–June	2,099	*6·1*	2,288	*6·6*	2,123	*6·1*	2,180	*6·3*	2,205	*6·4*
July–Sept.	2,988	*8·7*	3,235	*9·2*	3,183	*8·9*	3,297	*9·5*	3,421	*9·7*
Oct.–Dec.	1,941	*5·6*	1,871	*5·4*	1,781	*5·0*	1,848	*5·3*	1,674	*4·8*
	Infant Mortality									
Jan.–March	317	*45*	332	*46*	285	*39*	268	*39*	257	*36*
April–June	288	*38*	294	*37*	245	*32*	231	*30*	211	*27*
July–Sept.	232	*32*	225	*31*	202	*28*	216	*28*	178	*24*
Oct.–Dec.	280	*41*	239	*36*	219	*32*	222	*33*	204	*29*

* Home population comprises civilians and members of the armed forces stationed in N. Ireland. Birth, death and marriage rates are calculated accordingly.

† Provisional figures.

[1] Stillbirths are not registered in N. Ireland. The birth of one living child and one stillborn, or one living and two stillborn, is counted as one birth.

No. IV.—EIRE

BIRTHS, DEATHS *and* MARRIAGES *registered in the Calendar years* 1952–1956 *and in the Quarters of those years. Numbers, Annual and Quarterly Rates* per* 1,000 *persons living.* (*Deaths under* 1 *year of age, rate per* 1,000 *Live Births*).

Years	1952		1953		1954		1955†		1956†	
Estimated Mid-year Popn. in thousands	2,948		2,942		2,933		2,909		2,895	
	Number	Rate	Number	Rate	Number	Rate	Number	Rate	Number	Rate
Live births	64,631	21·9	62,558	21·2	62,534	21·3	61,575	21·2	60,738	21·0
Stillbirths‡			Not registered							
Deaths	35,105	11·9	34,591	11·7	35,535	12·1	36,525	12·6	33,898	11·7
Marriages	15,876	5·4	15,888	5·4	15,831	5·4	16,137	5·5	16,567	5·7
Infant Mortality	2,674	41	2,463	39	2,364	38	2,249	37	2,161	36
Quarters	Live Births in the Quarters of each Calendar Year									
Jan.–March	16,341	22·2	15,672	21·3	15,379	21·0	15,073	20·6	14,021	19·3
April–June	17,502	23·7	16,947	23·0	16,487	22·5	16,639	22·9	16,840	23·3
July–Sept.	16,048	21·8	15,728	21·4	15,974	21·8	15,674	21·6	15,219	21·0
Oct.–Dec.	14,740	20·0	14,211	19·3	14,694	20·0	14,189	19·5	14,658	20·3
	Deaths (excluding Stillbirths)									
Jan.–March	10,657	14·5	10,544	14·3	10,470	14·3	11,698	16·0	10,370	14·3
April–June	8,713	11·8	8,948	12·2	8,847	12·1	9,699	13·3	8,595	11·9
July–Sept.	7,256	9·8	7,104	9·6	7,673	10·5	7,403	10·2	7,056	9·7
Oct.–Dec.	8,479	11·5	7,995	10·9	8,545	11·7	7,725	10·6	7,877	10·9
	Marriages									
Jan.–March	3,373	4·6	2,935	4·0	3,332	4·5	3,086	4·2	2,813	3·9
April–June	3,754	5·1	3,816	5·2	3,642	5·0	3,866	5·3	4,130	5·7
July–Sept.	5,196	7·1	5,533	7·5	5,507	7·5	5,838	8·0	5,616	7·8
Oct.–Dec.	3,553	4·8	3,604	4·9	3,350	4·6	3,347	4·6	4,008	5·5
	Infant Mortality									
Jan.–March	886	54	703	45	749	49	673	45	632	45
April–June	617	35	686	43	594	36	595	36	557	33
July–Sept.	574	36	517	33	473	30	467	30	455	30
Oct.–Dec.	597	41	557	39	548	37	514	36	517	35

* Rates are based on the total estimated population as at the middle of the corresponding year.
† Provisional figures.
‡ Stillbirths are not registered in Eire. The births of one living and one stillborn, or of one living child and two stillborn are counted as one birth.

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